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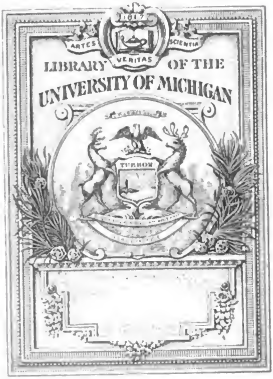

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Vol. I. No. 57.—January 5, 1861.

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The present Number commences what may almost be termed a new era in the existence of the Chemical News, and we owe it to our readers to explain the reasons why we have considered it necessary to increase its price, and to make a more than corresponding increase in its dimensions.

Most, if not all, of our learned and scientific Societies have a special organ of their own, published sometimes monthly, but generally less frequently. These publications, as a rule, give only the proceedings of the Society to which they respectively belong, and the papers that are read at its meetings. The papers are given in full, as they ought to be under such circumstances; and it is with no desire to detract from their value that we say that, for the general reader, they, as a consequence of this, remain unread. The working-men,—that is to say, the chemist, whose days are spent in the laboratory; the engineer, whose hours are employed in carrying out plans already complete, and in elaborating others; the electrician, who labours to simplify the transmission of intelligence, or to find a method of perfecting the means already in existence; and the host of scientific men, and those who take an interest in scientific subjects, who have daily avocations which exhaust their energies,—have not the courage to sit down and tax their intellect still further, by the study of an article which commonly requires a closer examination in proportion as its merits are greater. They would, however, be glad enough to read an abstract of such a paper, which embodied its chief points, the result of which would be that they themselves would receive new information and ideas, and the writers their reward, in the shape of a far more extended fame and reputation, which is what they principally seek.

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As heretofore, our columns will be open for the record of any new facts which may be observed in the conduct of operations in the laboratory; and we invite those who may observe such facts to forward them to us, in their own interest as well as in that of others engaged in the same professional pursuit.

In conclusion, we will state, as briefly as may be, what we aim at effecting by the alteration, and then leave it to our readers to decide in the future how far we accomplish what we propose.

Up to the present, there has not existed any Journal in Great Britain which professed to give a notice of all events of interest which occur in the scientific world. One Journal occasionally devotes a portion of one of its columns to the record of the most striking facts: but such desultory information does not meet the eye of the persons who are chiefly interested in such matters. We have, therefore, determined on making The Chemical News and Journal of Physical Science the permanent record of all matters interesting to the chemical philosopher, the man of science, and to educated men generally. We do not aim at superseding the recognised organs of the various learned Societies. The papers they contain, valuable as they are as records, are too full of detail, and often too abstruse to be published elsewhere at length; but such portions of them as are of principal importance we shall, as heretofore, embody in a summary,—an arrangement which we have found to be as gratifying to the authors as to our readers. In this way they will be kept thoroughly informed of all that transpires in any part of the world, whether relating to the discovery of a new chemical compound, or a novel application of something already known. That continually-increasing class of men, who, without being engaged in such subjects professionally, desire to be kept informed on their progress, will find in its pages all that he desires to know; and, that every facility may be given for reference to the past, a most copious Index will be published on the completion of each Volume.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Opacity of the Yellow Soda Flame to Light of its own Colour, by William Crookes.

In some remarkable investigations on the colours which certain substances impart to the flames in which they are heated,[1] Professors Kirchhoff and Bunsen describe certain experiments by which they prove that the luminous lines which are produced in the spectrum of a spirit- or gas-flame, when salts of the alkalies or alkaline earths are caused to volatilise therein, become *reversed* (*i.e.* that the bright lines become changed to dark ones) when a source of light of sufficient intensity, and giving a continuous spectrum, is placed behind the coloured flame. Amongst other experiments which were given to illustrate this point, was one in which, a solar spectrum showing the dark line D having been first obtained, a gas-flame, coloured yellow by the introduction of a bead of chloride of sodium, was interposed, when the dark line D (coincident with the yellow sodium line) appeared with *increased* blackness, instead of being partially, if not entirely, filled up by the luminous sodium line. In another experiment they showed that the dark double line D also appeared when a yellow soda flame was introduced in the apparatus in front of a white hot platinum wire; the previously continuous spectrum of the incandescent wire being at once cut across by a black line, occupying the position of D, when the soda flame was interposed (which *per se* would have given a luminous line, occupying the position of the now dark lines). The most conclusive experiment, however, was the following:—They held between the incandescent platinum wire and the optical part of the apparatus, a test-tube containing some sodium amalgam in a state of ebullition; where the luminous rays from the wire traversed that part of the tube containing sodium vapour, the dark line D was at once produced, disappearing as soon as the metallic vapour was removed from the path of the light. The authors conclude from this important experiment that sodium vapour, at a temperature much below that at which it becomes luminous, exerts its absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce. Professor Kirchhoff,[2] describing some experiments similar to the above, writes:—"I conclude from these observations, that coloured flames, in the spectra of which bright sharp lines present themselves, so weaken rays of the colour of these lines when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity, in the spectrum of which these lines are otherwise wanting. I conclude further, that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth, exist in consequence of the presence, in the incandescent atmosphere of the sun, of those substances which in the spectrum of a flame produce bright lines at the same place. . . In the course of the experiments which have at present been instituted by us in this direction, a fact has already shown itself which seems to us to be of great importance. The Drummond light requires, in order that the lines D should come out in it dark, a salt flame of lower temperature. The flame of alcohol, containing water, is fitted for this, but the flame of Bunsen's gas-lamp is not. With the latter the smallest mixture of common salt, as soon as it makes itself generally perceptible, causes the bright lines of sodium to show themselves."

During some researches on the spectra of artificial flames which I have been carrying on simultaneously with MM. Kirchhoff and Bunsen, one or two forms of experiment suggested themselves which, while they perfectly corroborate most of the facts mentioned above, seem to merit attention, from the facility with which they may be performed, and the striking manner in which the phenomena can be exhibited to several persons at once without the necessity of employing any optical apparatus. The atmosphere of the room—or, rather, that part of it in which the illustration is performed,—is to be first impregnated with soda-smoke, by igniting a piece of sodium, the size of a pea, on wet blotting-paper. Any flame, whether of gas, spirit, a candle, &c., which may now be burning anywhere in that part of the room, will exhibit in a marked manner the well-known yellow soda-flame; and if the full amount of gas in an ordinary wire-gauze air-burner is turned on and ignited, it will give a uniformly brilliant yellow flame, upwards of a foot high and three inches in diameter.

If a smaller flame be now moved in front of this large one it will exhibit a curious phenomenon. Those parts of it which are ordinarily seen to be luminous will suffer no change, other than that slight diminution in intensity which might be anticipated from their projection in front of a broad but not very brilliant source of light; but beyond these there will appear a sharply-cut and intensely black narrow border, closely surrounding the visible flame, and presenting the curious appearance of the latter being set in an opaque frame. A closer scrutiny will show that the position of this black rim is not, as I at first supposed, in that outer cone in which the yellow soda-flame is most distinctly seen, but that it lies in the dark space immediately outside the luminous part of the flame, affording proof of the existence of another invisible cone of vapour. The flame from a tallow candle shows this appearance better than that of wax or sperm, probably on account of its inferior luminosity. A small spirit- or gas-flame will also answer very well; but I think a tallow candle shows the phenomenon in a more striking manner.

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[1] "Chemical Analysis by Spectrum Observations," by Professors Kirchhoff and Bunsen. *Phil. Mag.*, s. 4, vol. xx. p. 89. August, 1860.

[2] "On the Simultaneous Emission and Absorption of Rays of the same Definite Refrangibility." *Phil. Mag.*, s. 4, vol. xix. p. 195. March, 1860.

The fact of the cone of yellow soda-flame being transparent, while the outer, non-luminous, space is perfectly opaque to the same kind of light placed behind it, appears worthy of attention. It seems to show that the yellow flame caused by the presence of incandescent solid particles of a sodium compound has no very marked absorptive action on light of its own colour; but that to give rise to this kind of opacity it is necessary that the sodium compound should be in the state of vapour. It appears, moreover, to prove that it is not necessary for this vapour to be in the metallic state; for it could hardly be supposed that so highly combustible a vapour as that of metallic sodium could be present in that part of the flame which is seen to possess this great opacity. That soda salts are easily volatile at the temperature of flame, is a fact abundantly proved by Bunsen.[1] The reason why the opacity is only exhibited by that part of the outer shell of vapour which is situated at the edge of the flame, and not by its entire extent, is owing to its thickness being insufficient to produce sensible absorption on rays which traverse it perpendicularly; an appreciable action taking place only when they pass as a tangent to the edge of the flame, and thus traverse a considerable extent of absorbing medium.

Cæsium.

AT the last meeting of the Chemical Society, Dr. Roscoe gave a short account of Professors Kirchhoff and Bunsen's spectrum researches, and mentioned that the new alkali-metal[2] which they had discovered by that means had been named *Cæsium*, from the Latin word *cæsius*, signifying *grayish-blue*, that being the tint of the two spectral lines which it shows. By working with the residues from twenty tons of the mineral waters of Kreuznach, Professor Bunsen had succeeded in obtaining about 250 grains of the platinum salt of the new metal. Cæsium is closely allied to potassium in its chemical characters, the chief point of difference being the solubility of its nitrate in alcohol. Its equivalent number is 117,—exactly three times that of potassium.

On the Determination of Phosphoric Acid in Complex Natural Substances and particularly in those containing Iron, by M. G. Chancel.

WHEN I published the process for the separation and estimation of phosphoric acid under the form of phosphate of bismuth,[3] I took care to insist on the necessity of previously eliminating the chlorine and sulphuric acid. It remains for me to warn chemists who wish to use this process against a source of error which iron will occasion when that metal is in the state of sesquioxide. In this case the phosphate of bismuth is precipitated with difficulty, and always carries down iron with it, which gives a red colour to the precipitate after calcination. But it is only necessary to reduce the iron to the minimum of oxidation to restore to the process all its generality, and to allow of its successful application to the analyses of ashes, arable land, coprolites, mineral waters, iron ores, &c. A very simple plan, which has always succeeded with me, consists in effecting this reduction with sulphuretted hydrogen.

The better to answer the various inquiries made of me, I think it necessary to give a summary account of the analytical process which ought to be followed in determining phosphoric acid in the most complicated cases.

1. Heat the weighed substance with excess of warm concentrated nitric acid, to transform, if expedient, the meta- or pyro-phosphoric acids into tribasic phosphoric acid; dissolve in a proper quantity of nitric acid, add water, and filter if necessary.

2. Eliminate from the diluted solution first the sulphuric acid by nitrate of baryta; then the chlorine, by nitrate of silver; separate each of these precipitates by filtration.

3. Bring the iron to the minimum of oxidation by passing to saturation a current of sulphuretted hydrogen through the filtered liquid. By this means the silver added in excess is precipitated, as well as the other metals whose sulphides are insoluble in weak acids. Numerous experiments have proved that the reduction of iron under these circumstances is always complete, even when the liquid contains a considerable quantity of free nitric acid. The operation is finished when the precipitate has collected together and the liquid is perfectly limpid. Before filtration all the sulphuretted hydrogen must be expelled. This result is speedily obtained by passing through it a current of carbonic acid, until the gas which is disengaged ceases to discolour paper saturated with acetate of lead.

4. Under these circumstances, the estimation of the phosphoric acid presents no difficulty. It is only necessary to add excess of acid nitrate of bismuth to the filtered liquid; allow the precipitate to deposit, then collect it on a filter, and, after well washing it with boiling water, dry, calcine, and weigh it. The nitrate of the protoxide of iron being remarkably stable, there is no cause to fear the peroxidation of the iron before the addition of acid nitrate of bismuth, provided the operation is conducted at the ordinary temperature. Moreover, I am convinced that the liquid may be boiled, without danger, after the phosphate of bismuth is collected; the sesquioxide of iron which then forms being no impediment to the determination of the phosphoric acid.

5. Treat the filtered liquid again with sulphuretted hydrogen, which eliminates from it the excess of bismuth, then determine the bases by the ordinary processes.

It should be remarked that the various foregoing operations are all exceedingly simple and speedily performed, and that this method serves not only for the quantitative determination of phosphoric acid, but also to detect its existence in substances which contain only traces of it, as mineral waters, certain iron ores, &c. In these qualitative trials, when the acid nitrate of bismuth yields a precipitate, it is easy to identify the phosphoric acid in it. It is sufficient for this purpose to treat the precipitate, suspended in a little water, with sulphuretted hydrogen, then to add excess of nitrate of silver, and shake it. The liquid cleared by the filter of the sulphides of bismuth and silver, is then free from sulphuretted hydrogen; if it contains phosphoric acid it will give the yellow precipitate so characteristic of phosphate of silver when exactly neutralised with very diluted ammonia.

In conclusion, I will point out a good method for the preparation of acid nitrate of bismuth; it consists of substituting for the subnitrate, crystallised neutral nitrate, $BiO_3 3NO_5 + 10Aq$, which it is easy to obtain perfectly pure. A convenient re-agent is obtained by dissolving

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[1] *Phil. Mag.*, s. 4, vol. xviii. p. 513.

[2] CHEMICAL NEWS, vol. ii. p. 281.

[3] *Comptes-Rendus*, t. l. p. 416; and CHEMICAL NEWS, vol. i. p. 230.

68·45 grammes of neutral crystallised nitrate in a quantity of nitric acid representing 68·5 grammes of anhydrous nitric acid, and in then adding water, so that the solution occupies exactly the volume of a litre. Each cubic centimètre of the re-agent thus prepared will precipitate 1 centigramme of phosphoric acid.—*Comptes-Rendus*, t. li. p. 882.

On the Insoluble Matter of Zinc, by George F. Rodwell.

In making hydrogen by the action of dilute sulphuric acid on commercial zinc, we observe a number of black flocculent particles floating on the surface of the liquid, which, when the zinc is all dissolved, gradually sink to the bottom, and crumble down to a grayish powder. This residue, in 100 parts of ordinary sheet zinc, amounted to

I.	II.	III.	IV.
1·3142	1·3661	1·3388	1·3017

or, taking the mean of the four determinations, 1·3339. It was found to consist of sulphate of lead, together with about ·5 per cent. of carbon, and a trace of iron. The black particles appear to be suboxide of lead, which, when the evolution of hydrogen has ceased (and not till then) are slowly converted into sulphate of lead.

The lead undoubtedly exists in the zinc as metallic lead, and its quick conversion into suboxide is probably due to the electric current which is established between it and the zinc with which it is in contact; for if a clean piece of lead be immersed in dilute sulphuric acid it will remain bright for some time; but if, now, a piece of zinc be placed in the liquid, so as to touch it, the lead will be speedily coated with a black film.

TECHNICAL CHEMISTRY.

On a Simple Means for Concentrating Sulphuric Acid without employing Platinum Retorts, by W. T. Clough.[1]

It is a well-known fact that lead is with difficulty attacked by cold concentrated sulphuric acid, whilst the same acid when hot, very rapidly transforms it into sulphate of lead.

M. Clough has endeavoured to take advantage of this circumstance to concentrate the crude sulphuric acid from the chambers in a leaden vessel kept cold. The arrangement of his apparatus is as follows:—

Place a large leaden holder in an iron one of the same shape but larger, and fill the interspace with water, which should always be kept cold. Line the interior of the leaden holder completely with some kind of brick or stone not affected by sulphuric acid, and surmount it with two brick arches, that the holder may be, as it were, the sole of a reverberatory furnace. Having filled the holder with sulphuric acid, of a density of 4° Baumé, pass over the liquid the flame of a fire placed a little distance from one of the extremities of the holder. Rapid evaporation is thus obtained without the sides or bottom of the lead holder becoming heated. In fact the warm acid becomes almost completely cold by passing across the bricks (non-conductors of heat), before reaching the lead which is itself maintained cold by the water in the iron holder. Between the two brick arches, so as to completely close and complete the arch, insert a boiler made of lead, in which the weak acid from the lead chambers is heated and partly concentrated before being made to flow into the lower holder.

[Is not a considerable loss of sulphuric acid to be feared, which when near its maximum of concentration and heated to 300°, forms vapour abundantly? These vapours are easily affected by the flame, and the current of warm air passing under the acid. When the sulphuric acid is concentrated in platinum retorts, the acid vapours given off in considerable quantities are collected by condensation, especially when near the boiling point and the maximum of concentration of the sulphuric acid.

M. Kuhlmann has proposed another method for concentrating sulphuric acid in lead. This method is founded on evaporation *in vacuo*.—*E. Kopp.*]

PHARMACY, TOXICOLOGY, &c.

On the Uncertainty of Composition of the Aqua Lauro Cerasi,[2] *by* Harry Napier Draper, *F.C.S.L.*

My object in this paper is to draw attention to the very uncertain constitution of cherry laurel water, and to the importance of providing some substitute in the shape of the much more stable hydrocyanic acid. Laurel water has on its side all the claim of antiquity, and I am aware that it is not easy to persuade the practitioner who has long been in the habit of prescribing any one remedy to use another in its stead. I am also fully alive to the importance of caution in presuming upon the value of any therapeutic agent, solely from its chemical composition, seeing that we possess many very valuable remedies, about the constitution of which we either know nothing, or nothing which will serve to elucidate their mode of action. If, therefore, experience pointed out any difference in the effects of laurel water and of dilute hydrocyanic acid, a proposition to substitute the latter for the former would be unjustifiable; but as it is the opinion of all who have written upon the subject that the action of the two remedies is in every respect the same,[3] if we become convinced that we gain an important advantage by the change we should certainly adopt it.

The aqua lauro cerasi is not included among the formulæ of the London Pharmacopœia, but finds a place among those of the Pharmacopœias of Dublin and Edinburgh. The process for its preparation is in both of these essentially the same. One pound of fresh laurel leaves are digested in two and a-half pints of water, and a pint of the liquid is distilled over and filtered. Laurel water, as thus prepared, is nothing more than a dilute solution of hydrocyanic acid, to which the presence of a small quantity of volatile oil gives its peculiar odour and taste. The dose is by no means well fixed, being given by some authorities at from 10 minims to 20 minims, and according to others ranging from ʒss. to ʒj.

The leaves of the cherry laurel have never been perfectly examined, but of the nature of two of their constituents there exists no doubt. These are,—firstly, a volatile oil, resembling in its physical properties very closely that obtained from bitter almonds, and in chemical constitution agreeing with it in every particular;

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[1] American Pat. Off. Report, vol. i. p. 495, and vol. iii. p. 166.

[2] *Dublin Medical Press.*

[3] According to Göppert, however, the poisonous action of cherry laurel water does not depend on the hydrocyanic acid which it contains, but upon some property peculiar to itself.

secondly, hydrocyanic acid. Both of these substances are, as has been already stated, held in solution by the distillate from the leaves.

The chief cause of the uncertainty of this preparation lies in the fact that the quantity of hydrocyanated oil obtained from the leaves varies with their age. Thus, Christison ("Dispensatory," p. 592) found that 1000 grains of the buds and unexpanded leaves of May and June gave 6·33 grains of oil, but when they had, in July, attained their full size, the same quantity yielded but 3·1 grains, while in the May following the quantity had diminished to 0·6 grains. Not only is the quantity of oil liable to variation, but, what is still more important, the quantity of hydrocyanic acid which it contains is uncertain. It is stated by some writers to contain 2·75 per cent., while according to others who have investigated the subject, it may exist in as large a proportion as 7·66 per cent. Christison confirms this discrepancy, he having obtained from developed young leaves twice as much acid as from old ones. Then, again, while it is well known that the leaves are constantly employed with impunity as a flavouring for articles of food, many cases have occurred in which serious results have supervened from their use in this manner. In a case cited by Dr. Paris ("Med. Jur." vol. ii. p. 400), several children were nearly poisoned by eating a custard so flavoured. Zeller (Quoted by Taylor, "Poisons," p. 717) states that the leaves gathered in wet, cold weather yield more hydrocyanic acid than when they are collected in hot, dry seasons.

From this collection of facts it will be seen that it is impossible for any two samples of laurel water to contain the same proportion of active constituent; but this is by no means the only source of error. Others, which are no less conducive to uncertain composition, creep in during the process of manufacture. As the whole of the oil comes over in the distillation with the first few ounces of water, if the whole product be not strongly agitated before filtration, a uniform preparation will not, even if other conditions be favourable, be obtained. Moreover, constant variations from the process given by the Pharmacopœias are frequently adopted to suit the conveniences of manufacture on the large scale. The most usual of these modifications consists in passing steam through a vessel containing the crushed leaves until the condensed vapour measures the proper quantity. More rarely it is prepared by adding a known proportion of the oil with distilled water, as is practised in the preparation of the aromatic waters of the Pharmacopœia.

In addition to the above sources of inaccuracy, there is one other which has by no means attracted the attention which the importance of the question seems to me to deserve. It is this:—Liebig and Wöhler (Pereira's "Materia Medica," vol. ii. part ii. p. 1782), though unable in their analysis of laurel leaves to detect *amygdalin*, concur in opinion as to its existence. If this be the case,—and all analogy leads to the inference that it is,—the well-known action by which this body gives rise to hydrocyanic acid should surely not be overlooked, as the duration of the digestion of the leaves in water, —now considered as a matter of but secondary importance,—would then exercise considerable influence over the amount of active matter contained in the product. Closely connected with this part of the question are the results obtained by Pereira ("Materia Medica," vol. ii. part ii. p. 1776) in experimenting on bitter almond oil. He found that a quantity of this substance, which had been carefully purified, and in which he could not detect a trace of hydrocyanic acid, gave, after having been kept for a few months, distinct evidences of its presence. If we assume—as we can scarcely avoid doing—that an analogy exists between this oil and oil of cherry laurel, another condition of change is introduced, and an increase of hydrocyanic acid in the water must result from its being kept. Experiments in this direction are, however, still wanting.

My next step is naturally to inquire in what degree this inconstancy in the composition of laurel water, which we assume upon theory, is borne out by experience of its effects, and we find that here facts bear us out more fully even than before. A short summary of these facts will, while occupying less space than a more detailed account, at the same time illustrate the case more clearly.

Fonquier (Richard: "Elém. d'Hist. Nat.," t. ii. p. 447) has given *twelve ounces* in the course of a day without any evident effect.

In 1781 Sir T. Boughton was poisoned, death resulting in half-an-hour, by *two ounces* (Report of the Trial of Donellan).

In another case, quoted by Taylor (*Op. cit.*, p. 719), *one ounce and a-half* caused death in an adult.

Then, as regards chemical analysis, Pereira (quoted by Taylor) gives the average strength as equal to a solution of hydrocyanic acid containing 0·25 per cent. Two samples which I have examined, and which were obtained from two of our first pharmaceutical laboratories, yielded respectively 0·011 and 0·009 per cent.; in fact, mere traces, the numerical value of which could not, but that large quantities were operated upon, have been stated at all.

Surely further evidence is not needed to show that this preparation is most inconstant, and that even if, on the one hand, no bad results often ensue from its administration, on the other, the practitioner must frequently experience much disappointment from the inactivity of a medicine which he believes to contain 12 per cent. of pharmacopœial hydrocyanic acid.

As it is not likely that in this country, where, from long usage, laurel water has become firmly established, the much more stable and reliable hydrocyanic acid will ever supersede it, I would suggest two methods by which the evil may be, if not wholly remedied, at least palliated, and the preparation at the same time be retained.

The first of these is, that, as laurel leaves can be obtained at all times of the year, the preparation should be much more frequently made, and, when made, be standardised in the manner directed by the London Pharmacopœia for fixing the strength of its dilute hydrocyanic acid. In this way a uniform product—as far, at least, as the acid is concerned—would be obtained.

By the second method the pharmaceutist would be rendered independent of circumstances which might prevent fresh, and especially young leaves being obtained, and, moreover, the proportion of volatile oil in the liquid, which by the first method could not be constant, would be always the same. I propose, then,—firstly, that the volatile oil be distilled from the leaves, and, by any of the well-known means, wholly deprived of hydrocyanic acid, the subsequent formation of which might be guarded against by keeping some peroxide of mercury in the bottle which contains it; secondly, that a fixed proportion of this oil be dissolved in distilled water, and that hydrocyanic acid of known strength be added to the solution in such quantity as to make it bear an easily-remembered relation of strength to the dilute acid of the Pharmacopœia.

It is clear that the time for such a change will be upon the advent of the New Pharmacopœia; but, by way of example, supposing that a dilute acid of 2 per cent. be decided upon, the following would be an advantageous formula for the preparation of *aqua lauro cerasi*:—

Dilute hydrocyanic acid	. .	35 grs.
Volatile oil of cherry laurel	.	5 „
Distilled water	. . . . .	4 oz.

This solution would contain 2 per cent. of hydrocyanic acid (which in its turn would contain 2 per cent. of real acid), or, in other words, 50 minims would be equivalent to 1 minim of dilute hydrocyanic acid. From the facility with which this water could be made, no valid excuse could be found for not throwing it away when from any cause it had become deteriorated.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. Faraday, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

Lecture I. (Dec. 27, 1860.)—*A Candle: The Flame—its Sources—Structure—Mobility—Brightness.*

I purpose thanking you for the honour you do us in coming to see what are our proceedings here, by bringing before you the Chemical History of a Candle. I have done so on a former occasion, and if I had my own will I should do it almost every year; so abundant is the interest that attaches itself to the subject, so wonderful are the varieties of outlet which it gives into the various departments of philosophy. There is not a law under which any part of this universe is governed which does not come into play and is touched upon in these phenomena. There is no better, there is no more open door by which you can enter into the study of natural physical philosophy, than by considering the phenomena of a candle. Therefore I believe I shall not disappoint you in choosing this for my subject rather than any newer form, which could not be better, if it were so good.

And having said so much to you, let me say this also: that though our subject be so great, and our intention that of treating it honestly, philosophically, and seriously, yet I mean to pass away from all those here who are seniors. I claim the right of speaking to juveniles as a juvenile myself. I have done it on former occasions, and, if you please, I shall do it again. And though I know that I stand here with the knowledge of having the words I utter given to the world, yet that shall not deter me from speaking in the same familiar way to those whom I esteem nearest to me on this occasion. You know that though we make no publication of our proceedings—neither I nor the authorities—we give all facilities to those who honour us by supposing that what they hear here is worth conveying further—we give them every facility to hear us and write about us, but it is entirely their own act. You have here the original, in whatever shape it appears anywhere else.

And now to my boys and girls.

I must first tell you what candles are made of. Some are very curious things. I have here some bits of timber, branches of trees particularly famous for their burning. And here you see a piece of that very curious substance taken out of some of the bogs in Ireland, called *candle-wood*, a hard, strong, excellent wood, evidently fitted for good work as a resister of force, and yet withal burning so well that when it is found they make splinters of it, and torches, since it burns like a candle, and gives a very good light indeed. And here in this wood is one of the most beautiful illustrations of the general nature of a candle that I can possibly give. The fuel provided, the means of bringing that fuel to the place of chemical action, the regular and gradual supply of air to that place of action—heat and light—all produced by a little piece of wood of this kind, forming, in fact, a natural candle.

But we must speak of candles as they are in commerce. Here are a couple of candles commonly called dips. They are made of lengths of cotton cut off, hung up by a loop, dipped into melted tallow, taken out again and cooled, then re-dipped, until there is an accumulation of tallow round the cotton. In order that you may have an idea of the various characters of these candles, you see these which I hold in my hand—they are very small and very curious. They are, or were, the candles used by the miners in coal mines. In olden times the miner had to find his own candles, and it was supposed that a small candle would not so soon set fire to the fire-damp in the coal mines as a large one; and for that reason, as well as for economy's sake, he had candles made of this sort, 20, 30, 40, or 60, to the pound. They have been replaced since then by the steel-mill, and then by the Davy-lamp, and other safety-lamps of various kinds. I have here a candle that was taken out of the *Royal George*, it is said, by Sir George Pashley. It has been sunk in the sea for many years, subject to the action of salt water. It shows you how well candles may be preserved, for though it is cracked about and broken a good deal, yet when lighted it goes on burning regularly, and the tallow resumes its natural condition as soon as it is fused.

Mr. Field, of Lambeth, has supplied me abundantly with beautiful illustrations of the candle and its materials; I shall therefore now refer to them. And, first, there is the suet—the fat of the ox—Russian tallow, I believe, employed in the manufacture of these dips, which Gay Lussac, or some one who entrusted him with his knowledge, converted into that beautiful substance, stearine, which you see lying beside it. A candle, you know, is not now a greasy thing like an ordinary tallow candle, but a clean thing, and you may almost scrape off and pulverise the drops which fall from it without soiling anything. This is the process he adopted:—The fat or tallow is first boiled with quick lime, and made into a soap, and then the soap is decomposed by sulphuric acid, which takes away the lime, and leaves the fat re-arranged as stearic acid, whilst a quantity of glycerine is produced at the same time. Glycerine—absolutely a sugar, or a substance similar to sugar—comes out of the tallow in this chemical change. The oil is then pressed out of it; and you see here this series of pressed cakes, showing how beautifully the impurities are carried out by the oily part as the pressure goes on increasing, and at last you have left that substance which is melted, and cast into candles as you here see them. The candle I have in my hand is a stearine candle, made of stearine from tallow in the way I have told you. Then here is a sperm candle, which comes from the purified oil of the spermaceti whale. Here also is yellow beeswax and refined beeswax, from which candles are made. Here, too, is that curious substance called paraffin, and some paraffin candles, made of paraffin obtained from the bogs of Ireland. I have here also a substance brought from Japan since we have forced an entrance into that out-of-the-way place—a kind of wax which a kind friend has sent me, and which forms a new material for the manufacture of candles.

And how are these candles made? I have told you about dips, and I will show you how moulds are made. Let us imagine any of these candles to be made of materials which can be cast. "Cast!" you say, "Why a candle is a thing that melts, and surely if you can melt it you can cast it." Not so. It is wonderful, in the progress of manufacture, and in the consideration of the means best fitted to produce the required result, how things turn up which one would not expect beforehand. Candles cannot always be cast. A wax candle can never be cast. It is made by a particular process which I can illustrate in a minute or two, but I must not spend much time on it. Wax is a thing which, burning so well, and melting so easily in a candle cannot be cast. However, let us take a material that can be cast. Here is a frame, with a number of moulds fastened in it. The first thing to be done is to put a wick through them. Here is one—a plaited wick, which does not require snuffing—supported by a little wire. It goes to the bottom, where it is pegged in—the little peg holding the cotton tight and stopping the aperture, so that nothing fluid shall run out. At the upper part there is a little bar placed across, which stretches the cotton and holds it in the mould. The tallow is then melted, and the moulds are filled. After a certain time, when the moulds are cool, the excess of tallow is poured off at one corner, and then cleaned off altogether, and the ends of the wick cut away. The candles alone then remain in the mould, and you have only to upset them, as I am doing, when out they tumble, for the candles are made in the form of cones, being narrower at the top than at the bottom, so that what with their form and their own shrinking, they only need a little shaking and out they fall. In the same way are made these candles of stearine and of paraffin. It is a curious thing to see how wax candles are made. A lot of cottons are hung upon frames, as you see here, and covered with metal tags at the ends to keep the wax from covering the cotton in those places. These are carried to a heater where the wax is melted. As you see, the frames can turn round, and as they turn a man takes a vessel of wax and pours it first down one, and then the next, and the next, and so on. When he has gone once round, if it is sufficiently cool, he gives the first a second coat, and so on until they are all of the required thickness. When they have been thus clothed, or fed, or made up to that thickness, they are taken off and placed elsewhere. I have here, by the kindness of Mr. Field, several specimens of these candles. Here is one only half finished. They are then taken down and well rolled upon a fine stone slab, and the conical top is moulded by properly shaped tubes, and the bottoms cut off and trimmed. This is done so beautifully that they can make candles in this way weighing exactly four, or six, to the pound, or any number you please.

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[1] Reported *verbatim* by special permission.

We must not, however, take up more time about the mere manufacture, but go a little further into the matter. I have not yet referred you to luxuries in candles (for there is such a thing as luxury in candles). See how beautifully these are coloured; you see here mauve, Magenta, and all the chemical colours recently introduced, applied to candles. You observe also, different forms employed. Here is a fluted pillar most beautifully shaped; and I have also here some candles sent me by Mr. Pearsall, which are ornamented with designs upon them, so that as they burn, you have as it were a glowing sun above and a bouquet of flowers beneath. All, however, that is fine and beautiful, is not useful. These fluted candles, pretty as they are, are bad candles, they are bad because of their external shape. Nevertheless, I show you these specimens sent to me from kind friends on all sides that you may see what is done and what may be done in this or that direction, though, as I have said, when we come to these refinements, we are obliged to sacrifice a little in utility.

Now as to the light of the candle. We will light one or two, and set them at work in the performance of their proper functions. You observe a candle is a very different thing from a lamp. With a lamp you take a little oil, fill your vessel, put in a little moss or some cotton prepared by artificial means, and then light the top of the wick. When the flame runs down the cotton to the oil, it gets extinguished, but it goes on burning in the part above. Now, I have no doubt, you may ask, how is it that the oil which will not burn of itself gets up to the top of the cotton where it will burn? We shall presently examine that; but there is a much more wonderful thing about the burning of a candle than this. You have here a solid substance with no vessel to contain it; and how is it that this solid substance can get up to the place where the flame is? How is it that this solid gets there, it not being a fluid? or, when it is made a fluid, then how is it that it keeps together? This is a wonderful thing about a candle.

We have here a good deal of wind, which will help us in some of our illustrations, but teaze us in others; for the sake, therefore, of a little regularity, and to simplify the matter, I shall make a quiet flame, for who can study a subject when there are difficulties in the way not belonging to it? Here is a clever invention of some costermonger or street stander in the market-place for the shading of their candles on Saturday nights, when they are selling their greens, or potatoes, or fish. I have very often admired it. They put a lamp-glass round the candle, supported on a kind of gallery, which clasps it, and it can be slipped up and down as required. By the use of this lamp-glass, employed in the same way, you have a steady flame, which you can look at, and carefully examine, as I hope you will do, at home.

You see then, in the first instance, that a beautiful cup is formed. As the air comes to the candle it moves upwards by the force of the current which the heat of the candle produces, and it so cools all the sides of the wax, tallow, or fuel, as to keep the edge much cooler than the part within; the part within melts by the flame that runs down the wick as far as it can go before it is extinguished, but the part on the outside does not melt. If I made a current in one direction, my cup would be lop-sided, and the fluid would consequently run over,—for the same force of gravity which holds worlds together holds this fluid in a horizontal position, and if the cup be not horizontal, of course the fluid will run away in guttering. You see, therefore, that the cup is formed by this fine, uniform, regular ascending current of air upon all sides which keeps the exterior of the candle cool. No fuel would do for a candle which has not the property of giving this cup, except such fuel as the Irish bogwood, where the thing is like a sponge and holds its own fuel. You see now, why you would have had such a bad result if you were to burn these beautiful candles that I have shown you, which are irregular, intermittent in their shape, and cannot, therefore, have that nicely-formed edge to the cup which is the great beauty in a candle. I hope you will now see that the perfection of a process, that is, its utility, is the better point of beauty about it. It is not the best-looking thing, but the best-acting thing which is the most advantageous to us. This good-looking candle is a bad burning one. There will be a guttering round about it because of the irregularity of the stream of air and the badness of the cup which is formed thereby. You may see some pretty cases (and I expect you to think of these things) of the action of the ascending current when you have a little gutter run down the side of a candle, making it thicker there than it is elsewhere. As the candle goes on burning, that keeps its place and forms a little pillar sticking up by the side, because, as it rises higher above the rest of the wax or fuel the air gets better round it, it is more cooled and better resists the action of the heat at a little distance. Now, the greatest mistakes and faults with regard to candles, as with regard to other points, often bring with them instruction which we should not receive if they had not occurred. We come here to be philosophers, and I hope you will always remember that whenever a result happens, especially if it is new, you should say, "What is the cause? Why does that occur?" and you will in the course of time find it out.

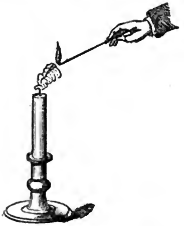
Then there is another point about these candles which will answer a question,—that is, as to the way in which this fluid gets out of the cup, up the wick, and into the place of combustion. You know that the flames on these wicks burning in candles made of beeswax, or stearine, or spermaceti, do not run down to the wax or other matter, and melt it all away, but keep to their own right place. They are fenced off from the fluid below, and do not encroach on the cup at the sides. I cannot imagine a more beautiful and more compact thing than the condition of adjustment under which a candle makes one part subserve to the other to the very end of its action. A combustible thing like that, burning away gradually, never being intruded upon by the flame,—is a very beautiful sight; especially when you come to learn what a vigorous thing flame is—what power it has of destroying the wax itself when it gets hold of it, and destroying its proper form even before it gets hold of it, if it come too near.

Now, how does it get hold of it? There is a beautiful point about that—*capillary attraction.* "Capillary attraction!" you say,—"the attraction of hairs." Well, never mind the name; it was given in old times before we had a good understanding of what the real power was. It is by what is called capillary attraction that the fuel is conveyed to the part where combustion goes on, and is deposited there, not in a careless way, but very beautifully in the very midst of the centre of action, which takes place around it. Now I am going to give you one or two instances of capillary attraction. It is that kind of action or attraction which makes two things that do not dissolve in each other still hold together. When you wash your hands you wet them thoroughly; you take a little soap to make the adhesion better, and you find your hand remains wet. This is by that kind of attraction of which I am about to speak. And what is more; if your hands are not soiled (as they almost always are by the usages of life), if you put your finger into a little warm water, the water will creep a little way up the finger, though you may not stop to examine it. I have here a substance which is rather porous—a column of salt,—and I will pour into the plate at the bottom, not water as it appears, but a saturated solution of salt which cannot absorb more; so that the action which you see, will not be due to its dissolving anything. We may consider the plate to be the candle, and the salt the wick, and this solution the melted tallow. (I have coloured the fluid that you may see the action better.) You observe that now I pour in the fluid, it rises and gradually creeps up the salt higher and higher; and provided the column does not tumble over, it will go to the top. If this blue solution were combustible, and we were to place a wick at the top of the salt, it would burn as it entered into the wick. It is a most curious thing to see this kind of action taking place, and to observe how singular some of the circumstances are about it. When you wash your hands you take a towel to wipe off the water, and it is by that kind of wetting, or that kind of attraction which makes the towel become wet with water, that the wick is made wet with the tallow. I have known some careless boys and girls (indeed, I have known it happen to careful people as well) who, having washed their hands and wiped them with a towel, have thrown the towel over the side of the basin, and before long it has drawn all the water out of the basin and conveyed it to the floor, because it happened to be thrown over the side in such a way as to serve the purpose of a syphon. That you may the better see the way in which the substances act one upon another, I have here a vessel made of wire gauze filled with water, and you may compare it in its action to the cotton in one respect, or to a piece of calico in the other. In fact, wicks are sometimes made of a kind of wire gauze. You will observe that this vessel is a porous thing, for if I pour a little water on to the top, it will run out at the bottom. You would be puzzled for a good while if I asked you what the state of this vessel is, what is inside it, and why it is there? The vessel is full of water, and yet you see the water goes in and runs out as if it were empty. In order to prove this to you I have only to empty it. The reason is this—the wire being once wetted, remains wet; the meshes are so small that the fluid is attracted so strongly from the one side to the other, as to remain in the vessel although it is porous. In like manner the particles of melted tallow ascend the cotton and get to the top; other particles then follow by their mutual attraction for each other, and as they reach the flame they are gradually burned.

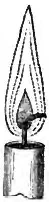
Here is another application of the same principle. You see this bit of cane. I have seen boys about the streets, who are very anxious to appear like men, take a piece of cane and light it and smoke it, as an imitation of a cigar. They are enabled to do so by the permeability of the cane in one direction, and by its capillarity. If I place this piece of cane on a plate containing some camphine (which is very much like paraffin in its general character), exactly in the same manner as the blue fluid rose through the salt will this fluid rise through the piece of cane. There being no pores at the side, the fluid cannot go in that direction, but must pass through its length. Already the fluid is at the top of the cane, and now I can light it and form it into a candle. The fluid has risen by the capillary attraction of the piece of cane, just as it does through the cotton in the candle.

Now, the only reason why the candle does not burn all down the side of the wick is that the melted tallow extinguishes the flame. You know that a candle, if turned upside down, so as to allow the fuel to run upon the wick, will be put out. The reason is, that the flame has not had time to make the fuel hot enough to burn, as it does above where it is carried in small quantities into the wick, and has all the effect of the heat exercised upon it.

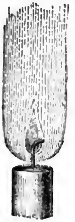
There is another condition which you must learn as regards the candle, without which you would not be able fully to understand the philosophy of it, and that is the vapourous condition of the fuel. In order that you may understand that, let me show you a very pretty, but very common-place experiment. If you blow a candle out cleverly, you will see the vapour rise from it. You have, I know, often smelt the vapour of a blown-out candle, and a very bad smell it is; but if you blow it out cleverly, you will be able to see pretty well the vapour into which this solid matter is transformed. I will blow out one of these candles in such a way as not to disturb the air round about it by the continuing action of my breath; and now, if I hold a lighted taper two or three inches from the wick, you will observe a train of fire going through the air till it reaches the candle. I am obliged to be quick and ready, because if I allow the vapour time to cool, it becomes condensed into a liquid or solid, or the stream of combustible matter gets disturbed.



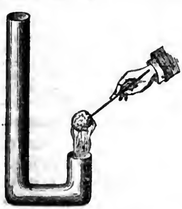
Now, as to the shape or form of the flame. It concerns us much to know about the condition which the matter of the candle finally assumes at the top of the wick, where you have such beauty and brightness as nothing but combustion or flame can produce. You have the glittering beauty of gold and silver, and the still higher lustre of jewels like the diamond and ruby; but nothing of these comes by comparison near to the brilliancy and beauty of flame. What diamond can shine like flame? It owes its lustre at night time to the very flame shining upon it. The flame shines in darkness, but the light which the diamond has is as nothing, until the flame shine upon it, when it is brilliant again. The candle alone shines by itself and for itself, or for those who have arranged the materials. Now, let us look a little at the form of the flame as you see it under the glass shade. It is steady and equal, and its general form is that which is represented in the diagram, varying with atmospheric disturbances, and also varying according to the size of the candle. It is a bright oblong, brighter at the top than towards the bottom, with the wick in the middle, and besides the wick in the middle certain darker parts towards the bottom where the ignition is not so good as in the part above. I have a drawing here, made many years ago by Hooker, when he made his investigations. It is the drawing of the flame of a lamp, but it will apply to the flame of a candle. The cup of the candle is the vessel or lamp; the melted spermaceti is the oil; and the wick is common to both. Upon that he sets this little flame, and then he represents what is true, a certain quantity of matter rising about it which you do not see, and which, if you have not been here before, or are not familiar with the subject, you will not know of. He has here represented the parts of the surrounding atmosphere that are very essential to the flame nd that are



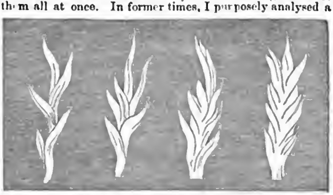
always present with it. There is a current formed, which draws the flame out, for the flame which you see is really drawn out by the current, and drawn upward to a great height, just as Hooker has here shown you by that prolongation of the current in the diagram. You may see this by taking a lighted candle, and putting it in the sun so as to get its shadow thrown on a piece of paper. What a remarkable thing it is that that thing which is light enough to produce shadows of other objects, can be made to throw its own shadow on a piece of white paper or card, so that you can actually see streaming round the flame something which is not part of the flame, but is ascending and drawing the flame upwards. Now I am going to imitate the sun-light, by applying the voltaic battery to the electric lamp. You now see our sun, and its great luminosity; and by placing a candle between it and the screen, we get the shadow of the flame. You observe the shadow of the candle, and of the wick; then there is a darkish part, as represented in the diagram, and then a part which is more distinct. Curiously enough, however, what we see in the shadow as the darkest part of the flame, is, in reality, the brightest part, and here you see streaming upwards the ascending current of hot air, as shown by Hooker, which draws out the flame, supplies it with air, and cools the sides of the cup of melted fuel.



I can give you here a little further illustration for the purpose of showing you how flame goes up or down according to the current. I have here a flame,—it is not a candle flame,—but you can, no doubt, by this time generalise enough to be able to compare one thing with another,—what I am about to do is to change the ascending current that takes the flame upwards into a descending current. This I can easily do by the little apparatus you see before me. The flame, as I have said, is not a candle flame, but it is produced by alcohol so that it shall not smoke too much. I will also colour the flame with another substance, so that you may trace its course, for with spirit alone you could hardly see enough to have the opportunity of tracing its course of action. By lighting this spirit-of-wine, we have then a flame produced, and you observe that when held in the air it naturally goes upwards. You understand now easily enough why flames go up under ordinary circumstances—it is because of the draught of air by which the combustion is formed. But now by blowing the flame down you see I am enabled to make it go downwards into this little chimney, the direction of the current being changed. Before we have concluded this course of lectures we shall show you a lamp in which the flame goes up, and the smoke goes down, or the flame goes down and the smoke goes up. You see, then, that we have the power in this way of varying the flame in different directions.



There are now some other points that I must bring before you. Many of the flames you see here vary very much in their shape by the currents of air blowing around them in different directions; but we can, if we like, make flames so that they will look like fixtures, and we can photograph them—indeed, we have to photograph them—so that they become fixed to us, if we wish to find out everything concerning them. That, however, is not the only thing I wish to mention. If I take a flame sufficiently large, it does not keep that homogeneous, that uniform condition of shape, but it breaks out with a power of life which is quite wonderful. I am about to use another kind of fuel, but it is truly and fairly representative of the wax or tallow of a candle. I have here a large ball of cotton, which will serve as a wick. And, now that I have immersed it in spirit and lit it, in what way does it differ from an ordinary candle? Why it differs very much in one respect, that we have a vivacity and power about it, a beauty and a life utterly unlike the light presented by a candle. You see those fine tongues of flame rising up. You have the same general disposition of the mass of the flame from below upwards, but, in addition to that, you have this remarkable breaking out into tongues which you do not perceive in the case of a candle. Now, why is this? I must explain it to you, because when you understand that perfectly you will be able to follow me better in what I have to say hereafter. I suppose some here will have made for themselves the experiment I am going to show you. Am I right in supposing that anybody here has played at snapdragon? I do not know a more beautiful illustration of the philosophy of flame, as to a certain part of its history, than the game of snapdragon. First, here is my dish; and let me say, that when you play snapdragon well you ought to have the dish well warmed; you ought also to have warm plums, and warm brandy, which, however, I have not got. When you have put the spirit into your dish, you have the cup and the fuel; and are not the raisins acting like the wicks? I now throw the plums into the dish, and light the spirits, and you see those beautiful tongues of flame that I have referred to. You have the air creeping in over the edge of the dish forming these tongues. Why? Because through the force of the current, and the irregularity of the action of the flame, it cannot flow in one uniform stream. The air flows in so irregularly that you have, what would otherwise be a single image, broken up into a variety of forms, and each of these little tongues has an independent existence of its own. Indeed, I might say, you have here a multitude of independent candles. You must not imagine that because you see these tongues all at once that the flame is of this particular shape. A flame of that shape is never so at any one time. Never is a body of flame, like that which you just saw rising from the ball, of the shape it appears to you. It consists of a multitude of different shapes, succeeding each other so fast that the eye is only able to take cognisance of them all at once. In former times, I purposely analysed a flame of that general character, and this diagram shows you the different parts of which it is composed. They do not occur all at once; it is only because we see these shapes in such rapid succession, that they seem to us to exist all at one time.



It is too bad that we have not got further than my game of snapdragon, but we must not, under any circumstances, keep you beyond your time. It will be a lesson to me in future to hold you more strictly to the philosophy of the thing than to take up your time so much with these illustrations.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 11, 1860.

Dr. J. P. JOULE, *President, in the Chair.*

Dr. FAIRBAIRN brought before the meeting four specimens of Submarine Telegraphic Cable, as constructed by Messrs. Hall and Wells. This cable has a copper wire insulated by india rubber in the centre for the transmission of the electric current. Outside of this are twenty longitudinal strands of hemp steeped in pitch and cork dust, and eight steel wires braided together with twenty-four strands of hemp saturated with Stockholm tar. The specific gravity of the cable in sea water is 1·4 and its weight in air 0·82 ton per mile. The length that would break with its own weight when suspended in sea water is 10,810 fathoms; its tensile strength being 2·875 tons. Dr. Fairbairn presented an account of experiments which had been made on the elongation of a sample of the cable twenty feet long by the application of different tensile forces. With a force of 4,480 lbs. there was an elongation of half-an-inch, and after the weight had been removed the cable was found to be permanently stretched $\frac{3}{16}$ths of an inch. With a force of 6,440 lbs. the cable broke after having stretched $1\frac{1}{16}$ inches.

Professor ROSCOE explained the recent discoveries by Bunsen and Kirchhoff of the lines in the spectrum produced by various substances when ignited in the flame of a laboratory lamp. He exhibited beautiful chromo-lithographic drawings of the spectra produced by lithia and various other earths and alkalis. Lithia, which had formerly been supposed to be a very rare earth, was by this means proved to be one of those most extensively distributed. Professor Roscoe stated that Bunsen had, by this new and most delicate system of analysis, been led to the discovery of a new metal which was present in a mineral spring in so small a quantity that twenty tons had to be boiled down to obtain 250 grains of the metal.

A Paper was read by the Rev. W. N. MOLESWORTH, M.A., entitled, "*On the Origin of Species.*"

PHYSICAL AND MATHEMATICAL SECTION.

November 8, 1860.

Mr. Baxendell was elected a Vice-President of the Section, in place of the late Mr. Long.

Mr. BAXENDELL read a paper, "*On a System of Periodic Disturbances of Atmospheric Pressure in Europe and Northern Asia.*"

[This paper was afterwards read at the Ordinary Meeting of the Society, on November 13.—See CHEMICAL NEWS, vol. ii. No. 52]

December 6, 1860.

Mr. George Mosley was elected Treasurer of the Section, in place of Mr. Baxendell.

Mr. ATKINSON read a paper, entitled "*Remarks on Abnormal Disturbances of the Barometrical Column at certain Seasons of the Year.*"

Mr. Atkinson considers that all the movements in the atmosphere of our earth, which have received the designation of *irregular*, are caused by the reflected or radiated heat of the sun, and take place at a very moderate elevation, say within five or six miles of the general surface level;—and that these apparently irregular movements or shiftings from place to place of lighter and heavier air, causing oscillations in the barometric column, are mainly if not wholly due to irregularities of the earth's surface. Had our earth been a globe possessing a smooth surface of uniform texture and properties, it seems clear that the atmosphere would have been acted upon by the reflected and radiated heat of the sun, in a manner so much in accordance with a uniform sequence of physical effects, that the periodic movements of the gases composing it would have been as regular as the planetary motions themselves. In the northern parts of our hemisphere, it appears by Mr. Baxendell's valuable paper, read before the General Meeting of this Society on November 13, that the barometric oscillations are least in amount when the sun is on or near the equator. This fact points to the inference that if the plane of the earth's orbit had coincided with the plane of its equator the disturbance of the barometric column wauld have been comparatively small and nearly uniform throughout the year. The coincidence of these two planes not existing, it is found, that as the sun retreats from the equator towards the southern tropic, the sum of the oscillations of mercury gradually increases for a considerable time, and then rapidly mounts up so fast as to form a prominence in Mr. Baxendell's curves resembling a mountain peak. This peak or summit of the "dynamical curve" occurs above different points of its axis—that is, at different periods of time—according to some peculiarity in the position—different from the latitude or the longitude—of the locality from which the data for constructing the curves were derived.

Speaking of the northern hemisphere, as the sun withdraws southward from the equator, less or greater portions of the northern part of the terraqueous surface becomes cooled down gradually to the freezing point, according to various peculiarities of substance, elevation above the sea-level, proximity to the open ocean, or to far-inland mountain ranges, and to other analogous causes. In similar latitudes, from the varying conditions just mentioned, there will exist, side by side, spots differing, or having a tendency to differ, very much in temperature, and where consequently currents of different density—set in motion by the constant struggle going on in the air to attain a state of equilibrium—will cause frequent fluctuations in the barometer. These disturbing causes will, in any region, be much increased at the setting in of winter and the commencement of hard frost; for at this crisis a large amount of latent heat will be liberated and will contribute its influence to disturb the equilibrium of the air. A similar crisis will occur at the end of winter on the breaking up of the frost, and will necessarily be attended with similar results. As the times of these *crises* appear to correspond in a remarkable manner with the times of maximum disturbance of the barometrical column, it seems but fair to infer that a relation exists between the causes here stated to be in operation, at the critical periods just named, and the periodical disturbance of the mercury in the barometer indicated by Mr. Baxendell's "dynamical curves." The correctness of this inference, or the contrary, can only be established by future observation of phenomena, and the collection of facts, many of them of a kind seldom thought of hitherto as constituting elements for the solution of problems in meteorology.

ROYAL DUBLIN SOCIETY.

Monday, December 17, 1860.

Mr. GILBERT SANDARS *in the Chair.*

A paper, by Mr. G. W. JACKSON and Mr. J. W. WONFOR, students in the Laboratory of the Museum of Irish Industry, "*On the Composition of Dublin Porter,*" was read by Mr. Wonfor. The authors described the methods they adopted for the estimation of the different organic and inorganic substances. The following are the results of their analyses:—

Specific gravity of the porter . 1019·565

The following are the amounts of the various constituents:—

IN 1000 PARTS, BY MEASURE, OF THE PORTER.

	I.	II.	Mean.
Total amount of fixed organic matter	67·566	66·426	66 996
Total amount of fixed inorganic matter	4·166	4·270	4·218
	71·732	70·696	71·214
Proof spirit	89·6	92·0	90·8
Acetic acid	3·6	3·6	3·6
Grape sugar	3·44	3·44	3·44
Albumen	8·272	7·50	7·886
Extractive matter	55·854	55·486	55·670
Silica	·290	·290	·290
Phosphoric acid 544 Magnesia ·307 ·851 = Phosphate of Magnesia	·851	·851	·851
Lime	·085	·085	·085
Phosphoric acid	·690	.703	·697
Chloride of sodium	·448	·449	·448
Sulphuric acid	·277	·274	·276
Potash	1·527	1·500	1·513
Soda	.082	·083	·082
	71·816	70·661	71·238

IN ONE GALLON, BY MEASURE.

Total amount of fixed organic matter		4689·70
Total amount of fixed inorganic matter		297·64
		4987·34
Alcohol (proof spirit)	6356	
Acetic acid.	252	
Sugar.	240·80	
Albumen	552·00	
Extractive matter	3896·90	
Silica.	20·30	
Phosphate of magnesia	59·71	
Phosphate of lime	11·06	
Phosphoric acid.	44·31	
Sulphate of potash	42·00	
Potash	83·16	
Chloride of sodium	31·36	
Soda.	5·74	
		4987·34

They stated that in giving the composition in 1000 parts they gave the bases and acids separately, with the exception that the chlorine they combined with sodium and the magnesia with phosphoric acid, as hypophosphate of magnesia; in the gallon they combined the lime with phosphoric acid, giving it the composition of bone-earth; the rest of the phosphoric acid and the potash and soda they left uncombined. The porter examined was Guinness's XX.

Dr. **William Barker** read a paper, "*On the Amount of the Rain-fall in Ireland during late Years.*"

Dr. **Davy** read a paper, "*On some Further Applications of the Ferrocyanide of Potassium in Chemical Analysis.*"

NOTICES OF BOOKS, PATENTS, &c.

Blue Colour for Fibres and Fabrics.

Charles Cowper, 20, Southampton Buildings, Chancery Lane.

(*Communication from* C. M. T. **du Motay**, *of Paris.*)

The patentee states the very interesting and remarkable fact that soluble Prussian blue and carmine of indigo, when mixed in the proportion of their equivalents, dissolve each other, and combine to form a new blue colour of definite chemical composition. It differs from blues generally in the important particular of retaining the same tint in solar and artificial light. The cause of this valuable property appears to lie in the circumstance that the red tint of the indigo is corrected by the supplementary green contained in the Prussian blue; the result being a pure or neutral blue. Another valuable property of this colour is that the green, formed by an admixture of it with yellow, also preserves the same tone in natural and artificial light.

Preparation.—Five pounds of prussiate of potash are dissolved in fifteen pounds (1½ gallons) of water. Two and a-quarter pounds of persulphate of iron (*sic*) are then boiled with one and three-quarter pounds of oil of vitriol, and eight pounds of water are added. [We need scarcely, perhaps, remind any of our readers who may feel inclined to try this process, of the danger of incautiously mixing water with hot oil of vitriol, and *vice versâ.*] The two solutions, prepared as above, are then to be mixed. The resulting precipitate of Prussian blue is to be thrown on a filter and drained. An equivalent of this soluble Prussian blue is then to be mixed with an equivalent of the carmine of indigo. The mixture is soluble in water, and forms the new colour. The solubility of this blue in water presents an interesting anomaly. Water, at a temperature of from 104° to 122° F., dissolves it better than when cold, but, on raising the heat above 176°, it is re-precipitated. This precipitation may be prevented by adding to the bath oxalic acid or an oxalate in the proportion of two pounds of the acid or four of the salt to every ten gallons of water. Such a bath will require ten or fifteen pounds of the new blue.

The presence of lime in water diminishes its capability of dissolving the colour. Silk dyes best between the temperature of 77° and 95° F., and wool between 140° and 176° F. The latter temperature should not be exceeded.

For printing, the new colour may be thickened as usual.

This very interesting patent discloses an entirely novel class of combinations. For the sulphindigotates of the alkaline bases (carmine of indigo) to combine with a complex body like Prussian blue to form a species of double salt is very anomalous, and we look with anxiety for a paper from M. du Motay on the chemical composition of this substance.

For the convenience of such of our readers as are not familiar with these compounds, we may mention that the general formula of the sulphindigotates is $C_{16}H_4MNO_2, S_2O_6$. Substituting any metal for M we have, of course, the formula for the sulphindigotates of that metal.

Fertilising Compound for Agricultural Purposes.

M. A. F. **Mennons**, of Paris, patents a method of working up urine into a manure. He takes advantage of Dr. Stenhouse's observations regarding charcoal. The absorbent power of gypsum is also made use of. The patentee places the urine to putrify in reservoirs closed by wire-gauze trays, containing a mixture of 2 to 5 quarts of pulverised vegetable charcoal, and 5 to 10 lbs. each of mould and gypsum. As soon as the mixture has become saturated with the gases, &c., it is taken from the tray and mixed with 25 gallons more urine, also 2½ lbs. of each of the following ingredients,—phosphate of ammonia, phosphate of lime, ammonio-magnesian-phosphate, nitre, nitrate of soda, sulphate or nitrate of magnesia, common salt, and about 4½ lbs. weight of sulphate of iron or of crude sulphate of zinc and magnesia mixed in equal parts; he then adds 14 lbs. of gypsum and, to bring the whole to a pasty consistence, 2 cwt. of burnt-clay is added. The mass is then dried and powdered. The process appears to be complicated, troublesome, and by no means satisfactory. Any method is more or less defective which requires us to add large quantities of such substances as gypsum and burnt-clay. Moreover, the valuable ingredients of urine may be separated by far more convenient processes. The patent is dated 4th January, 1860.

Separation of Silver and Lead.

P. J. Worsley, of Rotherhithe, patents a plan for the above, which appears to us to be almost absolutely the same as that generally known as Pattinson's crystallising process,—the chief difference being that, instead of fishing out the crystal with perforated ladles, the pots have strainers in them, and a hole at the bottom to allow of the liquid metal being run off. The process may be an improvement, but if such a modification be allowed to be worked as an original invention, there is not much encouragement for discoverers to patent their discoveries.

Preparation of Chlorine.

M. C. P. P. Laurents, of Rouen, patents the use of chloride of copper as a source of chlorine, as it gives that element off on heating, and may be reconverted into chloride by the action of hydrochloric acid an indefinite number of times. The patentee claims any metallic chloride capable of doing the same thing. This reaction is not in the least novel; chemists have been aware for many years that chloride of copper heated to a red heat gave off half its chlorine, and became converted into the sub-chloride. We doubt whether such a patent could stand for an instant in a court of law. We do not believe that the process possesses the merit of being economical; but if it is, and we wanted to obtain chlorine in quantity, we think we could find much more convenient sources of it than chloride of copper.

For Rendering Cotton and other Goods Uninflammable.

S. Rowbottom, of Putney, and T. Grafton, of Derby, patent the borates (neutral and acid) for this purpose. The method of applying them is pretty much the same as with the numerous other salts employed with the same intention.

Chimie Organique fondée sur la Synthèse. Par Marcellin Berthelot. Paris: 1860.

Analysis and Synthesis are the two opposite aspects of the chemical conception of Nature. In Mineral Chemistry we have long proceeded by the latter method, building up the more from the less complicated; while in Organic Chemistry the exact reverse has been the case. The chemist, in his fermentations, combustions, destructive distillations, operates for the most part analytically, and in direct opposition to the action of living Nature, which reconstructs the edifice broken down by these chemical processes. It is the object of the present treatise to show how the synthetical laws of Mineral Chemistry may be applied to the formation of organic compounds; to point out, in fact, how Organic Chemistry, instead of starting from the phenomena of life only, possesses an independent origin.

The two ponderous volumes, in which the author expounds his views and details his experiments, are replete with interesting facts and suggestions, but there is in them much repetition, and the style is in many places exceedingly diffuse. We shall scarcely attempt to do more than quote a few illustrative examples of synthetical methods, referring our readers to the work itself for further information.

From carbon we may obtain carbonic oxide; then by combining this with water, formic acid is produced; this may be converted into a metallic formiate, which, submitted to distillation, gives rise to various hydro-carbons, such as marsh-gas, olefiant-gas, and propylene. These hydro-carbons become, in their turn, means of constructing alcohols, for marsh-gas, united with oxygen, becomes methylic alcohol, and olefiant-gas, with the elements of water, constitutes ethylic alcohol.

It is in the first steps, the production of the hydrocarbons and the alcohols, that the chief difficulties of organic synthesis lie; the subsequent developments, such as the formation of the aldehydes, the acids, the ethers, the amines, and the amides being comparatively easy. Given any one of the four simple carbon compounds,—CO_2, CS_2, C_2Cl_4, C_2H_4, the other three may be obtained from it. Take the chloride as an instance. This body, heated with a mineral sulphide, yields CS_2; with an alkali, CO_2; and at a dull red heat with hydrogen it produces C_2H_4. So we may return in each case from the transformed to the primitive substance.

A *résumé* of the historical development of Organic Chemistry, and an account of the general laws of reactions, and the influence under which they are effected is given. One thing we must here mention,—the inadequate references to the labours of Laurent and Gerhardt: in his infrequent notices of their discoveries, the author sometimes contrives to misrepresent their views (*e.g.* p. lxxxv. of the Introduction); if it be unintentional the mistake is unfortunate; but if intentional, it is indeed to be deplored. The subjects treated of in the Introduction, which extends to 188 pages, are grouped as follows:—§ 1. Analysis and Synthesis. § 2. Elements and Immediate Principles. § 3. Elementary Composition; Equivalents. § 4. Analysis by Gradual Decomposition. § 5. Chemical Functions of Organic Bodies, and their Classification. § 6. Of Synthesis in Organic Chemistry.

In the First of the Four Books, into which the author divides the work itself, the synthesis of hydrocarbons is described. The production of formic acid from carbonic oxide and water is here the preliminary step, for by the distillation of formiates various hydrocarbons may be formed. One of the most interesting examples given is the formation of naphthaline from the so-called "chloride of Julin," $C_{20}Cl_{10}$, which is found among the last products of the decomposition of the chloride of carbon. The reaction is—

$$C_{20}Cl_{10} + H_{18} = C_{20}H_8 + 10\,HCl.$$

As we mentioned above, olefiant gas appears among the products of the dry distillation of formiates; from this gas alcohol may be prepared, and thence acetic acid. From acetates, propylene, butylene, and amylene may be produced. From alcohol itself, by submitting its vapour to a red heat, benzole, naphthaline, and phenic acid are obtainable.

Book II details the synthetical processes by which the various alcohols may be produced. In marsh-gas, the hydride of methyle, 1 equivalent of chlorine may be substituted for 1 equivalent of hydrogen, and the true chloride of methyle thus formed. This, heated with potash-solution at 100° C. for a week, yields methylic alcohol—

$$C_2H_3Cl + KO, HO = C_2H_4O_2 + KCl.$$

The author proceeds to describe at length the procedure in the case of ethylic alcohol produced from olefiant-gas, and of propylic alcohol from propylene. In the former instance the result is attained by means of sulphuric acid, and the formation of an ethylsulphate; in the latter, by means also of hydrochloric acid, with which propylene unites directly to form the chloride of propyle—

$$C_6H_6 + HCl = C_6H_7\ Cl.$$

Sect. 8 contains an interesting account of the formation of the benzylic, cymenic and cinnamic alcohols. From toluole, $C_{14}H_8$, we may prepare the chloride of toluenyle, $C_{14}H_7Cl$, from this the acetate of toluenyle, $C_{14}H_7$, $C_4H_3O_4$, and from this benzoic (called by Berthelot *benzylic*) alcohol. This alcohol may also be formed by taking as a starting point benzoic acid, the successive steps being the formation of the chloride of benzoyle, $C_{14}H_5O_2Cl$; the cyanide, $C_{14}H_5O_2Cy$; and the hydride or benzoic aldehyde, $C_{14}H_5O_2H$.

Berthelot gives the following scheme, by means of which we may arrive at the higher terms of the benzoic series:—

Starting from benzoic acid	$C_{14}H_6O_4$
we may prepare benzylic alcohol . . .	$C_{14}H_8O_2$
and from this the cyanide	$C_{14}H_7Cy$
which boiled with potash yields an acid .	$C_{16}H_8O_1$
and which, by analogy, should give an aldehyde	$C_{16}H_8O_2$
and thence xylenic alcohol	$C_{16}H_{10}O_2$
from this the derived cyanide . . .	$C_{16}H_9Cy$
might be obtained, and thence an acid .	$C_{18}H_{10}O_4$
and so on, consecutively, we might pass to	
cumolic	$C_{18}H_{12}O_2$
and to cymenic alcohol	$C_{20}H_{14}O_2$

But, as Berthelot observes, it is easier to indicate than to realise this series of reactions. Several chemists have, however, already made some progress in this direction, and it offers a field of inquiry full of promise. In the above list we have used the terms assigned by Berthelot to the various compounds mentioned, but we shall have a word to say on this nomenclature further on.

In Sect. 9 the author gives a method for the formation of campholic alcohol by heating camphor with potash. The campholic series is as follows:—

Campholic hydride (camphene)	$C_{20}\ H_{16}$
" chloride	$C_{20}\ H_{17}\ Cl$
" alcohol (camphol)	$C_{20}\ H_{18}\ O_2$
" aldehyde (camphor)	$C_{20}\ H_{16}\ O_2$
Camphic acid	$C_{20}\ H_{16}\ O_4$
Camphoric acid	$C_{20}\ H_{16}\ O_8$

The extracts and remarks which we have now made will give some idea of the nature of the work before us, and our limits will not allow us to do more than quote the titles of some of the sections which constitute the remainder of the work. We have details concerning cholesterine, or cholesteric alcohol and its ethers, concerning the derivatives of the alcohols properly so called, concerning the nomenclature of organic compounds, concerning ethers, hydrocarbons, alkalies, compound metallic radicals, aldehydes, acetones, acids, amides, cyanides and cyanic compounds, polyatomic alcohols and aldehydes, and phenoles. The Third Book contains an elaborate account, well worth attentive study, of glycerine and its numerous derivatives, and also of the various kinds of sugar. The Fourth Book relates to "Methods."

We cannot conclude the present notice of M. Berthelot's treatise without a few observations on his formulæ and nomenclature.

Berthelot rejects the expression toluole, cumole, &c., because they refer to acids whose carbon equivalents are higher than those of these derived bodies; and he, in consequence, introduces a further confusion into the names of these compounds. For instance, toluole becomes *benzoène*, and its derivatives therefore liable to be mistaken for those of benzine; thus the alcohol corresponding to the former hydro-carbon is named the *benzylic*, that corresponding to the latter the *benzenic*. So also cuminic alcohol, $C_{20}H_{14}O_2$, is termed "cymenic alcohol," while a similar change is made in the names of the lower members of the series. The author's reasons for such awkward alterations seem by no means conclusive. He says he has abandoned the expressions "hydride of ethyle," "chloride of ethyle," &c., because they rest upon an unproven hypothesis. Other names, as those of the compound ethers, are rejected, because they are founded on hypothesis, and tend to affirm an illusory resemblance between ethers and salts. We suggest that such resemblance is by no means altogether illusory. What is the meaning of the following parallel equations, quoted from M. Berthelot's own work,—do they establish an illusory resemblance only?

$$Ba\ Cl + SO_3,\ Na\ O = SO_3,\ Ba\ O + Na\ Cl$$

$$C_2H_3Cl + C_4H_3O_3,\ Na\ O = C_4H_3O_3,\ C_2H_3O + Na\ Cl.$$

The reactions are parallel, and the products present analogies quite as distinct as could be expected between bodies whose constituents are so different, especially when the complicated character of organic compounds, and the multitude of metamorphoses to which they are consequently subject, is considered. So, according to our author, all formulæ are to be rejected which rest upon hypothesis; but are there not at least four hypotheses contained in the expression given by Berthelot for the propylsulphate of baryta?—

$$S_2O_6,\ C_6H_6,\ HO,\ BaO + 6\ aq.$$

We confess to have been surprised at the parallelism which Berthelot insists on, between the oxides and chlorides of organic radicals. The author seems to pride himself on a return to obsolete or obsolescent formulæ and views; with the remembrance of Williamson's and Gerhardt's researches fresh in our minds, the following equation scarcely pleases us,—

$$C_4H_5\ Br + KO = C_4H_5O + KBr.$$

Surely the expression methylchlorhydric ether involves as many suppositions as the simpler form, chloride of methyle. Ether, C_4H_5O, C_4H_5O, becomes hydric ether with the formula C_4H_5O. Anhydrous formic acid is actually written with the formula C_2HO_3, and made to correspond exactly with the perchloride of formyle, C_2HCl_3, in direct contradiction to the principles involved in its synthetical production, upon which Berthelot in many cases lays so much emphasis. Our author's views of the probable constitution of the mixed alcohol radicals are strange; methylethyle, C_2H_3, C_4H_5, becoming with him, ethylformène with the formula C_2H_4, C_1H_4. It is not easy to understand the true relations of glycerine and its numerous derivatives—the history of which has been, for the most part, traced by Berthelot himself,—from the reactions and formulæ given in the present work. He accepts the view propounded by other chemists, that glycerine is a triatomic alcohol, but makes no attempt to show how the constitution of the various compounds which can be obtained from this interesting substance may be illustrated by the development of this view.[1]

CORRESPONDENCE.

Preparation of Chrome Yellow.

To the Editor of the Chemical News.

Sir,—In reply to a correspondent whose letter appears in No. 55 of the Chemical News, the orange colour of his precipitate is owing to the presence of a subchromate of lead. The colour of the chromates of lead varies from a bright red to a pale yellow,—all the intermediate colours may be produced by varying the mode of preparation.

To prepare the red chromate of lead, precipitate tribasic acetate of lead with monochromate of potassa (the tribasic acetate may be prepared by boiling ordinary sugar of lead with litharge). To prepare the yellow chromate of lead, precipitate the monobasic acetate of lead with bichromate of potassa.

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[1] See Chemical News, vol. ii., p. 141.

The ordinary sugar of lead generally contains an excess of base, which gives the precipitated chromate of lead an orange colour. This may be corrected by adding sufficient acetic acid to give the solution of acetate of lead a faint but distinct acid reaction before adding the bichromate of potassa.

I may, however, remark, that the use of chromate of lead as a pigment is scarcely more advisable than the use of arsenite of copper, it being almost, if not quite, as poisonous.—I am, &c. JAS. HARGREAVES.

Dale's Patent for the Preparation of a Colouring Matter.

To the Editor of the CHEMICAL NEWS.

SIR,—Nothing can surprise a man much more than to find a process with which he has been for years familiar put forth as a new invention and actually patented. You may judge, therefore, what were our feelings on reading, in No. 54 of the CHEMICAL NEWS, a notice of a patent taken out by Mr. J. Dale for the "Preparation of a Colouring Matter." It so happens that our Mr. Robinson applied this very process—extracting the colouring-matter of all the known dye-woods with alkaline solutions, and afterwards neutralising with an acid—no less than six years ago! Since that time we have been in the habit of making more or less of these preparations. If asked why did we not anticipate Mr. Dale with a patent, the answer is very simple. The principle, even in our hands, was not novel! The makers of dye-wood extracts have long been in the habit of using, in certain cases, weak alkaline solutions to draw out the last traces of colouring matter, and of subsequently neutralising with an acid, or an acid salt.

We are surprised that Mr. Dale should be ignorant of a fact thus known to the trade; but we are still more unwilling to believe that he could be aware of the custom and yet make a declaration claiming the principle as his own invention. For obvious reasons we must decline pronouncing whether the mechanical arrangements, proportions, &c., specified by Mr. Dale are the best in existence. Commercially we have no interest in the matter, as we have come upon a totally distinct principle for effecting the same object in a superior manner.

We are sorry to add that this is not the only case of a patent being taken out for some well-known process. Is every person free to claim as a novelty, and to monopolise by means of a patent, any process which he does not find described in common works of reference? Are manufacturing chemists, dyers, and printers to be debarred from methods which they—and perhaps their fathers before them—have for years pursued, or to be driven into continual litigation to maintain their rights?

Does not the law as regards chemical patents sadly need improvement?—We are, &c.

ROBINSON AND SLATER.

Moorhead Chemical Works, Huddersfield.

The Homogenesis of Forces.

To the Editor of the CHEMICAL NEWS.

SIR,—Perhaps I may be late in asking Mr. Rodwell a question through the medium of your Journal. *Does* steel become magnetic when placed in a particular portion of the solar spectrum? Are we justified, too, in classing motion among the forces? The difference between matter in motion and at rest has engaged the attention of many an able geometer and metaphysician without eliciting anything satisfactory. Is it not more a condition of matter than a force?

But in making gravitation, the primary force, productive of physical phenomena, Mr. Rodwell seems to ignore the equally inherent attractive forces of cohesion, or the repulsive energy exhibited by all matter at insensible distances. And what also are we to say about the operations of the centrifugal force? Until he can experimentally prove the effect which gravitation would produce when unaffected by these counterbalancing causes, I think we must hesitate to accept the proposed solution. When in such a property of bodies as elasticity we discover palpable operations of a repulsive force, we cannot yield to gravity an undivided sway over natural phenomena and physical laws.

I fear the theory is as the mouse at the tenth hole.—I am, &c. E. M. T.

Detection of Bisulphide of Carbon in Coal-Gas.

To the Editor of the CHEMICAL NEWS.

SIR,—I find that by forcing by an ordinary blow-pipe the flame of coal-gas for about a minute, on to distilled water containing a little acid chloride of barium, sulphate of baryta is formed, and the presence of sulphur in the gas thus proved.—I am, &c. J. T. B.

Chemical Notices from Foreign Sources.

1. MINERAL CHEMISTRY.

Extraction of Iodine from Peruvian Nitrate of Soda.—M. Leon Krafft has found that the mother liquors obtained in the purification of nitrate of soda by recrystallisation contain considerable quantities of iodine (*Le Moniteur Scientifique*, t. ii. liv. 95, p. 1070). The method he has adopted for separating it is by adding protochloride of copper which precipitates the whole of the iodine. M. Krafft has also found it in the mother liquor of saltpetre and in the chloride of potassium of commerce, but not in sufficient amount to pay for the extraction.

A Crystalline Alloy of Tin and Iron.—Noellner has noticed (*Annalen der Chemie und Pharmacie* bd. cxv. s. 233) that the deposit formed when salt of tin is prepared with commercial hydrochloric acid contains microscopic needles, soluble in aqua regia, which have a composition agreeing with the formula $Sn_2 Fe$. The author has found that the compound pre-exists in some tins. He has observed also that a solution of salt of tin prepared without heating the mixture of granulated tin and hydrochloric acid contains twice the quantity of acid necessary for the formation of the salt, but the excess is saturated when the mixture is heated. There is, however a loss of hydrochloric acid.

Desulphuration of Native Sulphides.—MM. Bronac and Deherrypon (*Polytech. Journal*, t. clvii. p. 342) effect the desulphuration and dephosphuration of ores by the use of what is called the *iron sponge*. This iron sponge is the invention of M. Chenot, and is made by reducing dry ores of iron by means of carbonic oxide. The native sulphides are reduced to powder and then mixed with a proper proportion of the iron sponge. The mixture is then placed in a reverberatory furnace and covered with a layer of charcoal powder to prevent the oxidation of the iron. The process is said to be particularly applicable to the treatment of galena and sulphide of antimony, inasmuch as it does away with the necessity for a preliminary roasting of the ore, and the reduction is effected at a lower temperature. The cost is said to be considerably lower than the usual method.

Action of Sulphuretted Hydrogen on Neutral Salts of Zinc.—Wernike has noticed (*Annalen der Physik und Chem.* bd. cx. p. 655) that when a solution of sulphate of zinc which has been treated with sulphuretted hydrogen as long as a precipitate is formed is filtered and set aside; it after a time becomes turbid and deposits plates, which, however, he has not analysed.

Oxide of Zinc for Polishing Glass.—M. Pohl (*Ibid.* p. 201) recommends oxide of zinc for polishing glass intended for optical instruments. The author states that it answers perfectly, and is quite free from the objections brought against the use of the oxides of iron and tin.

II. ORGANIC CHEMISTRY.

New Compounds of Sugar with Acids.—M. Berthelot, in a memoir to be found in the *Annales de Chimie et de Physique*, t. lx. p. 93, describes some compounds of glucose with stearic, butyric, acetic, and benzoic acids :—

I. *Stearic Glucose*, obtained by heating a mixture of stearic acid and dehydrated glucose to 120° for about sixty hours. It is a neutral, solid, colourless body, like stearine in appearance, fusible, soluble in ether and absolute alcohol, but not in water. Its formula is $C_{84}H_{78}O_{14}$.

II. *Butyric Glucose.*—Obtained by a process similar to the above. It is a neutral oily liquid, slightly soluble in water, and soluble in ether and spirit. It has a very bitter taste. Formula, $C_{28}H_{22}O_{14}$.

III. *Acetic Glucose.*—Prepared as above; is a neutral, oily, colourless liquid, soluble in alcohol, ether, and water, and having a very bitter taste.

IV. *Benzoic Glucose.*—Obtained like the above. A thick, neutral, bitter liquid, soluble in ether and alcohol, and slightly so in water.

V. *Ethyl Glucose.*—Prepared by heating a mixture of cane-sugar, hydrobromic ether and potash to 100° for several days. The product is shaken up with ether, and the etherial solution is evaporated *in vacuo*. It is a fixed oil, insoluble in water, bitter, and having an agreeable odour. Formula, $C_{20}H_{18}O_{10}$.

The author remarks that the sugars do not combine with acids only. Like polyatomic alcohols they may combine with other alcohols; and further may unite at once with alcohols and acids.

New Cinchona Alkaloid.—Schwabe (*Archiv. der Pharmacie*, bd. cii. s. 273) has extracted from quinoidine a new alkaloid which he calls β-cinchonine. To obtain it quinoidine is dissolved in dilute hydrochloric acid, then precipitated with ammonia; the precipitate is washed first with cold, then with hot water; afterwards dried and subsequently treated with cold alcohol, which in about twenty-four hours gives a brown tincture. The residue is exhausted successively with alcohol and then with water, and afterwards dissolved in dilute sulphuric acid. This solution is warmed, and, while hot, a solution of carbonate of soda is added until a crystalline pellicle forms. It is then set aside to cool, whereupon crystals of the sulphate of *beta-cinchonine* are deposited. These are a little coloured, but may be purified in the usual way. By dissolving the sulphate in acidulated water, precipitating by ammonia, and crystallising from alcohol, the pure alkaloid is obtained in right rhombic prisms.

Beta-cinchonine $C_{20}H_{12}NO$ is anhydrous, and fuses at 150° C.; on cooling it becomes a radiated mass; it is not volatile, and burns with a smoky flame. It is easily distinguished from the other cinchona alkaloids by its crystalline form, chemical properties, and particularly by its solubility.

The author gives the following table of the solubility of the cinchona alkaloid in different menstrua :—

	α Quinine.	β Quinine.	α Cinchonine.	β Cinchonine.
Cold water .	400 parts	15,000	insoluble	insoluble.
Boiling water .	200 "	750	2500	slightly sol.
Cold alcohol .	2 "	45		173
Boiling alcohol.	2 "	37	30	43
Ether . .	60 "	90	insoluble	378
Chloroform .	6 "		40	268

Fatty oils dissolve beta-cinchonine. The alcoholic solution turns the plane of polarization to the right. The following are the chemical reactions of salts of the new alkaloid :—Chloride of gold gives a sulphur yellow precipitate; bichloride of mercury gives a white precipitate, which soon becomes resinous; bichloride of platinum an orange yellow precipitate, soluble in much water; iodide of potassium a white precipitate; iodated water a red brown; bromated alcohol a yellow precipitate; picric and phospho-molybdic acids a yellow precipitate. With chlorine and ammonia the solution becomes yellow. Gallic acid gives a yellowish white precipitate; and prussiate of potash gives a rose coloration, as with the solutions of quinine, cinchonine, and quinoidine. The author has not succeeded in obtaining neutral salts with beta-cinchonine; all are basic. The sulphate crystallizes in prisms with a rhombic base; it is insoluble in ether, but soluble in alcohol and water. The hydrochlorate crystallizes with 4 equivalents of water in prisms which are slightly soluble in ether. With bichloride of mercury they form an amorphous double salt. With bichloride of platinum they form six-sided rhomboidal prisms. Iodine gives with the sulphate a salt resembling the compound obtained with iodine and sulphate of quinine. It is made by dissolving (at 30° C.) 10 parts of the sulphate in a mixture of 144 parts of acetic, and 12 parts of dilute sulphuric acid, to which is added 3 parts of iodine dissolved in 115 parts of alcohol. On cooling, plates without definite form separate from the solution.

MISCELLANEOUS.

THE SPECIMEN EXCHANGE CLUB.

For some time past we have received letters on the subject of exchanging specimens of chemical substances, some merely approving the idea, others containing suggestions for carrying it out in practice. Of the latter we have published several, but in doing so we have not desired it to be understood that we have in all cases approved the suggestions offered. One writer, for example, proposes that the sender of a specimen or specimens should communicate the best method of preparing them. Such information would, no doubt, be very useful; but is, we fear, impracticable, because no man whose time is valuable would take the trouble to write three or four or more descriptions, according to the number of specimens; not to speak of other reasons he might have for declining to furnish it; this, therefore, we cannot at present incorporate in the Regulations. Another suggests that chemists not having specimens to exchange should be enabled to purchase. There are many objections to this which we need not enumerate; it would, moreover, be impossible for us to spare the time which would be required for regulating money transactions. This difficulty might possibly be overcome at a future time, but we do not think it advisable to encounter it at present. As it is evident that the fewer the Rules the better, we now propose the following, and recommend that each member shall adhere strictly to them, until experience has shown whether any addition or alteration can be made with advantage.

Rule 1. To prevent dissatisfaction on the score of relative value of specimens, each specimen should be in such a quantity as the sender may fairly consider of the value of two shillings and sixpence. It will be evident that some such standard should be adopted, or an exchange of specimens of different values would be impracticable.

Rule 2. The specimen to be securely enclosed, if liquid, in a stoppered bottle; or, if solid, in a glass tube, distinctly and properly labelled, with full particulars as to formulæ, &c.

Rule 3. Specimens of the rarer minerals may be sent, provided they are of the estimated value stated in Rule 1, packed in pasteboard boxes, and having the name legibly written on the cover.

Rule . As the amount required for postage of each specimen will be published in the list, alongside the article, every applicant must send the requisite number of stamps for the purpose.

Rule 5. Each person requesting to have his name inserted in the list will be required to send, at the same time, to the Editor, a properly labelled specimen of the substance he desires to exchange, having his own name and address written on the cover. This will be kept for comparison, in the event of any complaint being made by a member of the Club, at a future time, with respect to the quality of the substance sent; and no person's name will be inserted without compliance with this rule.

Let us now state what we are willing to do to further the wishes of our subscribers.

We will, at certain stated intervals, publish in the News a list of specimens which members may desire to exchange for others. The member's initials, or other distinctive sign will be affixed to each, together with the number of stamps required to carry it through the post. In future Numbers we will, when necessary, intimate to a member,—Mr. Smith, for instance,—to whose initials are appended the words "*Oxide of Lanthanium*, 2d.," that Mr. Jones, "*Oxide of Yttrium*, 1d." desires to exchange yttria for lanthania, and has accordingly sent a specimen oxide of yttrium, with a 2d. postage-stamp, to our office. If Mr. Smith does not care about the exchange, he simply take no notice of it; whereas, if he desires to meet Mr. Jones's wishes in the matter, he will forward a specimen of his oxide of lanthanium, with a 1d. stamp, to our office, when we will forward the specimens to the respective addresses.

We are willing to receive the specimens sent for exchange, and to effect the exchanges in accordance with the wishes of the senders, so far as this may be practicable. It may occasionally happen that there will be a run on some particular substance, but a judicious system of exchanging and re-exchanging will, we hope, satisfy the desire of all persons. So far as regards the trouble attending this exchange, the posting of the specimens, &c., we undertake it cheerfully, only asking that, if any person considers he has cause to be dissatisfied with the arrangement, he will not visit his displeasure on us. That a hitch will now and then occur is probable, especially at the commencement, but so long as every individual acts in perfect good faith, the Specimen Exchange Club cannot fail to be beneficial to the members who compose it.

Iodide of Starch.—At the last sitting of the French Academy of Sciences, M. Duroy announced the discovery of a new neutral and colourless iodide of starch. It is well known that iodine gives a blue colour to starch, thus forming an iodide. M. Duroy brings this iodide into contact with yeast, and thus deprives it of its colour. In this state it is very soluble in water, insoluble in alcohol, sweet, gummy, and incapable of crystallisation.

The Act for Preventing the Adulteration of Articles of Food and Drink.—The various Boards of Works are busily considering the best means of securing to the public the advantages of this important measure. Public Analysts will be appointed forthwith in every locality; and no doubt properly-qualified persons will be sought out with care for this highly responsible duty. We are glad to see already, that the selection will be made in many cases in strict accordance with the rule of appointing not the local self-seeker, but the most approved candidate.

NOTICE OF REMOVAL.

Subscribers and the Trade are respectfully informed that the Office of this Paper has been Removed to Messrs. Griffin, Bohn, and Co., 10, Stationers' Hall Court, E.C.

ANSWERS TO CORRESPONDENTS.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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*** All *Editorial Communications* are to be addressed to Mr. Crookes and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

J. Sharples.—We are much obliged by the information. We have however, received a previous communication on the same subject.

J. McKelnie.—The book was forwarded by book-post last week.

H. Sherman.—A full answer to your question as to the best plan to produce from 1 to 7 tons per week of sulphuric acid at cheapest cost from pyrites and sulphur could not be given in the space at our disposal in this column. We will endeavour to find the best description of what you require, and will refer you to it in this column in a subsequent number,

Parchment-paper, E. Herring.—You must have had the acid too dilute. When properly prepared and well washed, parchment-paper, forms an admirable covering for jars instead of bladder, being very strong and tough when dry.

J. S.—You must file off some of the outer surface and then test the under part to see if the article is electro-plated. The deposit on the surface being pure silver would of course give the reactions of silver.

Soap Making.—Alpha desires to know if there is any monograph on the science and art of soap making.

Anhydrous Chloride of Tin.—Mix dry and warm stannic sulphate with an equal weight of dry, warm common salt, both in fine powder, distil in an iron retort and rectify the product in a glass retort with the addition of about three times its weight of oil of vitriol.

Reduction of Cryolite in Clay Crucibles.—F. W. G.—Under ordinary circumstances the silica of the crucibles will be attacked, but this can be avoided by following Wöhler's method, as follows:—Finely triturate and well dry the cryolite and mix it with an equal weight of a mixture of 7 parts of chloride of sodium and 9 parts of chloride of potassium previously fused together and finely powdered. Place the mixture into the crucible in alternate layers with discs of sodium strongly compressing the separate layers. To 50 parts of the mixture 8 to 10 parts of sodium are employed. The crucible must be previously well dried, and the whole is to be put into a hot wind furnace with a good draught, surrounded with hot coals and brought quickly to a full red-heat. The mass is readily fusible without eating through the crucible and the aluminium obtained is free from silicium, and will be about 4 per cent. of the weight of cryolite taken.

Books Received.—"The Successful Application of Charcoal Air-filters to the Ventilation and Disinfection of Sewers," a Letter to the Right Honourable the Lord Mayor, W. Cubitt, M.P., by Dr. John Stenhouse, F.R.S. London: John Churchill. "A Veterinary Toxicological Chart," by W. J. T. Morton, late Professor of Chemistry and Materia Medica, at the Royal Veterinary College. London: Longmans. "Report of Experiments on the Growth of Red Clover by different Manures," by J. B. Lawes, F.R.S., and Dr. J. H. Gilbert, F.R.S London: W. Clowes and Sons. "On the Sources of the Nitrogen of Vegetation," by J. B. Lawes, F.R.S., Dr. J. H. Gilbert, F.R.S., and E. Pugh, Ph.D.

Erratum in No. 55.—P. 334, col. 2, lines 13 and 14 from top, for "1 ounce" *read* "1 drachm" in both cases.

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To the Medical Profeſſion of Great Britain and Ireland.

GENTLEMEN,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports,*" *vol.* 1, *p.* 317.

> "Since this caſe came into my poſſeſſion, I have been led to make a good many clinical experiments, which have enabled "me to verify the reports which I had heard of the efficacy of JAMES'S POWDER, in ſometimes removing the APOPLECTIC "DIATHESIS in perſons advanced in life. It is, moreover, worthy of obſervation, that JAMES'S POWDER has been of "remarkable utility in certain inſtances of DETERMINATION OF BLOOD TO THE HEAD, which occurred at an early period of life, and "threatened to end by effuſion. Laſtly, in two caſes of GENERAL PLETHORA, in which, however, the head was more affected "than any other part, JAMES'S POWDER was exhibited by me with perfect ſucceſs."

Alſo *vol.* 1, *p.* 320.

> "The following very ſimple method of exhibiting JAMES'S POWDER, in caſes of undue determination of blood to the "head, is that which I have generally purſued. The patient is made to begin with a very moderate doſe, not more than "two grains at bedtime, and to increaſe the doſe by half-a-grain every night, until ſome ſenſible effect is produced upon the "ſtomach, bowels, or ſkin. Should the ſtomach be affected with ſickneſs, the doſe muſt be leſſened by one grain on the "following night. By the addition of a little rhubarb to it, a larger quantity of JAMES'S POWDER may be adminiſtered "than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be "increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen "or twenty grains taken for a conſiderable period without any inconvenience."

The following extract (showing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet,*" *June* 16, 1860, P. 590,) as follows:—

> "If the febrile excitement be ſuch as to require antimonial preparations, a pill containing 2 or 3 grains of James's Powder (prepared "by Newbery) ſhould be taken at intervals according to circumſtances."

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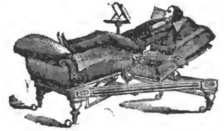
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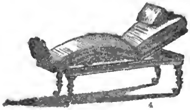
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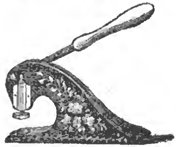

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To the Medical Profeſſion of Great Britain and Ireland.

GENTLEMEN,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports,*" *vol.* 1, *p.* 317.

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Alſo *vol.* 1, *p.* 320.

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> " than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be
> " increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen
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The following extract (ſhowing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet,*" *June* 16, 1860, P. 590,) as follows:—

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THE CHEMICAL NEWS

Vol. III. No. 58.—January 12, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Solubility of the Chloride, Bromide, and Iodide of Silver in Certain Solutions, by Frederick Field.

In continuing some experiments upon the separation of chlorine, bromine, and iodine, the solvent action of solutions of certain salts upon those elements, when in combination with silver, was tried. In a recent paper upon the subject, it was shown that a comparative large excess of iodide or bromide of potassium—provided the solution be dilute—has little or no action upon the iodide or bromide of silver. This fact has since been more fully confirmed by the following experiments:—

A solution of iodine of potassium was made by dissolving 10 grains of the salt in 6 ounces of water, and nitrate of silver carefully added. The solution of the latter salt was prepared by dissolving 1 grain in 1000 grains of water. The addition of 5 grains of this solution, containing $\frac{1}{200}$, or 0·005 grains of nitrate of silver, or 0·0031 of the metal, produced a permanent cloudiness in the liquid. 10 grains, containing $\frac{1}{100}$ of a grain of nitrate, or 0·0062 of metal, gave, as might be expected, a greater turbidity, and the liquid could be filtered clear after standing for some hours. 10 grains of bromide of potassium were next dissolved in a similar quantity of water, and the same amount of nitrate of silver added. A slight turbidity was occasioned; not quite so much, perhaps, as with the iodide, but still sufficiently apparent. 10 grains of chloride of sodium, under similar circumstances, gave a slight turbidity, which did not altogether disappear after several hours' standing.

It appears from these experiments that very weak solutions of the iodide, bromide, and chloride of the alkaline metals have little or no action upon their respective silver salts, although, when concentrated, the solvent action—especially of the iodide—is considerable.

Gmelin tells us, under the head of "Iodide of Silver" (see Gmelin's "Handbook," vol. vi.),—upon whose authority we are not informed,—that the chlorides of sodium and potassium dissolve iodide of silver abundantly. As this is in perfect contradiction to the results of my experiments in 1857, some of which are published in the *Quarterly Journal of the Chemical Society*, further researches were entered into. 0·005 of a grain of nitrate of silver were added to 4 ounces of a saturated solution of chloride of sodium, containing about 0·10 grain of iodide of potassium. As the chlorine cannot combine with the metal as long as any iodine, in the form of soluble iodide, exists, iodide of silver must be formed. 0·005 of a grain of the nitrate is equal to 0·0031 of silver, and this is converted into 0·0067 iodide of silver. This small quantity was immediately precipitated, showing the great insolubility of iodide of silver in an immense excess of cold solution of chloride of sodium. A few grains of iodide of silver were digested with 8 ounces saturated solution of chloride of sodium for some hours. No trace of silver could be found in the filtrate. Even boiling solution of common salt only dissolves traces of the iodide, and this is re-precipitated on cooling.

A similar experiment was tried with a concentrated solution of chloride of potassium, with precisely the same results.

The action of hyposulphite of soda upon this class of silver compounds is interesting. It is generally supposed that the chloride, bromide, and iodide of silver are very soluble in a solution of this salt; but this is not strictly true with regard to the two latter, especially if they are suspended in a considerable quantity of water; and it is worthy of remark that iodide of potassium precipitates iodide of silver from its solution in hyposulphite of soda, and bromide of potassium exercises a similar action upon bromide of silver when dissolved in a solution of the same salt. No excess, however, of chloride of sodium precipitates chloride of silver when once dissolved in hyposulphite of soda.

10·00 grains of nitrate of silver were dissolved in water, and precipitated by the addition of chloride of sodium in slight excess. The chloride of silver, after washing, was suspended in 4 ounces of distilled water, placed in an eight-ounce glass-stoppered bottle.

10·00 of nitrate of silver were treated in an exactly similar manner, bromide of potassium being substituted for chloride of sodium.

10·00 of nitrate of silver were precipitated by a solution of iodide of potassium, the iodide of silver being subsequently suspended in the same quantity of water as the chloride and bromide.

A highly-concentrated solution of hyposulphite of soda was prepared and poured into a graduated burette. 200 grains by measure of this liquid added to the 8 ounces of water in which the chloride of silver was suspended, entirely dissolved that compound, the liquid becoming perfectly clear. The bromide of silver required more than 800 grains from the burette to effect its complete solution, while the iodide did not entirely dissolve until after the addition of 1750 grains. To the solution of the chloride of silver in the hyposulphite of soda from 10 to 12 grains of the iodide of potassium were added, when an immediate precipitate of iodide of silver took place, which, after a few hours' digestion, was filtered off, washed, and weighed. Nearly all the silver was found to have been removed from solution by the introduction of the iodide of potassium, as the iodide of silver weighed 13·78, within 0·04 of the calculated amount, supposing the whole to have been precipitated. The filtrate afforded a brownish colour on the addition of sulphuretted hydrogen, which, after prolonged digestion in a warm situation, deposited black flakes of the metallic sulphide. As might be expected, iodide of potassium threw down by far the greater portion of the silver from the bromine compound in the alkaline hyposulphite, but not so completely as in the former case, owing, doubtless, to the larger quantity of hyposulphite of soda present, which had been added for the entire solution of the bromide of silver. Care was taken in each of the above experiments to add no great excess of iodide of potassium, but only just sufficient to decompose the metallic chloride and bromide, as it was found, as formerly stated, that an excess of iodide of potassium precipitates iodide of silver from its solution in hyposulphite of soda. 10·00 of iodide of potassium were added to the liquid in the bottle containing the iodide of silver in the hyposulphite, when an immediate precipitate took place. This was filtered off and found to weigh 5·34 grains, or considerably more than a third of the whole quantity of iodide of silver. The addition of a further quantity of iodide of potassium to the filtrate threw down more iodide of silver in small but distinct yellow crystals. These crystals are dissolved on heating the supernatant liquid, and deposited more beautifully as the solution cools.[1] This is not the first time iodide of silver has been obtained in a crystalline form. I mentioned[2] that by boiling red iodide of mercury with slight excess of nitrate of silver, the nitrate of mercury formed by decomposition dissolves the iodide of silver, which crystallises from the filtrate in brilliant crystalline plates.

There can be little doubt that iodide of silver is very insoluble in dilute hyposulphite of soda. It is only necessary to dissolve a small quantity of chloride of silver in a weak solution of this salt, and a drop of iodide of potassium in water will convince us of the fact. The same is the case, although to a far less extent, with bromide of silver. A solution of bromide of potassium precipitates bromide of silver from the solution of the chloride in the hyposulphite, and bromide of silver is also precipitated, but not in such abundance as the iodide under parallel circumstances, by the addition of bromide of potassium to its solution in hyposulphite of soda. Many years ago, when operating upon large quantities of silver mineral with a solution of hyposulphite of lime, I observed the comparative insolubility of iodide of silver in this menstruum. In order to be satisfied that the liquid which percolated through the ore still held the chloride of the precious metal in solution, a drop of iodide of potassium was added, which caused an immediate turbidity if such were the case. To refer for one moment to the former part of this paper, it will be seen that the separation of chlorine, bromine, and iodine can be effected with greater accuracy by the cautious employment of weak solutions of the iodide and bromide of potassium. Iodide of silver is very soluble in a *concentrated* solution of the potassium salt, but if the reaction be studied further, all cause for apprehension is lost. Add a few drops of strong nitrate of silver to a highly concentrated solution of iodide of potassium in a test tube; as each drop enters the liquid a bulky precipitate takes place, which immediately redissolves upon agitation. A considerable quantity can be thus added, without producing a permanent precipitate; but if the contents of the tube be thrown into 4 or 6 ounces of cold water, all the silver salt will be thrown down, and the filtrate will exhibit no blackening when tested with sulphide of ammonium. The preceding observations may be interesting, as illustrating still more fully the curious relationship of iodine, bromine, and chlorine, bromine appearing to be the connecting link between the other two elements. Independently of chlorine being a gas, iodine a solid, and bromine a liquid, and that the equivalents of the first and the last divided by two give the equivalent of bromine, the phenomena attendant upon their reactions may be traced still further.

Chloride of silver is white, bromide of silver very pale yellow, iodide of silver bright pale yellow; chloride of silver highly soluble in ammonia, bromide of silver difficultly soluble, iodide of silver nearly insoluble. Chloride of silver exceedingly soluble in hyposulphite of soda, bromide of silver far less so; iodide of silver twice as insoluble as the bromide. Chloride of silver not precipitated by the addition of chloride of sodium to its solution in the hyposulphite; bromide of silver to a certain extent; iodide of silver in very large quantity. Chloride of silver slightly soluble in *cold* concentrated chloride of potassium, bromide of silver scarcely soluble, iodide of silver perfectly insoluble; and, to conclude, iodide of silver soluble in concentrated alkaline iodides, bromide of silver to a less extent in bromides, and chloride of silver to still less extent in chlorides.

Instances, no doubt, might be multiplied in connection with the peculiarities exhibited by this interesting triad. The chloride and iodide of silver existing in the same mine, and almost in juxtaposition with the bromide, seems to be highly curious and suggestive.

[I have obtained from the mine "Delirio," at the foot of the hill of Chanarcillo, near Copiapo, in Chili, pure chloride of silver, iodide of silver, and bromide of silver, the latter in splendid crystals imbedded in carbonate of lime, through which a small vein of pure silver was running.—F. F.]

On the Polyatomic Derivatives of Ammonia, *by* A. W. Hofmann, *Ph.D., F.R.S.*

The author has been engaged for a considerable time in the investigation of the action of dibromide of ethylene upon ammonia; but, in consequence of the complexity of the reaction, and the difficulty of separating the various products, he has been induced to study the deportment of the dibromide with several of the derivatives of ammonia, which, owing to the smaller number of compounds formed, furnish more readily-accessible results.

The action of dibromide of ethylene on ammonia may give rise to no less than four series of salts, according to the different proportions of the two compounds which take part in the reaction. When two equivalents of ammonia and one of dibromide of ethylene act upon one another, the following changes may occur, producing four diatomic salts:[1]—

$$\begin{aligned}
\text{Ȼ}_2H_4Br_2 + 2\,H_3N &= [(\text{Ȼ}_2H_4)''\,H_6N_2]''Br_2\,;\\
2\,\text{Ȼ}_2H_4Br_2 + 4\,H_3N &= [(\text{Ȼ}_2H_4)''_2H_4N_2]''Br_2 + 2H_4N\,Br\,;\\
3\,\text{Ȼ}_2H_4Br_2 + 6\,H_3N &= [(\text{Ȼ}_2H_4)''_3H_2N_2]''Br_2 + 4\,H_4N\,Br\,;\\
4\,\text{Ȼ}_2H_4Br_2 + 8\,H_3N &= [(\text{Ȼ}_2H_4)''_4\quad N_2]''Br_2 + 6\,H_4N\,Br.
\end{aligned}$$

If, on the other hand, the reaction take place between equal equivalents of ammonia and dibromide of ethylene, another series of salts is produced which is derived from one molecule of ammoniacal salt:—

---

[1] In an early Number of the Chemical News there is an interesting note, to which the initials "C. L. B." are attached, concerning the purple colour observable in the liquid after the passage of sulphuretted hydrogen through a solution of perchloride of iron. When iodide of silver is dissolved in warm hyposulphite of soda the solution has a similar colour, appearing clear by transmitted, but slightly turbid by reflected light.—F. F.

[2] *Quarterly Journal of the Chemical Society,* vol. x. p. 243.

[1] $H = 1$; $\text{Ɵ} = 16$; $\text{Ȼ} = 12$, &c.

$$\begin{array}{l}
\text{Ꞓ}_2\mathrm{H_4Br_2} + \mathrm{H_3N} = [(\text{Ꞓ}_2\mathrm{H_4Br})\,\mathrm{H_3N}]\mathrm{Br};\\
2\,\text{Ꞓ}_2\mathrm{H_4Br_2} + 2\,\mathrm{H_3N} = [(\text{Ꞓ}_2\mathrm{H_4Br})_2\mathrm{H_2N}]\mathrm{Br} + \mathrm{H_4N\,Br};\\
3\,\text{Ꞓ}_2\mathrm{H_4Br_2} + 3\,\mathrm{H_3N} = [(\text{Ꞓ}_2\mathrm{H_4Br})_3\mathrm{H\,N}]\mathrm{Br} + 2\,\mathrm{H_4N\,Br};\\
4\,\text{Ꞓ}_2\mathrm{H_4Br_2} + 4\,\mathrm{H_3N} = [(\text{Ꞓ}_2\mathrm{H_4Br})_4\,\mathrm{N}]\mathrm{Br} + 3\,\mathrm{H_4N\,Br}.
\end{array}$$

This series may be considered as derived from bromide of ammonium by the substitution for one, two, three or four equivalents of hydrogen of one, two, three, or four equivalents of the radical $\text{Ꞓ}_2\mathrm{H_4Br}$, which may be called bromethyl.

When this series is produced in the presence of water, the bromine within the radical may be further replaced by the residue $\mathrm{H}\Theta$, hydrobromic acid being simultaneously eliminated, a third series of salts—viz., that of the oxethylated compounds—being thus formed, which contain

$$\begin{array}{l}
[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)\,\mathrm{H_3N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)_2\mathrm{H_2N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)_3\mathrm{H\,N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)_4\,\mathrm{N}]\mathrm{Br}.
\end{array}$$

Lastly, the bromethylated salts may be converted into vinyl-compounds by the simple loss of hydrobromic acid giving rise to the production of four additional bodies, containing

$$\begin{array}{l}
[(\text{Ꞓ}_2\mathrm{H_3})\,\mathrm{H_3N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_3})_2\mathrm{H_2N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_3})_3\mathrm{H\,N}]\mathrm{Br};\\
[(\text{Ꞓ}_2\mathrm{H_3})_4\,\mathrm{N}]\mathrm{Br}.
\end{array}$$

We thus see that no less than *sixteen* salts are, or may be, produced by the action of ammonia on dibromide of ethylene, and it is next to impossible to separate them, especially if the almost endless number of compounds intermediate between the principal classes be taken into consideration. But if the ammonia be replaced by monamines, it is obvious that in the same measure as the degree of substitution of the hydrogen in the ammonia advances, a smaller number of compounds will be obtained by the action of dibromide of ethylene; thus, a primary monamine could not give rise to the production of more than *twelve* salts; a secondary monamine, of not more than *eight*; and, lastly, a tertiary, of not more than *four* compounds.

By acting on dibromide of ethylene with triethylamine, triethylphosphine, and triethylarsine, Dr. Hofmann has, in fact, succeeded in obtaining in each series the four salts which are indicated by theory.

Diatomic Salts.

Ethylene-compounds.

Nitrogen series.

$$[(\text{Ꞓ}_2\mathrm{H_4})''(\text{Ꞓ}_2\mathrm{H_5})_6\mathrm{N_2}]''\mathrm{Br_2}.$$

Phosphorus series.

$$[(\text{Ꞓ}_2\mathrm{H_4})''(\text{Ꞓ}_2\mathrm{H_5})_6\mathrm{P_2}]''\mathrm{Br_2}.$$

Arsenic series.

$$[\text{Ꞓ}_2\mathrm{H_4})''(\text{Ꞓ}_2\mathrm{H_5})_6\mathrm{As_2}]''\mathrm{Br_2}.$$

Monatomic Salts.

Bromethyl-compounds.

Nitrogen series.

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{N}]\mathrm{Br}.$$

Phosphorus series.

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\mathrm{Br}.$$

Arsenic series.

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{As}]\mathrm{Br}.$$

Oxethyl-compounds.

Nitrogen series.

$$[(\text{Ꞓ}_2\mathrm{H_5}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{N}]\mathrm{Br}.$$

Phosphorus series.

$$[(\text{Ꞓ}_2\mathrm{H_5}\Theta)(\mathrm{C_2H_5})_3\mathrm{P}]\mathrm{Br}.$$

Arsenic series.

$$[(\text{Ꞓ}_2\mathrm{H_5}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{As}]\mathrm{Br}.$$

Vinyl-compounds.

Nitrogen series.

$$[(\text{Ꞓ}_2\mathrm{H_3})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{N}]\mathrm{Br}.$$

Phosphorus series.

$$[(\text{Ꞓ}_2\mathrm{H_3})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\mathrm{Br}.$$

Arsenic series.

$$[(\text{Ꞓ}_2\mathrm{H_3})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{As}]\mathrm{Br}.$$

In all these reactions the principal products are invariably the three bromethylated bromides,

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{N}]\mathrm{Br},$$

Bromide of bromethyl-triethylammonium,

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\mathrm{Br},$$

Bromide of bromethyl-triethylphosphonium,

$$[(\text{Ꞓ}_2\mathrm{H_4Br})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{As}]\mathrm{Br},$$

Bromide of bromethyl-triethylarsonium,

which give rise to a considerable number of interesting products.

When solutions of these compounds are mixed with silver-salts, only one-half of the bromine is precipitated in the form of bromide of silver, a salt of the bromethylated metal remaining in solution. When treated, however, with recently-precipitated oxide of silver, the whole of the bromine is eliminated and replaced by the residuo $\mathrm{H}\Theta$, and the solutions contain the oxethylated bases,

$$\left.\begin{matrix}[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{N}]\\ \mathrm{H}\end{matrix}\right\}\Theta;\quad \left.\begin{matrix}[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\\ \mathrm{H}\end{matrix}\right\}\Theta;$$

$$\left.\begin{matrix}[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{As}]\\ \mathrm{H}\end{matrix}\right\}\Theta.$$

These bases, when acted upon by acids, form three classes of salts, which may be regarded as ammonium-, phosphonium-, and arsonium-compounds, in which three equivalents of hydrogen are replaced by ethyl and one by the radical oxethyl, $\text{Ꞓ}_2\mathrm{H_4H}\Theta = \text{Ꞓ}_2\mathrm{H_5}\Theta$.

Exactly as hydrates in general may be converted into bromides and chlorides by the action of pentabromide and pentachloride of phosphorus, in the same manner the oxethylated salts may be transformed into the corresponding bromethylated and chlorethylated bodies. Thus, on submitting bromido of bromethyltriethylphosphonium to the action of pentachloride of phosphorus, the following reaction takes place:—

$$\begin{array}{c}
[(\text{Ꞓ}_2\mathrm{H_4H}\Theta)(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\mathrm{Br} + \mathrm{PCl_2Cl_3} = \mathrm{P}\Theta\mathrm{Cl_3} + \mathrm{HCl}\\
+ [(\text{Ꞓ}_2\mathrm{H_4Cl})(\text{Ꞓ}_2\mathrm{H_5})_3\mathrm{P}]\mathrm{Br}.
\end{array}$$

(To be continued.)

Note on Monohydrated Sulphuric Acid,

by Dr. Lyon Playfair.

At the meeting of the Royal Society of Edinburgh, on the 7th instant, Dr. Playfair drew attention to the researches of Marignic on sulphuric acid. This chemist always found too much water in monohydrated sulphuric acid, and fixed its specific gravity, as Beneau and others have recently done, from 1·842 to 1·845.

It is well known, indeed, that this hydrate loses anhydrous acid when distilled or boiled, and the object of the present communication is to ascertain the exact conditions under which this loss takes place, as this knowledge is of importance in a practical point of view.

The author occasionally found on distilling, and afterwards heating, oil of vitriol, that acid of the specific gravity of 1·848 was obtained, but at other times the specific gravity was as low as 1·842. To explain this difference, the following experiments were made:—

1. Sulphuric acid, having a specific gravity of 1·848, and a per-centage of anhydrids of 81·62 by the alkalimeter, was put in a retort, burned in hot sand, and distilled. The distillate had a specific gravity of 1·840, and a per-centage strength of 80·12. It had, therefore, lost by distillation 1½ per cent. of anhydrids.

2. The weak acid got by the last experiment was heated for half-an-hour to 550° F., and, after cooling, gave an acid of 1·84798 specific gravity, and strength of 81·615 anhydrids.

3. A portion of this acid now restored to its full strength and specific gravity, was violently boiled for two hours. On testing the acid on cooling, it was reduced in strength to 80·01 of anhydrids, and to a specific gravity of 1·838.

4. The weak acid obtained in the last experiment was kept for one hour at 550° F. On cooling, it had increased to 81·62 per cent. of anhydrids, and the specific gravity was 1·84792.

As a general result of these experiments, it follows that the old specific gravity of 1·848 is more correct than that given by Beneau and Marignac; that there is a true monohydrate of sulphuric acid which loses anhydride near its boiling point, but not below 550°. The latter temperature should not be exceeded in the concentration of oil of vitriol.

On the Analysis and Chemical Constitution of Cast Iron and Steel, by M. H. Caron.

The determination of carbon and silicium contained in cast iron and steel is not an easy operation, and no means have yet been discovered for the immediate analysis of these complex substances. It is, however, probable that their real nature will only be solved by comparing the results of a great number of analyses; and I shall, therefore, ask permission of the Academy to publish several of the means which I have actually tried, and then endeavour to throw light on some points which still remain obscure.

Carbon is easily separated from cast iron by means of a process long used in the laboratory of the Ecole Normale; but its inventor, M. H. St.-Claire Deville, has not, I believe, otherwise made it known.

In passing properly purified gaseous hydrochloric acid over the cast iron contained in a platinum vessel, in a red-hot porcelain tube, the carbon is isolated from all the associated substances, which will escape from it as volatile chlorides. Before employing the hydrochloric acid, it must be passed through a red-hot porcelain tube, containing wood cinders, or porous charcoal to remove oxygen. When this last precaution is neglected, a mixture of charcoal and silica is always obtained, whatever may be done to prevent it; and it is this circumstance which has led me to adopt the following process for estimating the silicium by the dry method.

In the preceding process, when the hydrochloric acid is replaced by a mixture of hydrochloric acid and atmospheric air, the silicium is obtained as silica, and remains in the vessel. This gaseous mixture, issuing from a small gasometer, passes, at the same time as the gaseous hydrochloric acid, through a small washing-bottle, containing a saturated solution of this acid, and then passes to the porcelain tube, where the analyses are effected. Perchloride of iron and carbonic acid are disengaged, and silica remains. If the cast iron contains titanium, aluminium, or calcium, the oxides or chlorides of these metals remain with the silica, from which it is easy to separate them. The theory of this operation is so simple that an explanation is superfluous. By this method I have succeeded in accurately weighing the silicium contained in cast iron and steel, and have found much more of it in them than is generally believed. The actual quantities will be more suitably brought forward in a work which I shall soon have the honour of presenting to the Acadamy. As to nitrogen it should be searched for in cast iron and steel under two distinct forms, which it appears to me have hitherto been unperceived, since M. Fremy, in his "Note," of October 8th, 1860,[1] has not mentioned them. On this point I ask permission of the Academy to discuss several of the results published by M. Fremy.

Since the experiments of MM. Wöhler and H. St.-Claire Deville, it is well known that nitrogen has a special affinity for silicium and titanium. A considerable number of cast irons contain nitrocarbide of titanium or the titanium of the blast-furnaces. I am convinced that silicium is also to be found there,—in very small proportions it is true,—in the state of nitride of silicium. This it is which the immediate analysis ought to demonstrate, and in this the chief difficulties are encountered. The nitride of titanium and the nitride of silicium are substances which oppose an energetic resistance to chemical agents; but when they are separated in that state of tenuity to which they are brought by the powerful re-agents with which it is necessary to attack cast iron, they, unfortunately are acted on with a little more facility. An indirect process must, therefore, be resorted to when they are not visible either to the naked eye or by the microscope, as is sometimes the case with nitride of titanium. These difficulties are still further augmented by the fact that cast iron, after being dissolved, leaves a little protoxide of silicium, recently discovered by M. Wöhler. Neither must it be forgotten that the odour of the disengaged hydrogen is due almost exclusively to the presence of siliciuretted hydrogen, according to the observation of this illustrious correspondent of the Academy. However, the existence of nitrogen in cast irons is not so constant as M. Fremy seems to admit, according to the works of M. R. F. Marchand, whom he cites in his "Note;" for, in accordance with the conclusions of the German chemist, it is "impossible to be certain of the existence of nitrogen in cast iron and steel."[2] And, in fact, M. Marchand himself points out how easy it is to err in similar experiments. He vindicates the numerous precautions by which M. Boussingault, in his researches on azotised bodies, avoids all causes of error. M. Marchand has not yet recognised the exactitude of Schœffhautl's observations, who admitted that the nitrogen contained in iron and cast iron concentrates itself in the carbonaceous residue obtained by dissolving these metals in hydrochloric acid.

As to the brown carbonaceous substance, soluble in potash, spoken of by M. Fremy, Berzelius was perfectly acquainted with it.[3] He compares it to ulmic acid, all the properties of which he, with some reason, attributes to it. He has not discovered nitrogen either in it or in the fetid oil of hydrogen which he considers as a carburetted hydrogen, apparently possessing the same composition as petroleum. If this brown substance disengages ammonia with soda,—which is not always the case,—it must be ascertained whether this nitrogen does not proceed from the titanium, and, above all, from the silicium, which is uniformly met with.

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[1] See *Comptes-Rendus*, t. li. p. 567.

[2] I think it is evident, after these experiments, that the existence of nitrogen in cast iron and steel should not be positively asserted. In all instances the amount of nitrogen should never be above 0·02 per cent., and in most cases, ought to be much below. If the iron contains nitrogen, it necessarily appertains to the substances mingled with the iron, which are no more integral parts of the iron than the scoria found mixed with it.—*Journal für Praktische Chemie*, v. Erdmann und Marchand, 1860, bd. xlix. p. 362.

[3] Berzelius, "Traité de Chimie," Second Edition, t. ii. pp. 697, 698.

Neither do I believe it possible to reconcile the action of sulphur, phosphorus, or arsenic, which communicate brittleness to all metals with which they are combined, by the action of carbon on the special group of metals analogous to iron. It must be admitted, it seems to me, that the cast iron hardened by charcoal differs essentially from the metals rendered gray by metalloids which indistinctly modify them.

After all, when cast iron and steel contain nitrogen, among the numerous bodies entering into their composition, which in particular fixes it? This is the query which I propose to myself. In answering it, I have been guided by the following considerations:—Pure iron and carbon do not combine directly with nitrogen at any temperature. Silicium and titanium—which latter burns in nitrogen—combine directly and very easily with nitrogen. Are not, then, the small quantities of nitrogen—M. Marchand has never found more than $\frac{2}{10000}$ths, and, for the most part, much less—combined with silicium or titanium? I believe, then, that it is to the nitride of silicium or of titanium—existing in large quantities in certain cast irons[4]—that the presence of nitrogen in the carbonaceous residuums of cast iron is to be attributed. When previously speaking of cast irons and steel, I did not include simply cemented iron in this group. This particular kind of steel may—as, in fact, Berzelius foresaw—contain paracyanogen.[5] After fusion it may be classed among the steels already mentioned.—*Comptes-Rendus*, t. li. p. 938.

On Nitro-prusside of Sodium as a Re-agent,

by A. Oppenheim, *Ph.D.*

The colour of the compound formed through the action of nitro-prusside of sodium on alkaline sulphurets depends upon the nature of the alkali employed, and upon the proportion of sulphuret and alkali in solution. Ammonia always produces a purple colour; but if soda or potash are present in great excess the colour formed is red instead of purple. Organic compounds of sulphur which are not easily decomposed by alkalies seem, therefore, to give a different reaction to that of inorganic compounds. Bisulphuret of carbon, or oil of mustard, for instance, when digested with caustic soda, at a common temperature, give not a purple but a deep red colour with nitro-prusside of sodium. When boiled with soda, these compounds form sulphuret of soda more freely, and then give the purple colour with the re-agent. Albumen may be boiled for some time before the first reaction changes into the second one. Mercaptan proves the alkaline properties of its radical by forming the red colour without the addition of soda, which when added, however, intensifies it. A solution of sulphur in an organic medium, such as benzine, or oil of turpentine, when acted upon by soda, at a common temperature, does not give the red but the purple colour—the former appearing only when soda was added in great excess.

The action of nitro-prusside of sodium on alkaline sulphurets is much stronger and more rapid than that of metallic salts. I tried to determine the quantity of an alkaline sulphuret in solution by volumetric analysis, by precipitating it with alkaline solutions of metallic oxides [of lead or of zinc], testing from time to time for the presence of the sulphuret with nitro-prusside of sodium. I found that the alkaline sulphuret was thus indicated, even in the presence of an excess of the metallic solution. For indicating the presence of alkalies, nitroprusside of sodium is superior to the best litmus paper, and for indicating alkaline earths it can be used when litmus fails to give a reaction. For the purpose of testing for these substances, sulphuretted hydrogen is passed for a short time through the solution previous to its being mixed with a few drops of the re-agent. A solution of 1 part of dry carbonate of soda in 20,000 parts of water thus assumes a colour dark enough to be diluted with 3 times its own volume of water without becoming indistinct. The reaction, however, is the slower in appearing and the quicker in disappearing in proportion as the solution is the more dilute. If, therefore, one part of carbonate of soda be dissolved in more than 40,000 parts of water, the reaction becomes indistinct. The limit of a distinct reaction for litmus paper seems to be, 1 part of carbonate of soda in 15,000 parts of water. Tincture of litmus is equally sensitive as nitro-prusside of sodium for indicating alkalies, but not for earths. One part of carbonate of lime in 20,000 parts of water containing carbonic acid is distinctly indicated through sulphuretted hydrogen and nitroprusside of sodium; whereas litmus, in such instance, indicates the carbonic acid only. Basic salts, such as the borate or phosphate of soda, give, as might be presumed, a strong reaction with nitro-prusside of sodium. Organic bases act in an analogous manner with regard to both re-agents. Those which are not indicated by litmus seem not to be capable of forming a sulphuret reacting upon nitro-prusside of sodium. Nicotine is indicated by both re-agents; quinine, cinchonia, and aniline are not. The sulphuret of nicotine, when evaporated at a common temperature over sulphuric acid, is split into sulphuretted hydrogen, which goes off, and the base which remains in solution. An alcoholic solution of aniline, when submitted to the reaction, assumes a green colour of a very transitory nature. In conclusion, it may be remarked, that nitro-prusside of sodium is capable of acting as a powerful oxidising agent. It forms brown resinous substances when acting upon volatile oils.

On the Reduction of Hydrate of Potassium to the Metallic State at the Boiling Point of Caoutchine,

by C. Greville Williams.

It is customary to attribute the difficulties encountered by Sir Humphry Davy, in his endeavours to obtain boron, silicon, &c. by means of potassium, to the fact that he operated on the oxides. It is a popular expression, therefore, to say that he had thus to overcome more powerful affinities than would have been the case had he used the haloid compounds of the elements alluded to.

The experiments of M. Caron have shown that the chloride of calcium is not decomposed by sodium alone, whereas the iodide readily yields almost the theoretical amount of metal. This fact has served to increase the tendency to too rapid generalization as to the comparative affinities of the alkali metals for oxygen and the halogens.

It is common to say that potassium is more electro-positive than sodium, and, therefore, that its affinity for oxygen is greater. I shall, however, presently relate an experiment, which, if regarded alone, and from one point of view, might be said to prove sodium to have a greater affinity for oxygen than is possessed by potassium.

The researches of Berzelius, Wöhler and Oerstedt, and the later experiments of M. Caron, have led Chemists generally to the infere :—that the following is the order of difficulty of reduction

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[4] In the Collection of the Ecole des Mines de Paris there are some specimens of cast iron so strongly impregnated with nitrocarbide of titanium, that the compound is visible to the eye.

[5] Berzelius, t. f. p. 323.

1. Oxides.
2. Chlorides.
3. Fluorides.
4. Iodides.

Again, it is usual, from the interpretation of one class of phenomena, to say that potassium is more electro-positive than any other metallic substance, accordingly it ought to decompose the oxides of all other metals, and, more especially, the oxides of comparatively electro-negative bodies like boron and silicon. If we grant this it is not correct to explain the difficulties Sir H. Davy encountered, by saying that, owing to his employing oxides, he had to overcome the highest affinities; because if silicon, boron, &c., are said to have more affinity for oxygen than for iodine, chlorine, &c., so also, on the other hand, has potassium.

The success of M. Caron's experiment, may, perhaps, be attributed less to sodium having more affinity for iodine than for chlorine, than to the circumstance of calcium having, at a red heat, a comparatively low affinity for iodine.

Further,—Sodium, in elementary works, is represented as standing next in electro-positive energy to potassium. Consequently it should decompose the oxides, chlorides, fluorides, &c., of all metals save potassium.

In these instances no allowance is made for special affinities. One or two experiments are considered sufficient to enable judgment *à priori* to be pronounced on all others. It is seldom, however, that a reaction of any one substance, though of a highly characteristic kind, can safely be taken as typical of the behaviour of all its congeners. This remark applies forcibly to organic chemistry. When the existence of homologous series began to force itself upon the attention of chemists, when instance upon instance became multiplied of homologous bodies yielding analogous reactions, an idea began to prevail of the existence of a universal law, and when exceptional reactions were observed, in many cases they were believed to arise from an insufficient fulfilment of the original conditions of experiment.

It is under the impression, therefore, of its being as useful to record apparent perturbations in laws, as to enunciate laws themselves, that I now record an observation or two, the results of which are at variance with the doctrines usually laid down in elementary works.

If, after fusing iodide of potassium in a glass tube, fragments of sodium be dropped in, no reaction becomes evident, even at a decided red heat. It is true that the sodium, after cooling, is much harder and more crystalline than before, and the globule breaks up, on the least touch, into angular fragments like starch; but no reduction takes place. This circumstance alone excites no surprise; but, taken in connection with M. Caron's mode of producing calcium, and the experiment about to be described, it becomes remarkable. If, on the other hand, we pour into a long test-tube a little caoutchine and then a few fragments of sodium and caustic potash, and boil the mixture a short time, it will be found, on cooling, that the sodium has undergone a change in its physical properties. It is still perfectly metallic, but it no longer congeals at ordinary temperatures. Moreover, large or small fragments instantly inflame in contact with cold water. The sodium has, in fact, become largely alloyed with potassium derived from the reduction of the hydrate of potassium, at a temperature not exceeding 171° C. (339·8 F.) A globule formed on one occasion, was found on analysis to contain—

Sodium . .	76·5
Potassium .	23·5
	100·0

These numbers approximate to the formula, Na_6K, which requires 22·03 per cent. of potassium. I do not, however, regard this as more than a coincidence.

From the extreme ease with which the reaction takes place, it is perhaps not too much to expect that potassium may one day be formed on the large scale by a modification of this process.

As regards the theory of the reaction, it is evident that the presence of hydrogen in the hydrate of potassium facilitates its reduction, and, consequently, that under certain circumstances, an oxide may be more readily reducible than an iodide of the same metal, and that the most difficultly reducible metallic oxide may be decomposed, and the metal obtained at a lower temperature than has, hitherto, been deemed possible.

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,

by **Frank H. Storer.**

(*Continued from page 305.*)

On the other hand, the very fact which I have myself observed, namely, that most, and probably all, alloys of copper and zinc may be crystallised in octahedrons, is in itself strong presumptive evidence that zinc is capable of assuming a similar form. Indeed, in the absence of any marked tendency in these alloys to separate by eliquation, there remains no hypothesis other than that of isomorphous mixture by which to explain their composition.

I must observe in this connection that G. Rose (*Pogg. Ann.* July, 1859, cvii. 448), has quite recently described a specimen of crystals, labelled "*Mengepresse,*"—a term applied to an alloy prepared from 1 lb. of copper from Lauterberg, and 2 lbs. of calamine,—which exists in the Royal Mineralogical Museum at Berlin, having been obtained from the collection of Klaproth. These crystals, in the opinion of Rose, appear to belong to the monometric system; but, since Professor Rose has published no analysis of them, we not only have no clue to their probable quantitative composition, but are left in doubt whether they are really a compound of copper and zinc, though the facts which I have here brought forward render the supposition an extremely probable one.

Notwithstanding this, and in spite of the fact that his alloy is only a single isolated example, Rose has urged, in direct opposition to his previous opinion, that it proves that zinc must belong to the regular system. In point of fact, however, the observation of Rose, taken by itself, evidently proves nothing of the kind. For, admitting that his crystals are really brass, he has offered no evidence to show that they do not belong to one of several possible definite alloys; only after proving that such crystals do occur through the whole series of alloys, as I have shown is really the case, and demonstrating that no definite compounds exist, could the idea of isomorphous mixture be entertained.

The crystals obtained by myself vary in size; some of them being more than half-an-inch in length, while others are quite minute. In general they are smallest in those instances where the greater part of the alloy had solidified before the crust was pierced.

In a few of the specimens of alloys, the crystals have exhibited a tendency to assume a somewhat tabular form,—a single face of an octahedron being largely developed, to the exclusion of the other faces; while in other specimens separate individual crystals have occurred. Both of these peculiarities presented themselves in the alloy of 83·72 per cent. of copper.

Very fine groups of crystals were obtained from those alloys which contained only 1 or 2 per cent. of zinc. It is worthy of note, that, although these crystals have the same form and general appearance as those prepared by the same method from an equal quantity—3 lbs. or 4 lbs.—of pure copper, they are nevertheless much larger and more perfect. Since they may, for all practical purposes, be considered as crystals of copper with slight impurity of zinc, and are easily to be obtained, it would almost seem advisable to add 1 or 2 per cent. of zinc to the metal employed in preparing specimens of crystallised copper for the cabinet. A similar case is presented by lead, which is very readily crystallised when it contains a little antimony,—a fact well exemplified by the beautiful cups of crystals of *Krätzblei*, which are prepared by partially cooling the metal in ladles, at the Frankensharner smelt-works near Clausthal, and doubtless at other localities in the Hartz.

Since the crystals rich in copper which have just been described do not possess in any marked degree the yellow colour peculiar to brass, they are somewhat less interesting than those obtained from alloys containing more zinc. Crystals of the latter can be obtained with the greatest ease by re-melting old brass, or, better, by filling a Hessian crucible from the molten metal of the pots of a brass-founder, in which case all annoyance from the formation of a false crust of mixed oxide of zinc and metal is obviated. I mention these details, because I have myself found it somewhat difficult to acquire the knack of obtaining at will good crystals, when the alloys were prepared directly from the pure metals, having frequently been compelled to repeat a single experiment three or four times before satisfactory results were obtained. This is owing to the different degrees of rapidity with which the alloys cool,—a fact to which I have already alluded.

The most perfect individual crystals were obtained from a quantity of brazier's solder which had been prepared at the foundry of the Revere Copper Company in Boston, by fusing together 50 parts of copper with 50 parts of zinc. When an alloy of about this composition solidifies, and especially if it be suddenly cooled,—as happens when it is poured into iron ingot-moulds,—it assumes, as is well known, a highly crystalline structure, consisting almost entirely of a mass of coarse fibres, which shoot out from the points at which the alloy comes in contact with the cold metal of the mould. In the instances which have fallen under my notice, the ingots being from an inch to an inch and a half in depth, the fibres have shot up three-quarters of an inch, or more, from the bottom of the ingot, leaving only a sheet of metal about a quarter of an inch in thickness on top, which had cooled more slowly by contact with the air. Indeed these fibres, although described by Calvert and R. Johnson (*Journal of the Franklin Institute* [3], xxxvii. 200. See also *Philosophical Transactions*, vol. cxlviii. p. 367) as prismatic crystals, indicating that the alloy Cu Zn is a definite chemical compound, are evidently nothing more than collections of octohedral crystals, similar to those which form the fibres of sublimed sal-ammoniac and of several metals (*Vid.* Savart, *Annales de Chimie et de Physique* [2], xli. 65).

By comparing the striations on these fibres with those upon any of the crystals of the series, it is impossible to resist the conviction that the former are mere aggregations. This conclusion is confirmed by the fact that the individual crystals which have been just mentioned as occurring above and among the fibres are evidently parts of the same system as the latter. It often happens that this alloy, known as "brazier's solder," is run into cylindrical moulds of two or three inches in diameter, pierced in earth. In this case the fibres shoot out from all sides towards the centre, leaving at the middle of the bar a sort of neutral ground in which tolerably well-formed crystals frequently occur. The solder is prepared for market by warming it slightly,[1] and then pounding it in an iron mortar till it is reduced to a somewhat coarse powder. With the fibres, this result is easily obtained, since no great effort is necessary to tear asunder the numberless little crystals of which they are composed; but the larger individual crystals which occur in the crust and core, as just described, are much more refractory; they are removed from the finer powder by sifting, and are subsequently remelted. Among these rejected "kernels" very good crystals may often be found.

The tendency to shoot out into fibres, which has been alluded to, and which deserves something more than a passing notice, extends over quite a space, from alloys containing 57 or 58 per cent. of copper, or even more, down to those containing 43 or 44 per cent., where it gradually disappears, as I have proved by casting a series of ingots. Although it does not altogether prevent one from obtaining crystals by the method of pouring off the still fluid portion of the alloy from that which has been allowed to solidify, still the crystals which I have obtained in this way within the limits of its influence are in general less perfect than those of the alloys containing more copper; indeed, on remelting the solder from which the finest separate crystals were obtained, and pouring off a portion of it after the rest had become solid, only indifferently good specimens could be procured, the alloy appearing to pass so quickly from the liquid to the solid state, that the crystals have but little time in which to form. It is remarkable that this inclination to form fibres is strongest in those alloys which contain nearly equal equivalents of zinc and copper, being less clearly marked as one recedes in either direction from this point, until a stringy texture analogous to that of copper is reached on the one hand, and the peculiar pastiness of zinc on the other. In preparing crystals, this pastiness manifests itself decidedly in the alloys immediately below those which are fibrous, becoming more strongly marked as the alloys are richer in zinc,—at least, so far as my own experiments have extended, *i. e.*, to 30 per cent. or less of copper. The fracture of these white alloys is for the most part vitreous. The pasty condition appears to depend, to a certain extent, on the manner in which the alloy is cooled, being less apparent when this process has been rapid. I have repeatedly obtained fine cups, lined with tolerable crystals, from alloys which in other trials afforded nothing but a mass of paste. The transition, however, from complete liquidity to the pasty condition, when the latter is assumed, is very rapid.

The fact that the alloys just mentioned take on the fibrous texture when cooled under ordinary circumstances has moreover a very important practical bearing; alloys at the upper limit of this fibrous tendency being the lowest—*i. e.*, richest in zinc—which can be rolled or subjected to the various processes by which metals are wrought. Singularly enough, at a point just beyond the limit at which the fibres cease to be apparent,—viz., at 60 per cent. of copper,—an alloy of peculiar homogeneity occurs; its fracture, as seen when small bars are broken, being smooth and compact, and entirely unlike either the coarse, irregular, stringy fracture of alloys richer in copper, or that of alloys containing only a little more zinc, upon the fracture of which small bundles of fine crystalline fibres are often apparent.

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[1] It must not be heated above a very moderate temperature, for, like metallic zinc at certain temperatures, it then becomes somewhat tenacious or pasty, and cannot be powdered.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

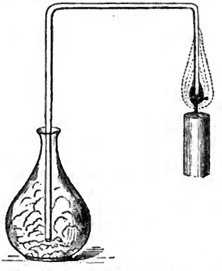
ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. FARADAY, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

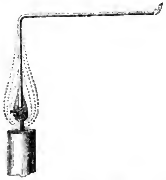
LECTURE II. (Dec. 29, 1860.)

A Candle: Brightness of the Flame—Air necessary for Combustion—Production of Water.

We were occupied the last time we met in considering the general character and arrangement as regards the fluid portion of a candle, and the way in which that fluid got into the place of combustion. You see, when we have a candle burning fairly in a regular steady atmosphere it will have a shape something like the one shown in the diagram, and looking pretty uniform, although very curious in its character. And, now, I have to ask your attention to the means by which we are able to ascertain what happens in any particular part of the flame; why it happens; what it does in happening; and where, after all, the whole candle goes to: because, as you know very well, a candle being brought before us and burned, disappears, if burned properly, without the least trace of dirt in the candlestick—and this is a very curious circumstance. Now, in order to examine this candle carefully, I have arranged certain apparatus, the use of which you will see as I go on. Here is a candle; I am about to put the end of this glass tube into the middle of it—into that part which old Hooker has represented in the diagram as being rather dark, and which you can see at any time if you will look at a candle carefully, without blowing it about. We will examine this dark part first.



Now I take this bent glass tube, and introduce one end into that part of the flame, and you see at once that something is coming from the flame, out at the other end of the tube; and if I put a flask there, and leave it for a little while you will gradually see that something from the middle part of the flame is drawn out and goes through the tube and into that flask, and there behaves very differently from what it does in the open air. It not only escapes from the end of the tube, but falls down to the bottom of the flask like a heavy substance, as indeed it is. We find that this is the wax of the candle made into a vaporous fluid—not a gas.—(You must learn the difference between a gas and a vapour: a gas remains permanent, a vapour is something that will condense.)—If you blow out a candle, you perceive a very nasty smell, consequent on the condensation of this vapour. That is very different from what you have outside the flame; and, in order to make that more clear to you, I am about to produce and set fire to a larger portion of this vapour—for what we have in the small way in a candle, to understand thoroughly, we must, as philosophers, produce in a larger way if needful, that we may examine the different parts. And, now, Mr. Anderson will give me a source of heat and I am about to show you what that vapour is. Now, here is a glass flask, and I am going to make it hot, as the inside of that candle-flame is hot, and the matter about the wick is hot. [The Lecturer placed some pieces of wax in a glass flask, and heated them over a lamp.] Now, I dare say, that is hot enough for me. You see that the wax I put in it has now become fluid, and there is a little smoke coming from it. We shall very soon have the vapour rising up. I will make it still hotter, and now we get more of it, so that I can actually pour the vapour out of the flask into that basin, and set it on fire there. This, then, is exactly the same kind of vapour as we have in the middle of the candle; and that you may see that that is the case, let us try whether we have not got here, in this flask, a real combustible vapour out of the middle of the candle.—[Taking the flask into which the tube from the candle proceeded, and introducing a lighted taper.]—See how it burns. Now this is the very vapour from the middle of the candle, produced by its own heat; and that is one of the first things you have to consider with respect to the progress of the wax in the process of combustion, and as regards the changes it undergoes. I will arrange another tube carefully in the flame, and I should not wonder if we were able, by a little care, to get that vapour to pass through the tube to the other extremity, where we will light it, and obtain absolutely the flame of the candle at a place distant from it. Now, look at that. Is not that a very pretty experiment? Talk about laying on gas—why, we can actually lay on a candle! And you see from this that there are clearly two different kinds of action—one the *production* of the vapour, and the other the *combustion* of it—both of which take place in particular parts of the candle.



I shall get no vapour from that part that is already burnt. If I raise the tube (Fig. 1) to the upper part of the flame, so soon as the vapour has been swept out, what comes away will be no longer combustible; it is already burned. How burned? Why burned thus: In the middle of the flame where the wick is, there is this combustible vapour; on the outside of the flame is the air which we shall find necessary for the burning of the candle; between the two, intense chemical action takes place whereby the air and the fuel act upon each other, and at the very same time that we obtain light the vapour inside is destroyed. If you examine where the heat of a candle is, you will find it very curiously arranged. Suppose I take this candle and hold a piece of paper close upon the flame, where is the heat of that flame? Do you not see that it is *not* in the inside? It is in a ring, exactly in the place where I told you the chemical action was; and even in my irregular mode of making the experiment, if there is not too much disturbance, there will always be a ring. This is a good experiment for you to make at home. Take a strip of paper, have the air in the room quiet, and put the piece of paper right across the middle of the flame,—(I must not talk while I make the experiment,)—and you will find that it is burnt in two places, and that it is not burnt, or very little so, in the middle; and when you have tried the experiment once or twice, so as to make it nicely, you will be very interested to see where the heat is, and to find that it is where the air and the fuel come together.

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[1] Reported *verbatim* by special permission.

This is most important for us as we proceed with our subject. Air is absolutely necessary for combustion; and what is more, I must have you understand that *fresh* air is necessary, or else we should be imperfect in our reasoning and our experiments. Here is a jar of air, I place it over a candle, and it burns very nicely in it at first, showing that what I have said about it is true; but there will soon be a change. See how the flame is drawing upwards, presently fading, and at last going out. And going out, why? Not because it wants air merely, for the jar is as full now as it was before; but it wants pure, fresh air. The jar is full of air, partly changed, partly not changed; but it does not contain sufficient of the fresh air which is necessary for the combustion of a candle. These are all points which we as young chemists have to gather up; and if we look a little more closely into this kind of action, we shall find certain steps of reasoning extremely interesting. For instance, here is the oil-lamp I showed you,—an excellent lamp for our experiments,—the old Argand lamp. I now make it like a candle [obstructing the passage of air into the centre of the flame]; there is the cotton; there is the oil rising up it; and there is the conical flame. It burns poorly because there is a partial restraint of air. I have allowed no air to get to it, save round the outside of the flame, and it does not burn well. I cannot admit more air from the outside, because the wick is large; but if, as Argand did so cleverly, I open a passage to the middle of the flame, and so let air come in there, you will see how much more beautifully it burns. If I shut the air off, look how it smokes; and why? We have now some very interesting points to study: we have the case of the combustion of a candle; we have the case of a candle being put out by the want of air; and we have now the case of imperfect combustion, and this is to us *so interesting*, that I want you to understand it as thoroughly as you do the case of a candle burning in its best possible state. I will now make a great flame, because we need the largest possible illustrations. Here is a larger wick [burning turpentine on a ball of cotton]. All these things are the same as candles, after all. If we have larger wicks we must have a larger supply of air, or we shall have less perfect combustion. Look now at this black substance going up into the atmosphere; there is a regular stream of it. I have provided means to carry off the imperfectly-burned part, lest it should annoy you. Look at the soots that fly off from the flame: see what an imperfect combustion it is, because it cannot get enough air. What, then, is happening? Why, certain things which are necessary to the combustion of a candle are absent, and very bad results are accordingly produced; but we see what happens to a candle when it is burnt in a pure and proper state of air. At the time when I showed you this charring by the ring of flame on the one side of the paper, I might have also shown you, by turning to the other side, that the burning of a candle produces the same kind of soot—charcoal, or carbon.

But, before I show that, let me explain to you, as it is quite necessary for our purpose, that, though I take a candle and give you, as the general result, its combustion in the form of a flame, we must see whether combustion is always in this shape,—when I say "shape" I mean condition,—or whether there are other conditions of flame; and there are, and they are most important to us. I think perhaps the best illustration of such a point as that, being young ones, is to give you the result of strong contrast. Here is a little gunpowder. You know that gunpowder burns with flame; we may fairly call it flame. It contains carbon and other materials, which altogether cause it to burn with a flame. And here is some pulverised iron, or iron filings. Now, I purpose burning these two things together. I have a little mortar in which I will mix them. (Before I go into these experiments, let me hope that none of you, by trying to repeat them, for fun's sake, will do any harm. These things may all be very properly used if you take care, but, without that, much mischief will be done.) Well, then, here is a little gunpowder, which I put at the bottom of that little wooden vessel, and mix the iron filings up with it, my object being to make the gunpowder set fire to the filings and burn them in the air, and thereby show the difference between substances burning with flame and not with flame. Here is the mixture, and when I set fire to it you must watch the combustion and you will see that it is of two kinds. You will see the gunpowder burning with a flame and the filings thrown up. You will see them burning too, but you will see them burning otherwise than in flame. They will each burn separately. [The Lecturer then ignited the mixture.] There is the gunpowder, which burns with a flame, and there are the filings: they burn with a different kind of combustion. You see, then, these two great distinctions; and upon these differences depend all the utility and all the beauty of flame which we use for the purpose of giving out light. When we use oil, or gas, or candle, for the purpose of illumination, their fitness all depends upon these different kinds of combustion.

There are such curious conditions of flame that it requires some sharpness and some cleverness to distinguish the kinds of combustion one from another. For instance, here is a powder which is very combustible, consisting, as you see, of separate little particles. It is called *lycopodium*, and each of these particles can produce a vapour, and produce its own flame; but to see them burn you would think it was all one flame. I will now set fire to a quantity and you will see the effect. We saw a cloud of flame, apparently in one body; but that rushing noise [referring to the sound produced by the burning] was a proof that the combustion was not a continuous or regular one. This is the lightning of the pantomimes, and a very good one too. [The experiment was twice repeated by blowing lycopodium from a glass tube through a spirit flame.] That is not a combustion like that of the filings I have been speaking of, to which I must now bring you back again.

Supposing I take a candle and examine it in that part which appears brightest to our eyes. Why, there I get these black particles, which already you have seen three or four times evolved from the flame, and which I am now about to evolve in a different way. I will take this candle and clear away the gutterage which occurs by reason of the currents of air; and if I now arrange a glass tube so as just to dip into this luminous part, as in our first experiment, only higher, you see the result. In place of having the same white vapour that you had before, you will now have a black vapour. There it goes, as black as ink. It is certainly very different from the white vapour, and when we put a light to it you will find that it does not burn, but that it puts the light out. Well, these particles, as I said before, are just the smoke of the candle; and this brings to mind that old employment which Dean Swift recommended to servants for their amusement, namely, writing on the ceiling of a room with a candle. But what is that black substance? Why, it is the same carbon which exists in the candle. How comes it out of the candle? It evidently existed in the candle, or else we should not have had it here. And now I want you to follow me in this explanation. You would hardly think that all those substances which fly about London, in the form of soots and blacks, are the very beauty and life of the flame, and which are burned in it as those iron filings were burned here. Here is a piece of wire-gauze, which will not let the flame go through it, and I think you will see, almost immediately, that when I bring it low enough to touch that part of the flame which is otherwise so bright that it quells and quenches it at once, and allows a volume of smoke to rise up.

I want you now to follow me in this point,—that whenever a substance burns, as the iron filings burnt in the flame of gunpowder, without assuming the vaporous state,—they may become liquid or they may remain solid,—they become exceedingly luminous. I have here taken three or four cases away from the candle, on purpose to illustrate this point to you; because, what I have to say is applicable to all substances, whether they burn or whether they do not burn,—that they are exceedingly bright if they retain their solid state, and that it is to this presence of solid particles in the candle that it owes its brilliancy.

Here is a platinum-wire which does not change by heat. If I heat it in this flame see how exceedingly luminous it becomes. I will make the flame dim for the purpose of giving a little light only, and yet you will see that the heat which it can give to that platinum-wire, though far less than the heat it has itself, is able to raise the platinum-wire to a far higher state of effulgence. This flame has carbon in it; but I will take one that has not carbon in it. There is a material, a kind of fuel—a vapour, or gas, which ever you like to call it—in that vessel, and it has no solid particles in it; so I take that because it is an example of flame itself burning without any solid matter whatever; and if I now put this solid substance in it, and you see what an intense heat it has, and how brightly it causes the solid body to glow. This is the pipe through which we convey this particular gas, which we call hydrogen, and which you shall know all about next time we meet. And here is a substance called oxygen, by means of which this hydrogen can burn; and although we produce, by their mixture, far greater heat than you can get by the candle, yet there is very little light. If, however, I take a solid substance, and put that into it, we get a great light. If I take a bit of lime, which is a thing which will not burn, and which will not vaporise by the heat, and because it does not vaporise remains solid, and remains heated, you will find what happens as to the glowing of it. I have a most intense heat here produced by the burning of the hydrogen in contact with the oxygen; but there is as yet very little light—not for want of heat, but for want of particles which can retain their solid state; but when I hold this piece of lime in the flame of the hydrogen as it burns in the oxygen, see how it glows! This is the glorious lime-light, which rivals the voltaic-light, and which is almost equal to the sun-light. I have here a piece of carbon or charcoal which will burn and give us light exactly in the same manner as if it were burnt as part of a candle. The heat that is in the flame of a candle decomposes the vapour of the wax and sets free the carbon particles; they rise up heated and glowing as this now glows,' and then enter into the air. But the particles when burnt never pass off from a candle in the form of carbon. They go off into the air as a perfectly invisible substance, about which we shall know hereafter.

Is it not beautiful to think that such a process is going on, and that such a dirty thing as charcoal can become so incandescent? You see it comes to this—that all bright flames contain these solid particles; all things that burn and produce solid particles, either during the time they are burning, as in the candle, or immediately after being burnt, as in the case of the gunpowder and iron-filings, all these things give us this glorious and beautiful light.

I will give you a few illustrations. Here is a piece of phosphorus, which burns with a bright flame. Very well; we may now conclude that phosphorus will produce, either at the moment that it is burning or afterwards, these solid particles. Here is the phosphorus lighted, and I cover it over with this glass for the purpose of keeping in what is produced. What is all that smoke? That smoke consists of those very particles which are produced by the combustion of the phosphorus. Here again are two substances. This is chlorate of potassa, and this is sulphuret of antimony. I shall mix these two things a little, and then they may be burnt in many ways. I shall touch them with a drop of sulphuric acid, for the purpose of giving you an illustration of chemical action, and they will instantly burn. [The Lecturer then ignited the mixture by means of sulphuric acid.] Now, from the appearance of things, you can judge whether they produce solid matter in burning. I have given you the train of reasoning which will enable you to say whether they do or do not. And what is this bright flame but the solid particles passing off?

Mr. Anderson has in the furnace a pretty hot crucible,—I am about to throw into it some zinc filings, and they will burn with a flame like gunpowder. I make this experiment because you can make it well at home. Now, I want you to say what will be the result of the combustion of this zinc. Here it is burning—burning beautifully like a candle, I may say. But what is all that smoke, and what are those little clouds of wool which will come to you if you cannot come to them, and make themselves sensible to you in the form of the old philosophic wool, as it was called? We shall have left in that crucible, also, a quantity of this woolly matter. But I will take a piece of this same zinc and make an experiment a little more closely at home, as it were. You will have here the same thing happening. Here is the piece of zinc; there [pointing to a jet of hydrogen] is the furnace, and we will set to work and try and burn the metal. It glows, you see; there is the combustion; and there is the white substance into which it burns. And so if I take that flame of hydrogen as the representative of a candle, and show you a substance like zinc burning in the flame, you will see that it was merely during the action of combustion that this substance glowed—while it was kept hot; and if I take a flame of hydrogen and put this white substance from the zinc into it, look how beautifully it glows, and just because it is a solid substance.

I will now take such a flame as I had just now and set free from it the particles of carbon. Here is some camphine, which will burn with a smoke; but if I send these particles of smoke through this pipe into the hydrogen flame you will see they will burn and become luminous, because we heat them a second time. There they are. Those are the particles of carbon re-ignited a second time. They are those particles which you can easily see by holding a piece of paper behind them, and which, whilst they are in the flame, are ignited by the heat produced, and, when so ignited, produce this brightness. When the particles are not separated you get no brightness. The flame of coal-gas owes its brightness to the separation, during combustion, of these particles of carbon, which are equally in that as in a candle. I can very quickly alter that arrangement. Here, for instance, is a bright flame of gas. Supposing I add so much air to the flame as to cause it all to burn before those particles are set free, I shall not have this brightness; and I can do that in this way:—If I place over the jet this wire-gauze cap, as you see, and then light the gas over it, it burns with a non-luminous flame, owing to its having plenty of air mixed with it before it burns; and if I raise the gauze up, you see it does not burn below. There is plenty of carbon in the gas; but, because the atmosphere can get to it and mix with it before it burns, you see how pale and blue the flame is. And if I blow upon a bright gas-flame, so as to consume all this carbon before it gets heated to the glowing point, it will also burn blue. [The Lecturer illustrated his remarks by blowing on the gas-light.] The only reason why I have not the same bright light when I thus blow upon the flame is, that the carbon meets with sufficient air to burn it before it gets separated in the flame in a free state. The difference is solely due to the solid particles not being separated before the gas is burnt.

You observe that there are certain products as the result of the combustion of a candle; and that of these products one portion may be considered as charcoal, or soot; that charcoal, when afterwards burnt, produces some other product; and it concerns us very much now to ascertain what that other product is. We showed that something was going away; and I want you now to understand how much is going up into the air; and for that purpose we will have combustion on a little larger scale. From that candle ascends heated air, and two or three experiments will show you the ascending current; but, in order to give you a notion of the quantity of matter which ascends in this way, I will make an experiment by which I shall try to imprison some of the products of this combustion. For this purpose I have here what boys call a fire-balloon; I use this fire-balloon merely as a sort of measure of the result of the combustion we are considering; and I am about to make a flame in such an easy and simple manner as shall best serve my present purpose. This plate shall be the "cup," we will so say, of the candle; this spirit shall be our fuel; and I am about to place this chimney over it, because it is better for me to do so than to let things proceed at random. Mr. Anderson will now light the fuel, and here at the top we shall get the results of the combustion. What we get at the top of that tube is exactly the same, generally speaking, as you get from the combustion of a candle; but we do not get a luminous flame here, because we use a substance which is feeble in carbon. I am about to put this balloon—not into action, because that is not my object, but to show you the effect which results from the action of those products which arise from the candle, as they arise here from the furnace. [The balloon was held over the chimney, when it immediately commenced to fill.] You see how it is disposed to ascend; but we must not let it up, because it might come in contact with those upper gas-lights, and that would be very inconvenient. [The upper gas-lights were turned out at the request of the lecturer, and the balloon was allowed to ascend.] Does not that show you what a large bulk of matter is being evolved? Now, there is going through this tube [placing a large glass tube over a candle] all the products of that candle, and you will presently see that the tube will become quite opaque. Suppose I take another candle and place it under a globe, and then put a light on the other side, just to show you what is going on. You see that the sides of the jar become cloudy and the light begins to burn feebly. It is the products, you see, which make the light so dim, and this is the same thing which makes the sides

of the jar so opaque. If you go home and take a spoon that has been in the cold air and hold it over a candle—not so as to soot it—you will find that it becomes dim just as that jar is dim. If you can get a silver dish, or something of that kind, you will make the experiment still better: and now, just to carry your thoughts forward to the time we shall next meet, let me tell you that it is *water* which causes the dimness, and when we next meet I will show you that we can make it, without difficulty, assume the form of a liquid.

[*The Third Lecture will appear in our next Number.*]

PHARMACEUTICAL SOCIETY,

Wednesday, January 2, 1861.

T. N. R. Morson, *Esq., President, in the Chair.*

The adjourned discussion on the "Process of Displacement" was resumed by the Chairman, who remarked that for the process to be successful, it was necessary that the bodies to be percolated, should be reduced to a minute state of division, so that the fluid might readily act on them. In a majority of cases, no process answered so well, and it is largely employed practically. It cannot in all cases replace maceration; but whenever it can be employed, there is no doubt it is superior.

Mr. Waugh said the great recommendation to the process was its economy. Ten gallons of spirit were put into the percolator, and ten gallons of tincture were obtained; while, in the case of a tincture prepared by maceration, a certain quantity always remained in the marc. He thought the use of a tap to control the flow very advisable, and always employed one.

Mr. Sandford objected that the use of a tap encouraged an operator to pack carelessly.

Professor Redwood said, that what was wanted was an indication of the relative advantage of the two processes in particular cases. In some, the advantages of percolation were very decided: in the case of ginger, for example, one pound of which could be perfectly exhausted with a pound of rectified spirit in about two hours. The finest powder ought, however, to be used. But in many other cases the process of percolation was altogether inapplicable, and it was very desirable that a careful set of experiments should be made, in order to determine in what cases it might be used with advantage, and in what it could not. Poppy-heads, for example, could not be percolated; and in general, he might state that where only coarsely-powdered materials could be employed, the process was disadvantageous. Nor could all finely comminuted substances be percolated. Nut-galls in fine powder form a magma, through which nothing will pass. He might mention one case in which percolation might be useful,—that is, in the preparation of liquor potassæ. Mr. Wood had found that a solution of carbonate of potash after percolating through finely-divided lime, came out completely decomposed. Tinctures, as a class, he thought, should not be percolated; but he repeated that what was wanted, was a careful classification of the cases in which the process might be employed, and in which it ought not. Soubeiran, one of the first advocates of the process, had now given it up.

Mr. Robbins remarked that the case of liquor cinchonæ was one in which percolation was particularly applicable, inasmuch as the bark in that way could be exhausted with very little water. In the preparation of the acetous extract of colchicum, also it was very useful, for the colchicum could be exhausted by a small bulk of fluid and a good deal of time saved in the subsequent evaporation. He might instance one case which illustrated the economy of the process. He had recently prepared a tincture of orris-root with half-a-pound of the powdered root and 16 ounces of spirit, and by adding water to displace the spirit, he had recovered 15½ ounces of tincture.

A Member thought the principle of displacement good, and that there were very few cases in which it could not be employed. In the case of gummy substances, all the difficulties could be overcome by mixing them with clean fine sand before placing them in the apparatus. Leaves, such as henbane and hemlock, gave a much better result when percolated than when macerated. Poppy-heads, too, when carefully dried, and powdered fine enough to pass through a sieve with thirty holes to the inch, could be very successfully percolated.

Mr. Squire said that a process to be generally useful should not require much art, and he thought that for druggists who prepared but small quantities of tinctures at a time maceration was decidedly preferable. He thought that Dr. Burton's plan of placing the materials in a bag, and suspending them at the top of the vessel deserved more consideration than it had yet received. He recommended that comparative experiments should be made with substances which might be used in fine powder.

Mr. Haselden thought that the displacement process had its advantages and disadvantages. There was no doubt that most concentrated infusions could be made best by percolation. With cascarilla and calumba it answered perfectly, but for the compound infusions of gentian and orange peel it was not applicable.

The Chairman said that when the form of apparatus necessary, and all the conditions to be observed—such as the state of division to which the substances should be reduced, and the length of digestion—were well understood, the process might be universally adopted, but not before.

A beautiful stuffed specimen of the Musk Deer, presented to the Society by Messrs. Allen and Peake, was exhibited to the meeting, and a short paper "*On Musk*," by Mr. Peake, was read.

The specimen is a full grown male, with the musk pod *in situ*. In general form and size it closely resembles the common deer, but the hair is thick and strong, and very like small porcupine quills. The habits of the musk deer are a good deal like those of the hare. It is common on the spurs of the Himalayas, at an altitude of from 10,000 to 14,000 feet, and is usually snared by the hunters, but the specimen exhibited was shot. The pursuit of the animal is extremely toilsome, and sometimes hazardous, so that there is no probability of sufficient musk being obtained to cause any great alteration in the price. The musk is found in a thin membranous sac, which lies under the outer skin of the abdomen. The sacs, or pods, usually contain from two drachms to one ounce of musk, according to the age of the animal. At one year old, the pod contains no musk; at two years old, only a yellowish milky substance is found, and it is not until the animal is three years old that sufficient musk is found for extraction. The natives, however, sometimes cut off the young pods and fill them with a mixture of the dried liver and blood of the animal, with some musk, and sell them for full-grown pods. It is difficult to prevent the natives from making additions to the best, and a correspondent of the author mentioned an instance in which musk pods given by a native prince as a valuable present, were nearly worthless from the adulteration which had been practised. The mountains on which the musk deer are found extend from the interior of India towards Thibet and Chinese Tartary, and it was probable that all the varieties of musk known as Russian, Chinese, and Nepaul, came from the same district, and was the produce of the same species, the differences in appearance depending upon the age of the animal, and the way in which the pod is dried. The produce of one year received by the correspondent of the author was 120 pods, containing from 110 to 120 ounces of musk.

A vote of thanks to the donors for their valuable present was passed unanimously.

Mr. Squire exhibited an Improved Liebig's Condenser. It consisted of a helical glass tube, instead of a straight one, which could be enclosed in the ordinary tin casing, and set up vertically.

A paper "*On the Prevention of Accidental Poisoning,*" by Mr. Julius Schweitzer, was then read.

The leading ideas in Mr. Schweitzer's paper have been already communicated to the readers of the Chemical News in a letter from Messrs. Savory and Moore (See Vol. ii. p. 116). It will only be necessary for us, therefore, to give a short *resumé*. The principal questions discussed in the course of the paper were the following:—1. What is a poison? 2. Is is possible to prevent cases of accidental poisoning, and what are those cases? 3. Can we improve the present mode of keeping poison? 4. Can we insure the proper and safe mode of using strong, active medicines after they have been confided to the hands of the public? On the first question, the author observed that poisons, as a class, do not differ from other medicines in external, physical, or chemical properties: their injurious influence on healthy animal life alone distinguishes them from everything else. But as an excessive use of even the most innocent article of daily life may prove fatal, it requires more than the simple property of doing harm to constitute a poison. These enemies to health and life are dangerous or poisonous in consequence of the small amount which has the power to deprive us of life. Easy as it may seem at first sight to separate poisons from ordinary household medicines, a closer inspection reveals considerable difficulties. From the innocent manna, to the poisonous calomel and croton-oil, such a number of intermediate drugs and preparations are met with, which, from mild, to stronger and more active, lead us so gradually from one to another, as to leave us apparently no opportunity of drawing a clear line of distinction between the manna and its poisonous followers. The only characteristic of a poison, therefore, is the small quantity or dose which will prove fatal to life. As an arbitrary zero separates heat and cold, both one and the same physical force, the author proposes a similar conventional separation of medicines based on their recognised doses, by means of which a pretty correct estimate of their comparatively poisonous nature can always be formed. The selection of a zero in the scale of poisonous medicines requires a certain amount of consideration; but the author suggested that every solid, the maximum dose of which does not exceed five grains, and every liquid, the maximum dose of which does not exceed a drachm, should be considered a poison. Leaving the criminal and intentional poisonings,—which, it was remarked, could only be prevented by the watchfulness of the chemist, and the terror of the law—the author proceeded to show that the accidental poisonings, which happened from selling a poisonous for a harmless medicine, or from a mistake in dispensing a prescription, or from the administration of an overdose by the friends or attendants of a patient might be easily prevented by keeping and sending out poisonous medicines in vessels which indicate the dangerous nature of their contents by the way in which they discharge them. As many poisonous medicines are more frequently in demand than any others in the Materia Medica, it would be found practically impossible to keep them secluded and in out-of-the-way places; the author therefore in such cases suggested the use of a dispensing bottle, which is so constructed as to limit the flow of the contents. It consists of an ordinary bottle having a removeable neck, with a closed bottom perforated with two small holes. When the stopper is removed and an attempt is made to pour from the bottle, one of these holes admits air, while the other allows a small stream of the fluid to flow out. So far from causing any appreciable delay in dispensing, the use of such bottles would probably save time, as they would assist in the accurate measurement of the liquid, and so obviate the necessity of pouring backwards and forwards from measure to bottle. The advantage of having a removable neck is that the bottle may be quickly filled and easily washed out. In like manner, a poisonous solid might be dispensed from a bottle having a tapering contracted neck which could be filled through a larger aperture in the bottom. (Specimens of such bottles as the author described were exhibited to the meeting.) On the last question the author urged the propriety of always sending out "drops" in bottles with contracted necks, and poisonous external applications in others with a rough distinctive exterior. These simple and practical contrivances, he believed, would make every poisonous ingredient and medicine its own watchful monitor of danger, without occasioning the slightest impediment to its daily use; and so, he expected, would contribute to the prevention of many a fatal mistake or life-long repented oversight.

Mr. Waugh said every one could see the advantage attending the use of the small bottles for laudanum, but he preferred a clear direction for a poison label, and feared that by permitting nurses and others to trust to the mere shape of the bottle, the mischief would be increased. The great protection in these as in other cases, was to be found in people using their common sense and keeping their wits about them. Considering how much medicine was taken, it was a standing miracle that so few accidents happened, and the only way to entirely prevent accidental poisonings, would be to get people to give up taking physic altogether, and let them die in a natural way.

Dr. Squire said, that in attempting to define what was a poison, Mr. Schweitzer had only shifted the difficulty from the word "poison" to "dose," there being no recognised maximum dose for anything, and with many things it would be a matter of option whether it should be placed in a poison bottle or not. A coloured label was the most useful protection, and if people would not take the trouble to read labels he thought they deserved to be poisoned. He believed that to trust to the shape of the bottle to indicate a poison was to lean on a broken staff that could afford no support.

Mr. Haselden said there could not be a doubt of the utility of the small bottles for laudanum, which, if sent out in that way, could never be taken in a hurry and for something else.

Mr. Schweitzer, in reply, said that one case of accidental poisoning was one too many. By the use of the proposed bottles an oversight or mistake would be an impossibility. When a dispenser, nurse, or patient took up such a bottle by mistake and attempted to pour out a poisonous dose, they would find that it did not flow, but came out drop by drop, which assuredly would remind them that they had taken the wrong bottle. All that these bottles were intended for was to remind the patient and dispenser, and furthermore, enforce time for consideration.

CHEMICAL SOCIETY, *December 20.*

Professor BRODIE, *F.R.S., President, in the Chair.*

THE Rev. R. W. Bowditch and Dr. L. Thudichum were elected Fellows.

Mr. J. H. SIMS read a paper "*On the Laws of Gas Absorption.*" Regnault had shown that several gases which within certain limits of pressure obeyed Boyle's law at one temperature, disobeyed it at another. Roscoe and Dittmar had shown that many very soluble gases, examined at the temperature 0°C. did not obey Dalton's law of absorption in proportion to pressure. The author determined to examine some of these gases in order to ascertain whether the variations from Dalton's law of pressures were persistent at different temperatures, or whether, like the variations from Boyle's law, they existed at one temperature but not at another. The solubility of sulphurous acid gas in water was ascertained at the temperature of 7°, 20°, 40°, and 50° C. From these experiments it was concluded that the deviations from the law of Dalton which sulphurous acid and water exhibit, measured under pressures from 0·03 to 2 metres of mercury, vary according to the temperature at which the absorption takes place, and that this deviation becomes less as the temperature increases, so that while below 40° the non-accordance with the law is considerable, above that temperature no variation can be perceived. The author took an opportunity of examining the vapour-tension of sulphurous acid at temperatures varying from 0° to 15°. His numbers correspond very closely with those subsequently published by Regnault. Experiments on the absorption of ammonia in water were also made, and the results obtained were found to correspond with those obtained with sulphurous acid. Roscoe and Dittmar's numbers at 0° C. were compared with numbers obtained by the author at 20°, 40°, and 100°. The variations from Dalton's law diminished with the increase of temperature, and were imperceptible at 100°. In both gases the variations at low temperatures are variations of excess, that is to say, the absorption increases more rapidly than the pressure.

Dr. BENCE JONES, F.R.S., read a paper "*On Sugar in the Urine.*" His principal conclusions were as follows:—Lehmann's process for detecting sugar in the urine by extracting the evaporated residue with absolute alcohol, and precipitating the sugar therefrom in the form of potash sugar, by means of alcoholic potash, cannot be employed when small quantities of sugar are present in large quantities of urine. The process of fermentation is stopped by the residue of the urine, by much urea, and still more decidedly by oxalate of urea. Half-a-grain of sugar in water can be detected by the alcohol produced, and may be estimated by the carbonic acid produced; but much larger quantities may be entirely overlooked in concentrated urine. In decolorising urine for examination in the polarising saccharometer some sugar is always lost. Animal charcoal removes sugar in proportion to the amount of charcoal used. This sugar may be recovered by washing with boiling water. Two-thirds of the sugar in urine may be lost by Robiquet's method of decolorising with basic acetate of lead and ammonia. Pettenkofer's test for sugar by means of cholic or glycocholic acid and sulphuric acid is the most delicate known. Two-thirds of a milligramme may be detected in a little distilled water, and the presence of a small amount of urinary colouring-matter does not affect the reaction. Trommer's test with sulphate of copper and potash is capable of discovering $\frac{1}{20}$th of a per cent. of sugar in urine, but when very small quantities of sugar are in solution with muriate of ammonia or urea, the reduction of the oxide is not perceived. Brücke's alcohol process was not found to be satisfactory, but his lead process furnished excellent results. The urine is precipitated first with neutral acetate of lead, then with basic acetate of lead, and lastly with ammonia. The ammoniacal precipitate contains the sugar which is extracted by treating the precipitate with oxalic acid, or preferably by sulphuretted hydrogen. By Brücke's process one-seventh of a grain of sugar added to 200 cubic centimètres of urine could be detected, and two-thirds of all the sugar added could be recovered. Moreover, the sugar is obtained free from salts, so that it can be fermented, and free from colour so that it may be examined by the saccharometer. The presence of sugar could be readily ascertained by this process in 1000 cubic centimètres of urine. The sugar separated by Brücke's process from 1000 cubic centimètres of the urine of a healthy man was estimated by the reduction test to vary from 1·4 to 2·2 grains, and in that of another man to vary from 2·3 to 3·0 grains. The sugar separated from 5000 cubic centimètres of the urine of one healthy man gave from 7 to 8 degrees of rotation in the saccharometer, and that of another healthy man from 10 to 11 degrees. The sugar extracted from 14·000 cubic centimètres of healthy urine yielded by fermentation 1·8 grains of carbonic acid, together with a recognisable quantity of alcohol. These and other experiments fully confirmed Brücke's statement as to the habitual presence of sugar in healthy urine. Hence diabetes must be regarded as an exaggeration of a healthy state, and not as a distinct and peculiar condition of the system.

Dr. OPPENHEIM read a paper "*On the Separation of Tellurium from Selenium and Sulphur,*"—(this will be given next week)—also, a paper "*On Nitro-prusside of Sodium as a Re-agent*"—(see page 21).

NOTICES OF BOOKS.

The Boy's Playbook of Science. Second Edition. By J. H. PEPPER. *The Playbook of Metals.* Same Author. London: Routledge, Warne, and Routledge, Farringdon Street. 1860.

PERHAPS there is no public scientific lecturer whose name is so familiar to the present and rising generations as that of the author of the above-named works. His presence at the Polytechnic Institution, where most of us have had the opportunity of appreciating the animated style of his delivery, and the cleverness with which he selected the most striking experiments to illustrate his subject, was a tower of strength to that establishment, which, by the way, deserves a far greater amount of patronage than it receives. By him some thousands of the rising generation have been first made practically acquainted with the nature of an electrical shock, and initiated into the effects which a magic lantern on a large scale is capable of producing. One of the first among the hundreds of engravings contained in the "Boy's Playbook of Science," is an amusing representation of the *modus operandi* by means of which certain of the accessories were produced. On one side of the screen sits the audience, on the other we see a man actively engaged in striking two sheets of metal together; another is hammering a big drum, and a little imp is tumbling head-over-heels in his eagerness to overtake a cannon-ball which is rolling rapidly towards the screen. In the same open and undisguised manner many other of the experiments which have astonished and delighted thousands of youngsters, are revealed, so that they are taught how to produce like effects. It would, however, convey a false idea of the contents of this book, if we were to allow it to be inferred that it treats merely of tricks and illusions; on the contrary, it is full of information on scientific subjects of every kind. The style in which they are treated is as free from technical terms as is consistent with a clear exposition of the matter under consideration; and even the most abstruse of them are dealt with in a way to bring them within an ordinary comprehension. The illustrations employed are simple and accurate, and the anecdotes which are introduced are well chosen. In writing of sound, he mentions a very curious fact which, in these days of "mediums," is not without interest. He says that there was in Paris a woman who could produce musical sounds by means of the muscles and tendons of the thigh, which could be heard distinctly from one end of the room to the other. That portion of the book which treats of steam, and the construction of steam-engines, is especially good; and he seems to have been anxious to do justice to all concerned in the application of this power; but we cannot help regretting that in the list of names, which commences with that of Hero of Alexandria, who flourished, as the term is, about 100 years B.C., he has omitted that of Papin, who, in 1707, launched his vessel, moved by steam, on the river Fulda, in the presence of the Landgrave of Hesse-Cassel, and shortly after set out on a voyage to London in it, but was stopped by the watermen on one of the German rivers, and himself and his vessel seized. What became of the latter it is impossible to say, it is only known that he could not recover it. All his hopes seemed to be set on reaching the port of London in his steamer, and, having been disappointed in this, he did not live long after his arrival in this country.[1]

In reference to the application of scientific knowledge to the affairs of common life he mentions two instances, which are too interesting to be passed over in silence. It will be remembered that during the war in the Crimea it was of no unfrequent occurrence for artillerymen to be shot while working the guns in the batteries, from their eagerness to see the effect of the shell or shot they had just sped in the direction of Sebastopol. All remonstrances were useless; they cared less for exposing their lives to the fire of the Russian riflemen than for seeing the damage they caused the enemy's works. In order that they might gratify this propensity without risk, the Rev. W. Taylor invented a reflecting spy-glass, which enabled them to see this without raising their heads above the parapet of the battery. The arrangement of the mirrors is fully explained and illustrated in the work before us. The same gentleman also invented another form of glass for the purpose of enabling the gunner to aim his gun in safety; and the value of the invention was so clearly demonstrated to Lord Panmure that he ordered several of them to be constructed, but, in consequence of the cessation of the war, there was no opportunity of using them. The other instance we shall mention refers to the discovery, by means of a microscope, of the locality where a box was emptied of the gold it contained, and its place supplied with sand. On the robbery being made known, Professor Ehrenberg, of Berlin, requested that specimens of the sand, at every station along the railway over which the box had travelled, should be forwarded to him. These he examined under the microscope, and compared with that contained in the box, and so arrived at the discovery of the station where the substitution had been effected.

So perfect is this book in other respects, that we regret to have to mention inaccuracies in the spelling of names, and in one instance a total change of name, which ought, certainly, not to have appeared in a second edition. Thus, Canson is spelt Cansan, Allan is spelt Allen, and E. J. Lowe is metamorphosed into E. S. Lane.

The title of a book is supposed to convey some idea of its contents; therefore when we read "The Playbook of Metals," we expect to find a work giving a little information as to the nature of the metals, and a great deal respecting the more striking instances of their application to engineering purposes, and so forth; but we certainly do not expect to find it to be a book so complete in all its parts, as that which presents itself to notice under this title. Commencing with the mineral, coal, it gives an account of its formation, the districts in which it is found, and the mode of working it. The appearances presented by the coal-bearing strata, are carefully and clearly shown by numerous diagrams; and the mode of boring for its discovery is fully explained; nor does the author fail to describe the principal fossils which have been, and are still, commonly found in the beds of this mineral. As he has himself descended into a large coal-mine, and travelled through it, he is able to give us his own experience with respect to the manner in which the operations are carried on, and the circumstances under which the hardy miner performs his part in the battle of life. With the Lundhill colliery explosion, by which one hundred and ninety persons lost their lives, and the still more recent Risca colliery explosion, which destroyed one hundred and forty-two individuals, fresh in our recollection, we should have been glad if the author had turned his scientific and mechanical knowledge to account in suggesting a light which would be more effectual in the prevention of such accidents. The Davy lamp is objected to by the miners on the ground of its giving an insufficient light; and though the improved Davy lamp constructed by Simons, is not open to that objection to the same extent, yet we cannot agree with Mr. Pepper, that the miner cannot remove the wire gauze without throwing down the extinguisher over the flame. One of these lamps was shown to us when first invented, and this latter safeguard was especially dwelt upon; but it was not difficult to see that a reckless miner—of whom there are only too many—would have no great difficulty in preventing the extinguisher from acting while he removed the gauze to light his pipe, without the overseer of the mine being likely to discover it.

After having disposed of the fuel so essential in the preparation of metals, the writer proceeds to give an account of the early workers in these substances. The principal alchemists of whom records exist are noticed in succession, and some very curious extracts from their works are given, the jargon in which they are written being very ingeniously interpreted. The means by which they performed some of their most striking experiments, the reading of which still astonishes many persons who think that, after all, these impostors must have known something which we modern chemists do not know, are explained in the simplest manner; and, on the whole, very fair justice is meted out to these worthies of the olden time. The extent to which metallurgists are indebted to their labours for information on the subject of smelting ores, and for designs of vessels, such as crucibles and so forth, is clearly shown in the quaint engravings copied from ancient MSS., and in the very complete summary which accompanies them. It is astonishing to find how many of the utensils used in mining ages ago are still retained, almost without alteration.

The method of separating gold from quartz and other substances with which it is associated, as well as of silver and the other metals, is described in a clear and popular style, as are also the various modes of performing an assay of the respective ores; and the information on this, as well as on other points, is brought down to the present date.

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[1] Writing to Leibnitz from Cassel, before starting on his voyage, he said:—"Il est important que ma nouvelle construction de bateau soit mise a l'épreuve dans un port de mer comme Londrès, ou on pourra luy donner assez de profondeur pour appliquer la nouvelle invention qui, par le moyen du feu, rendra un ou deux hommes capable de faire plus d'effet que plusieurs centaines de rameurs."

There are many cautions scattered through the book, which though for the greater part unnecessary to experienced metallurgists and chemists, are yet very properly given when it is considered that it is professedly written for beginners, and among these is one founded on an example which may not be without its use to some among our readers. "Gold is attacked by mercury, and dissolved in it with considerable rapidity, and the author remembers a case of vanity reproved in which the mercury contained in a mercurial trough used for certain experiments with gases that are soluble in water, formed the corrective agent. A young gentleman who had made himself conspicuous as a pupil in a chemical class, by wearing a very showy collection of jewellery on his person, was induced by the brilliancy and pretty appearance of the quicksilver in the open trough, to stir it about with his fingers, on one of which was a diamond ring; no one made any remark, but at the end of the discourse a sudden cry was heard, 'where's my ring?' The *debris* of this precious ornament remained floating and dissolving in its mercurial grave. The diamond alone remained perfect, and this, if the author remembers properly, the other mischievous pupils recommended should be heated red-hot in a crucible with some nitre, and was either burnt away or lost." The moral of this anecdote is that pupils in chemistry should either abstain from jewellery altogether, or put it in their pockets before entering the laboratory.

In concluding our notice of "The Playbook of Metals," we cannot refrain from complimenting the author on the extent and variety of the knowledge he has displayed in writing it, and on his industry in searching through so many old books for valuable and curious information. The inaccuracies in the spelling of names which we noticed in the first-mentioned book do not occur in this, except in one instance, evidently due to the reader, where the name of a historical personage is so entirely changed that it would be impossible to recognise for whom it was intended but for the circumstance with which it is associated. The engravings with which it is illustrated are 300 in number, it is stated, and these are good and highly interesting, with a few exceptions which might as well have been omitted.

CORRESPONDENCE.

Homogenesis of Forces.

To the Editor of the Chemical News.

Sir,—Your correspondent, "E. M. T." asks if steel does become magnetic when placed in a particular part of the solar spectrum? I would refer him to any good book on Magnetism, where he will find an account of Mrs. Somerville's experiments.

"E. M. T." seems to think that I am the originator of the theory that all the phenomena of nature are produced by gravitation, motion, light, heat, electricity, and chemical affinity, and that these six may be produced by one of them, which in its turn cannot be produced; but this is quite a mistake: the sole object of my letter being to endeavour to prove, that "*if* the causation of all the phenomena of the universe be referred to one force," (of the six above mentioned) "it must be to gravitation, not to matter in motion;" therefore, if he has anything to say for or against the theory, he must say it to the propounder of it, not to me.—I am, &c.

George F. Rodwell.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Preparation of Subnitrate of Bismuth.—MM. Bechamp and C. St. Pierre recommend the following method of making subnitrate of bismuth (*Montpellier Medical,* April, 1860). They first make a neutral nitrate, by pouring nitric acid on powdered bismuth. The crystals of this neutral salt are drained, and then washed with a mixture of 1 part of acid and 3 of water. They are then dissolved in as small a quantity as possible; and the experiments of the authors have shown that a cubic centimètre of water, at 80°, containing 10 per cent. of nitric acid, is sufficient to dissolve 1 gramme of the salt. 12½ parts of water are now sufficient to precipitate a part of the crystallised nitrate so dissolved. The reaction is represented by the following formula:—

$$3(Bi_2O_3,3NO_5,10HO)+Aq = Bi_2O_3 3NO_5,2(Bi_2O_3,3HO)$$
$$+6NO_5+24HO+Aq.$$

10 grammes of the crystallised nitrate should yield 6·3 grammes of the subnitrate, but only two-thirds of this quantity are obtained. The rest of the bismuth, which remains in the mother liquors in the state of subnitrate, and not as oxide, may be precipitated by carbonate of ammonia. The salt precipitated by water gives, on analysis, 16·98 of nitric acid, and 76·8 of oxide of bismuth. That precipitated by ammonia contains 80 per cent. of oxide of bismuth. The two products differ slightly. The second, when too much ammonia has not been used, has an evident crystalline texture, a faintly acid taste, and reddens litmus. The first is crystallised, and is slightly soluble in water; it reddens litmus strongly and has a decided acid taste. The authors add, that all subnitrate of bismuth should be rejected which is in the form of an amorphous powder, is without taste, and does not act on litmus. M. St. Pierre recommends that, to free bismuth from arsenic, from 2·5 to 5 per cent. zinc should be added, and the whole heated strongly for an hour, a piece of charcoal being placed in the crucible to prevent the oxidation of the zinc. After this treatment the bismuth will contain neither arsenic nor zinc.

New Use for Persulphate of Iron.—M. Bacco employs persulphate of iron instead of nitric acid in Bunsen's battery, and so, it is said, has a constant power and no disagreeable smell. (*Repertoire de Chimie pure et Appliquee,* liv. xl p. 354, 1860.)

Reactions of Soda and Potash with Nitric and Hydrochloric Acids.—The chlorides of the alkalies, according to Von Baumhauer, are more easily changed into nitrates than the nitrates into chlorides. The reaction of equivalents of hydrochloric acid and nitrate of potash are explained by the following formula:—

$$5KNO_6+5HCl = 4KNO_6+KCl+NO_6H+4HCl, \text{ or}$$
$$4KNO_6+4HCl = 3KNO_6+KCl+NO_6H+3HCl.$$

The reactions of equivalents of hydrochloric acid and nitrate of soda are as follows:—

$$3NaNO_6+3HCl = 2NaNO_6+NaCl+NO_6H+2HCl.$$

The residue of the decomposition is the same when either equivalents of hydrochloric acid and nitrate of soda and potash, or of nitric acid and chloride of potassium and sodium, are taken. The same results happen if a mixture of hydrochloric and nitric acids is made to act on equivalents of potash and soda. When one equivalent of nitric acid acts on several equivalents of chloride of potassium and sodium, the results obtained are the same: 5 equivalents of nitric acid give 4 equivalents of nitrate of potash, and 3 equivalents of nitric acid give 2 of nitrate of soda. In a mixture of chloride of potassium and sodium the former is first decomposed by nitric acid, and the latter is not acted upon until after the total decomposition of the potash salt. (*Repertoire de Chimie pure et Appliquée,* liv. xi p. 355, 1860.) [The entire paper will be found in the "Verslazen en Mededulingen der Koninselzke Akademio von Waten-Schappen," vol. x. p. 26.]

Preservation of Chloride of Lime.—After quoting Dr. Hofmann's account of the spontaneous decomposition of chloride of lime from the Chemical News, vol. ii. p. 243, M. Barreswil states (*Répertoire de Chimie pure et Appliquee*, liv. ii. p. 350) that he has found the chances of decomposition much lessened when the chloride has been submitted to great pressure. So treated, it becomes as compact and hard as a stone, and may be kept a long time, perhaps, without undergoing any change. The plan might be worth trying when the chloride is exported. When required for use it would be necessary to reduce it to powder again, but the author thinks the cost of this extra labour would be compensated for by the diminished cost of packing.

II. ORGANIC CHEMISTRY.

Crystals of Proteine in the Potato.—Cohn has observed (*Journal für Praktische Chemie*, bd. lxxx. s. 129) that the cortical cellules lying immediately under the corky envelope of the potato contained only a few starch granules, but have as well some transparent cubic crystals, which exhibit the same reactions as crystals of proteine compounds. The cellules seldom contain more than one crystal, which is in general attached to the primordial utricle, and is, more or less, enveloped by the liquid protoplasm. Dilute potash dissolves the crystals, while dilute acids transform them into globules, which appear to be coagulated. Boiling water does not alter their appearance, but coagulates them, and renders them insoluble in ammonia and acetic acid.

LABORATORY MEMORANDA.

Action of Iodine on Turpentine.—I do not find any mention of the consequence of adding iodine to turpentine in chemical works. If it has not been the subject of observation, I think that it might be worthy of the same. After the lapse of a few seconds from the time of adding a piece of iodine to turpentine, a most violent bubbling commences, developing heat sufficient to inflame the mixture, except cooled artificially, co-existent with the development of heat is a disengagement of white gaseous vapour; the result is, a dark-brown turbid liquid, still possessing the property of changing the colour of starched paper blue. After the mixture has been allowed to rest for a week or more, the liquid becomes perfectly clear, having deposited a dark-brown resinoid body capable of being melted by heat, but not inflaming readily, not soluble in a fresh quantity of turpentine, neither does it act upon starch as the solution still does. This solution produces a change in a soap liniment not observable when a mere solution of iodine is added, causing a beautiful white emulsion. The exact nature of the chemical change will probably be the formation of hydriodic acid, which dissolves in the spirit, together with the disengagement of carbonic acid, or, on being more fully investigated, the decomposition may be more complex.—James Dearden.

On a New Form of Iodide of Potassium.—Although iodide of potassium when pure will endure a high temperature without undergoing decomposition, I find that when intimately mixed with about equal parts of ether, magnesia, silicic acid, baryta or fused boracic acid, and raised to a red heat in a crucible, copious fumes of iodine are given off, and that, if the contents of the crucible be afterwards dissolved in water and filtered (which is easy in the case of the magnesia or silicic acid), the liquid will contain an iodide of potassium having properties different from that of the ordinary iodide. The heat, I may as well mention, should be continued as long as any scent of iodine is given off, otherwise the decomposition will not be complete. This iodide differs chiefly from the ordinary iodide in the amount of precipitate produced by acetate of lead being less owing to the smaller quantity of iodine contained in it; also in its being less easily crystallised. Bromide of potassium appears to undergo a similar decomposition under similar circumstances, as, when mixed with the above-mentioned substances, and subjected to heat, fumes of bromine are distinctly visible; but I have not as yet either separated the salt or examined it.—Bernard Piffard.

Chemical Society.—The next meeting of this Society will take place on Thursday, January 17, at 8 p.m., when there will be papers read "On the Analysis of the Saline Water of Purton, near Swindon, North Wilts," by Dr. Noad, F.R.S., and "On the Electrolytic Test for Arsenic," by Professor Bloxam.

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

⁂ All *Editorial Communications* are to be addressed to Mr. Crookes and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

J. Wright.—Dragon's Blood is a gum which can be obtained at any large drug warehouse in London. You might perhaps experience difficulty in obtaining it in the country. We do not know its lowest market price at present, a short time ago it fetched from £5 to £14 per cwt. according to quality.

A Druggist.—(1), (2), (3), in our next. (4). Berthelot's "Chimie Organique," reviewed in our last. The number can be obtained of Mr. Nutt, 270, Strand. It is in two large volumes and costs £1. (5). The discount varies with every house you go to. Oertling, of Store Street, Bedford Square, is the best person to apply to for accurate weights. Either grains or grammes may be used. We prefer the former. The reduction of one into the other is easily effected.

J. C. S.—(1). Salicylate of oxide of methyl is to be met with in the crude state in commerce under the name of Wintergreen oil (oil of *Gaultheria procumbens*). It may be obtained in the pure state by rectification, collecting that portion which distils at 435° Fahr.

F. M.—To clean iron, immerse the plate in dilute sulphuric acid for four or five minutes, then well scour with bran and water; transfer to a mixture of dilute sulphuric and hydrochloric acids, clean again with bran, and transfer to lime-water until required for use. This is the plan adopted by the tin plate makers.

Crispin.—For several good receipts for making blacking, both liquid and paste, see Beasley's "Druggists' Receipt-book," 4th Edition, Pp. 314, 315. London: J. Churchill.

L. T. writes to ask if it is possible for a persom of two or three-and-twenty, by a change of country or diet, to increase in stature. The only increase would be in breadth.

A. B. P.—We have scarcely yet decided whether to report them or not.

Books Received.—"London Medical Practice; its Sins and Shortcomings." London: Simpkin, Marshall and Co. "On Coal Gas," by the Rev. W. R. Bowditch. London: Van Voorst. Braithwaite's "Half-yearly Retrospect of Medicine." London: Simpkin, Marshall and Co.

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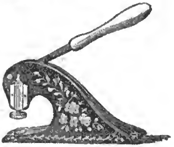

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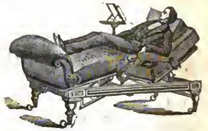

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THE CHEMICAL NEWS

VOL. III. No. 59.—January 19, 1861.

THE ADULTERATION OF FOOD ACT.

In another page will be found the Report of Dr. Letheby with respect to the working of the Adulteration of Food Act in the City. The number of applications which he has received, in his capacity of Public Analyst, to analyse articles of food has been only four, and of these four articles three were adulterated,—one specimen of bread being particularly bad, containing at most only 50 per cent. of wheaten flour. We are not told that any action was taken on his certificate, and the offending baker may still be manufacturing bread of a like quality. The only other sample of bread analysed is certified by the Doctor to be of pure wheaten flour. In both cases the names of the bakers are given. Now, we have no sort of objection to this, so far as regards the fraudulent one; but we would suggest that this practice is not advisable in the case of the supposed honest one, in the present state of the law, since it gives facilities to the rogue to obtain a certificate from an eminent analyst, to the effect that the goods he vends are genuine, whereas they may be the veriest trash in reality. All that is necessary to enable a man to obtain such a certificate is to place half-a-sovereign in a friend's hand, to request him to purchase a loaf, or a pound of coffee, or anything else, at his shop, and take it to a Public Analyst to be analysed. Of course this specimen will be genuine, and the party, at the small cost of half-a-guinea, will have the satisfaction of seeing his name and address published in all, or most of the newspapers, as a vendor of genuine and unadulterated articles of food. Some may urge that it is in the interest of the public, and only just to the honest trader that his name should be published, if it were only to encourage others. To the latter argument we have only to reply, that we do not see the necessity for rewarding a man simply because he is not a rascal, every man being expected to do his duty as a matter of course. As regards the public, what we have already said with respect to the manner in which such a certificate can be obtained settles their interest in the matter. Of course, supposing the sample to be bought by a trusty official, it would be a very different matter. A certificate of genuineness would, in that case, be a voucher to be relied upon, and it would be a benefit to the public to make known the place where bread could be purchased which was not a compound of bad wheaten flour, rice, and alum, combined with 44 per cent. of water. To publish the names and addresses of tradesmen, however, of whom the analyst knows nothing beyond the fact that a person brings him samples of food bought at their respective shops, and which he finds to be genuine, can only result in the public being misled, if they do not think of the manner in which it was brought about, which the generality are pretty certain not to do.

We have no sort of doubt that whatever the Act is capable of accomplishing Dr. Letheby will make it accomplish, but this will be very little indeed. The more conscientiously he labours, the more conspicuous will its weakness and inefficiency become. Timorous in its character, it betrays a fear of the trading-classes which we firmly believe to be unnecessary; for we do not think that anything like a majority of the shopkeepers sell adulterated goods, and only those who do would object to an Act which would really protect the public, and, at the same time, be beneficial to all honest tradesmen. We pointed out many of its weak points when it was in its embryo state, but it was said that there was no hope of getting anything stronger through the present House of Commons. We trust, however, that an attempt will be made during the next Session to put some vitality into it,—an attempt which would have the better chance of success now that Dr. Letheby has demonstrated that the Revenue is robbed, by adulteration in one article alone, to the extent of about £100,000 annually.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Polyatomic Derivatives of Ammonia,
by A. W. HOFMANN, *Ph.D., F.R.S.*

(Continued from page 19.)

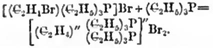
That the radical Ꞓ_2H_4Br may, with justice, be considered as monobrominated ethyl is fully proved by the two following facts:—First, when triethylphosphine is submitted to the action of brominated bromide of ethyl obtained by the action of bromine at a high temperature on bromide of ethyl, a brominated compound, absolutely identical with the one produced by the action of dibromide of ethylene on triethylphosphine, is formed. Secondly, when the bromethylated bromide is submitted to the action of hydrogen in the nascent state, the bromide of triethylphosphonium is obtained with all its properties,—

$$[(\text{Ꞓ}_2H_4Br)(\text{Ꞓ}_2H_5)_3P]Br + H_2 = HBr + [(\text{Ꞓ}_2H_5)_4P]Br.$$

The following series is thus established:—

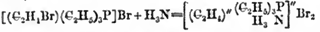
Bromide of tetrethylphosphonium,
$[(\text{Ꞓ}_2H_4H)(\text{Ꞓ}_2H_5)_3P]Br$;
Bromide of chlorethyl-triethylphosphonium,
$[(\text{Ꞓ}_2H_4Cl)(\text{Ꞓ}_2H_5)_3P]Br$;
Bromide of bromethyl-triethylphosphonium,
$[(\text{Ꞓ}_2H_4Br)(\text{Ꞓ}_2H_5)_3P]Br$;
Bromide of oxethyl-triethylphosphonium,
$[\text{Ꞓ}_2H_4H\Theta)(\text{Ꞓ}_2H_5)_3P]Br$.

When the bromide of bromethyl-triethylphosphonium is acted upon by triethylphosphine, the two substances combine, and give rise to the formation of the same diphosphonium salt which is produced directly by the action of dibromide of ethylene on triethylphosphine,—



$$[(C_2H_1Br)(C_2H_5)_3P]Br + (C_2H_5)_3P = \left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C_2H_5)_3P\end{matrix}\right]''Br_2.$$

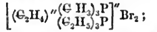
If ammonia be substituted for triethylphosphine, dibromide of ethylene-triethylphosphammonium is produced,—



$$[(C_2H_1Br)(C_2H_5)_3P]Br + H_3N = \left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\H_3\ N\end{matrix}\right]''Br_2$$

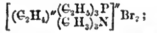
Again, the ammonia may be replaced by any primary, secondary, or tertiary monamine, when other more or less complicated bodies are obtained. Instead of monamines, monarsines may be employed; in this case, phospharsonium-compounds, containing at the same time both phosphorus and arsenic, are formed. Amongst others, the author has studied the following compounds:—

Dibromide of ethylene-trimethyl-triethyl-diphosphonium,



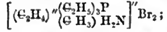
$$\left[(C_2H_4)''\begin{matrix}(C\ H_3)_3P\\(C_2H_5)_3P\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-trimethyl-thriethyl-phosphammonium,



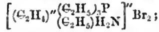
$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C\ H_3)_3N\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-methyl-triethyl-phosphammonium,



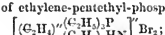
$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C\ H_3)\ H_2N\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-tetrethyl-phosphammonium,



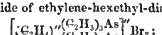
$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C_2H_5)H_2N\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-pentethyl-phosphammonium,



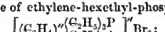
$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C_2H_5)_2HN\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-hexethyl-diarsonium,



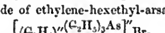
$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\(C_2H_5)_3As\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-hexethyl-phospharsonium,



$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3P\\(C_2H_5)_3As\end{matrix}\right]''Br_2;$$

Dibromide of ethylene-hexethyl-arsammonium,



$$\left[(C_2H_4)''\begin{matrix}(C_2H_5)_3As\\(C_2H_5)_3N\end{matrix}\right]''Br_2.$$

What has been said with regard to the bromethylated bromide of the phosphorus series, applies with equal force to the corresponding salts in the arsenic and nitrogen groups.

A monatomic ammonium-salt may therefore be converted into a diatomic salt by introducing into the former a chlorinated or brominated radical, the chlorine or bromine of which presents a point of attack for another molecule of ammonia, the fixation of the ammonia giving rise to the formation of a diammonium-compound. On this principle it is evident that by the introduction of several molecules of chlorine or bromine into the monammonium, several equivalents of ammonia may be associated to form a very extended series of polyammonium compounds.

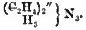
The introduction of chlorine and bromine may be conceived to take place in two different ways. In the first place, one equivalent of hydrogen in the ammonium salt may be replaced by organic radicals,—ethyl, for instance, in which two, three, four, or five equivalents of bromine are substituted for the hydrogen. On submitting these bodies to the action of ammonia, two, three, four, or five equivalents of the latter may be fixed, di-, tri-, tetra-, penta-, and hex-ammonium salts being formed in this manner.

$$\begin{array}{l}[(C_2H_3Br_2)'H_3N]'Br + 2H_3N = [(C_2H_3)'''\ H_9\ N_3]'''\ Br_3,\\ [(C_2H_2Br_3)'H_3N]'Br + 3H_3N = [(C_2H_2)''''H_{12}N_4]''''\ Br_4,\\ [(C_2H\ Br_4)'H_3N]'Br + 4H_3N = [(C_2H)'''''H_{15}N_5]'''''Br_5,\\ [(C_2\quad Br_5)'H_3N]'Br + 5H_3N = [(C_2)''''''\ H_{18}N_6]''''''Br_6.\end{array}$$

Again, the accumulation of the bromine equivalents might be accomplished in a different manner. Several of the equivalents of hydrogen in the monammonium salt might be replaced by a monobrominated organic radical,—monobrominated ethyl for instance; under the influence of ammonia, these compounds also would be transformed into a series of polyatomic salts.

$$\begin{array}{l}[(C_2H_4Br)_2H_2N]Br + 2H_3N = [(C_2H_4)_2''H_9\ N_3]'''\ Br_3,\\ [(C_2H_4Br)_3H\ N]Br + 3H_3N = [(C_2H_4)_3''H_{10}N_4]''''\ Br_4,\\ [(C_2H_4Br)_4\quad N]Br + 4H_3N = [(C_2H_4)_4''H_{12}N_5]'''''Br_5.\end{array}$$

The author has already obtained a series of triammonium-compounds, of which the most interesting is diethylenetriamine.



$$\left.\begin{matrix}(C_2H_4)_2''\\H_5\end{matrix}\right\}N_3.$$

On the Separation of Tellurium from Selenium and Sulphur, by A. Oppenheim, *Ph.D.*

No difficulty occurs in separating tellurium from sulphur. In fact, although they are frequently, in nature, found together, combined with bismuth, they are not isomorphous, and, although there is a simple relation between their combining numbers, there are but few analogies between their compounds, none of which are found to crystallise in the same system. On the other hand, certain compounds of tellurium with chlorine, iodine, and bromine are quite similar to the analogous compounds of antimony; both tellurium and antimony crystallise in the same system, and antimony precipitates tellurium from a solution of tellurous acid in hydrochloric acid, while tellurium again precipitates antimony from its perchloride. The combinations of tellurium with antimony and with arsenic have the appearance of alloys, and differ in their properties entirely from sulphurets. The diversity of tellurium and antimony is also demonstrated by the existence of fully crystallised double sulphurets of tellurium with alkalies; while, on the other hand, tellurium is not capable of replacing sulphur in sulphur salts. In a word, the analogies between sulphur and tellurium would, perhaps, not have been considered obvious, did not selenium form a connecting-link between the two, offering, in many respects, great similarities with the former, and, in other respects, with the latter element. Both selenium and tellurium are precipitated from tellurous and selenious acids, by means of sulphurous acid, protochloride of tin, metallic tin, and several other metals. The difficulties arising in the separation of tellurium, selenium, and sulphur consist, therefore, in the analogies between tellurium and selenium and between sulphuric and selenic acids. The usual way of separating selenium from tellurium is based upon the insolubility of seleniate of barytes. But the difficulties in preparing selenic and telluric acids, and the slight solubility of the tellurate of barytes, make this process a very tedious one. A mixture of seleniate and sulphurate of barytes are sometimes separated by reducing the former in a current of hydrogen and dissolving the selenide in hydrochloric acid. I tried, some time ago, to found an easier method of determining these elements, based upon the different way in which they are acted upon by cyanide of potassium.[1] Sulphur is dissolved when fused with this compound and not precipitated by hydrochloric acid. Selenium—as Mr. Crookes was the first to observe—is likewise dissolved and precipitated as such by any acid. Tellurium does not only not form a tellurocyanide when fused with cyanide of potassium, but takes the place of the cyanogen therein, forming telluride of potassium, which dissolves in water, with a purple colour, and is speedily decomposed, by the action of the air, into potash and metallic tellurium. This process, however, is imperfect, on account of a slight loss, which was first ascribed to the volatility of the elements at the temperature employed. The loss, however, occurs chiefly on the side of tellurium, which, of the three elements, is the least volatilous; and lately, while repeating the experiments, I observed that it was owing principally to part of the tellurium being oxidised and dissolved as telluriate of potash. It also became evident that, instead of melting the elements with cyanide of potassium, it sufficed to digest them with a solution of the salt. Sulphur and selenium are thus completely dissolved. A small proportion of tellurium forms tellurite of potash, and the rest remains in the metallic state. The separation of the elements was, therefore, conducted in the following manner:—A mixture of them, reduced to a fine powder, was boiled with a solution of cyanide of potassium in a water-bath for about eight hours. Tellurium was then collected on a filter. Selenium was precipitated in the filtrate by means of hydrochloric acid, and the second filtrate was mixed with some sulphite of soda, heated, and allowed to stand for twenty-four hours. The portion of tellurium *which had been dissolved* as tellurite of potash was thus *completely precipitated*. It was then added to the other portion collected on the filter, dried in the water-bath, and weighed. Selenium was determined in a similar manner, whilst the quantity of sulphur present was indicated by the difference in weight between the quantities employed and obtained anew. In this manner results of sufficient exactitude were arrived at, as for instance:—

	Grms. Employed.		Grms. Obtained.	Grms. Calculated.
Tellurium	1·190	as Tellurium	1·172	1·188
		as Tellurous acid	0·016	
Selenium	0·270		0·271	—
Sulphur	0·128		—	0·129

The same method may be employed for separating selenium from metals not soluble in cyanide of potassium. But if iron, copper, or other metals are present, which form soluble compounds with the re-agent, partly precipitated by hydrochloric acid, it is necessary to dissolve the mixture of the elements in acids, and to add a quantity of sulphuret of ammonium, sufficient to dissolve the selenium and tellurium. They must then be precipitated from this solution as sulphurets, and treated with cyanide of potassium, after the manner described. When sulphuret of selenium is acted upon by cyanide of potassium, the selenium is first dissolved, and a residue of sulphur remains behind, which disappears but slowly. The red modification of selenium is dissolved more easily than the black. Selenious acid cannot be reduced by being boiled with cyanide of potassium.

On the Production of Ethylamine by Reactions of the Oxy-ethers,[2] *by* M. Carey Lea.

While engaged in making a series of experiments on this subject I met with the paper of Juncadella (*Rép. de Chimie pure*, t. i. p. 273) and the observations of De Clermont (*Ibid.* p. 274) on the same subject. Finding that the subject had less novelty than I supposed, I merely offer here one or two of the results which I have obtained.

Nitrate of ethyl, C_4H_5O, NO_5, heated in sealed tubes with chloride of mercurammonium $\left.\begin{matrix}H_3\\Hg\end{matrix}\right\}N\,Cl$ for many hours in the water-bath, did not appear to react upon it. Kept for some time in a boiling saturated solution of chloride of calcium the tubes, although extremely thick green-glass combustion-tube of small calibre was used, exploded with great violence, shattering the vessel in which they were contained, although they had been wrapped in strong cloth.

Nitrate of ethyl, heated in a sealed tube with chloride of zincammonium $\left.\begin{matrix}H_3\\Zn\end{matrix}\right\}N\,Cl$ in the water-bath, does not appear to act upon it.

Nitrate of ethyl, heated in the water-bath in a sealed tube with carbamate of ammonia $NH_4O, NH_2C_2O_3$ dissolves the salt. On cooling, radiated crystals form. The contents of the tube evaporated to dryness with excess of chlorhydric acid, and then exhausted with ether, to which a few drops of strong alcohol have been added, yielded a solution which gave a chamois-coloured precipitate with bichloride of platinum, consisting of chloroplatinate of ethylamine.

·1185 gm. substance gave ·0465 metallic platinum corresponding to 39·25 per cent; theory requires 39·29.

The product was but small. Probably portions remained undissolved by the ether. No doubt portions of di- and tri-ethylamine are also formed in the above reaction, in the same manner as in those of the halogen ethers with ammonia.

An Account of Guy's Hospital Well, by William Odling, *M.B., F.R.S.*

In 1858, it was determined to sink a deep well at Guy's Hospital, which should yield a quantity of excellent water, sufficient to supply the entire wants of the institution. The well, with its appurtenances, was planned and constructed by Mr. R. W. Mylne, C.E., F.R.S. It was commenced in the winter of 1858, and completed in the summer of the following year. Its total depth from the surface of the yard is 297½ feet; its depth down to the chalk, 197 feet; and, consequently, its depth into the chalk, 100½ feet. The level of the yard is two feet above Trinity high-water mark. The following table gives an enumeration of the principal strata gone through, with their respective thicknesses, in feet:—

	Feet.
Made ground	12
Peat	2
Gravel	24
London clay	63
Red and yellow mottled clay	24
Blue shelly clay	7
Red mottled clay	10
Sand and clay, with pebbles	15
Gray sand	44
Chalk	100½
	297½

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[1] *Journal für Praktische Chemie*, vol. lxxi.

[2] Read before the American Association for the Advancement of Science, at Newport, August, 1860.

The water supplied by the well is bright and colourless to the eye, brisk and pleasant to the taste, and has a marked alkaline reaction to test-paper. Its temperature, when fresh from the well, was found to be 54°, that of the atmosphere being 65·5° F. When boiled, the water becomes slightly turbid, from the deposition of the carbonates of lime and magnesia, which the excess of carbonic acid, driven off by ebullition, formerly held in solution. When boiled down to half its bulk, the clear water filtered from the deposit does not afford any evidence of the presence of either lime or magnesia. It has still, however, a strong alkaline reaction, from the carbonate of soda which it contains. This filtered water, when slowly evaporated, yields a white, saline residue, in which crystals of chloride of sodium and sulphate and carbonate of soda may be readily distinguished by microscopic examination. The residue left by the evaporation of the water to dryness undergoes, when heated to redness, a slight charring, from the presence of a small proportion of organic matter, but does not evolve any ammoniacal vapour recognisable by delicate test-paper. In addition to the constituents already mentioned, the water was found to contain some potash-salt, minute quantities of phosphoric acid, iron, and alumina, and a scarcely-perceptible trace of nitric acid, but no ammonia. The specific gravity of the water at 60° F. is 1000·77. Its total solid contents amount to 51·69 grains per gallon. This residue is composed of the following constituents, in the quantities indicated:—

Separate Constituents.	Grains per Gallon.
Chlorine	9·75
Sulphuric acid	9·24
Carbonic acid	6·90
Silicic acid	0·79
Lime	1·47
Magnesia	1·15
Soda	13·87
Sodium	6·32
Potash	0·66
Phosphate of iron and alumina	0·05
Organic matter	0·94
Total by summation	51·14
Total by direct experiment	51·69

A sufficient amount of sodium to combine with the whole of the chlorine was calculated as sodium, the remainder as soda. The phosphate of iron and alumina was determined as follows:—A portion of the water was evaporated to dryness, and the residue, after ignition at a low red heat to destroy the organic matter, treated with an excess of hydrochloric acid. This was expelled by evaporation, and the dried residue heated to about 250°, to separate the silica. The resulting mass was then treated with strong hydrochloric acid, and dissolved in hot water. A few drops of nitro-muriatic acid were next added to oxidise the iron, after which the solution was filtered. The filtrate was precipitated with a slight excess of ammonia, and the whole gently heated. After standing for a night, the pale ochrey precipitate was collected on a filter, washed, dissolved in hydrochloric acid, reprecipitated by ammonia, collected on a filter, washed, dried, ignited, and weighed. The ignited mass was then treated with a little carbonate of soda, to render it soluble, and dissolved in hydrochloric acid. Phosphoric acid, alumina, and sesquioxide of iron were detected in the resulting solution. There was more than sufficient iron and alumina present to carry down the whole of the phosphoric acid. The organic matter was estimated by evaporating a portion of the water to dryness at 250°, then igniting the residue for some time at a low red heat, moistening it with water, treating the moist mass with carbonic acid for several hours, and then again drying at 250°. After this restoration of the carbonic acid, driven off from the lime and magnesia, the loss on ignition was considered to be organic matter. This method of determination, introduced by Dr. Clark, though not rigidly accurate, is, I believe, the best at present known. The other estimations were made in the usual manner, and do not call for any remark.

In the evaporated residue, the acid and basic constituents of the water seem to be combined with one another, somewhat in the manner indicated below:—

Combined Constituents.	Grains per Gallon.
Chloride of sodium	16·05
Carbonate of soda	12·36
Sulphate of soda	15·21
Sulphate of potash	1·33
Carbonate of lime	2·62
Carbonate of magnesia	2·37
Silica	0.79
Phosphate of iron and alumina	0·05
Organic matter	0·94
Total residue	51·72

It is observable, however, that the mode in which the different constituents are combined in the residue affords no information as to the mode in which they are combined in the water itself. It appears probable, indeed, that, in the water, every acid is united with every base, so as to form the greatest possible number of salts, the amount of each different salt formed being dependent conjointly upon the relative masses and affinities of its constituents. Even the arrangement of the different constituents of the residue, to form definite salts, is, to some extent, a conventional affair. Thus, in the above statement of results, the silica is placed apart, in accordance with ordinary custom. It was ascertained, however, that in the dried residue of the water, and still more in the considerably evaporated water, a portion of the silica existed in the state of silicate of soda, and, doubtless, the silica in the natural water was also present in that form. The magnesia, again, is set down as a mono-carbonate; whereas, neither in the water nor the residue does it really exist in that state, but in the former as bicarbonate, and in the latter as three-fourths carbonate. The water is but sparingly aërated. The total quantity of carbonic acid in a gallon was found to be, as a mean of two concordant determinations, 13·98 grains. Deducting the 6·90 grains of fixed carbonic acid, we have an excess of 7·08 grains, or just sufficient to convert the neutral carbonates into the state of bicarbonates, so that practically the water contains no free carbonic acid. There were also present in a gallon of the water 5·49 cubic inches of nitrogen, and 0·91 cubic inches of oxygen gas, measured at mean temperature and pressure. The low ratio of the oxygen to the nitrogen is probably due to a portion of the originally-dissolved oxygen having been expended in oxidation, during the percolation of the water through the earth. The water of which the above-described analysis was made was taken from the well on May 16th, 1860. Another sample was taken on July 23rd, 1860. The total residue in this second sample was found to be, as a mean of two experiments, 50·84 grains,[1] including 0·7 grain of combustible matter. In the interval between the taking of the samples there had been more than 7½ inches of rainfall, which may possibly have had to do with the slight diminution in the amount of solid contents yielded by the water.

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[1] Dr. Taylor examined the water in October, 1859, and found it to yield 47 grains per gallon of fixed residue.

In the following table, the analytical results obtained with Guy's Hospital water are compared with those obtained with some other deep-well waters in London, to which allusion is about to be made:—

Constituents.	I.	II.	III.	IV.	V.	VI.
Chloride of sodium	16·05	12·7	19·04	10·53	13·92	14·30
Carbonate of soda .	12·36	11·6	11·37	8·63	10·92	6·73
Sulphate of soda .	15·21	24·25	20·27	13·14	10·75	15·69
Sulphate of potash	1·33	—	1·33	—	2·10	1·92
Carbonate of lime .	2·62	6·18	2·74	3·50	4·14	5·39
Carbte. of magnesia	2·37	1·08	2·07	1·50	2·98	3·20
Silica . . .	0·79	0·44	0·40	0·50	0·70	0·60
Phosphate of iron and alumina .	0·05	0·43	0·97	traces	traces	traces
Organic matter .	0·94	—	0·66			
Total grs. per galln.	51·72	56·80	58·85	37·80	45·41	47·83

No. I. is my analysis of Guy's Hospital water.

No. II. is Mr. Graham's analysis of the water from Messrs. Combe and Delafield's well in Long Acre. In his analysis, the phosphoric acid was considered to be in combination with both iron and lime; so that the 0·43 grain, in the table, should be set down to phosphate of iron and lime, instead of to phosphate of iron and alumina.

No. III. is Mr. Dugald Campbell's analysis of the water of the Orange Street well, from which the fountains in Trafalgar Square are supplied. I have made a trifling alteration in the arrangement of his results, so as to make them comparable with my own.

No. IV. is Mr. Brande's analysis of the water of the Mint.

No. V. is my analysis of the chalk spring water, and

No. VI. my analysis of the sand spring water, from a deep well at Mr. Burnett's Distillery, in Vauxhall.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,

by Frank H. Storer.

(Continued from page 24.)

This alloy readily admits of being rolled, either hot or cold, and may be subjected to the operations of hammering or drawing without detriment, while alloys containing only a few per cent. more copper can be rolled hot only when the sheets are raised to a very high temperature. Even then it is exceedingly difficult to obtain thin sheets without cracking their edges.

On the other hand, alloys containing somewhat less than 60 per cent. of copper cannot well be rolled hot, since, when the ingots are heated, the exterior—especially at the corners—is liable to become pasty before the centre is soft enough to admit of being rolled.

In the preparation of the alloy of 60 per cent. of copper, now so extensively used for sheathing vessels, under the name of Muntz's sheathing or yellow-metal,—also known as malleable brass,—it is the custom of founders to reserve a portion of the zinc which has been weighed out for a charge, until the alloy in their pots or furnace has become sufficiently hot;[1] the last portions of zinc are then added in small pieces, a sample of the alloy being tested after each such addition. This is done by dipping out a small portion of the melted mass and pouring it into a mould; a little ingot, five or six inches long by an inch or less in thickness, is thus obtained, which, after cooling, is broken on an anvil, and its fracture observed. If this does not exhibit a smooth and homogeneous surface, more zinc is added to the alloy. The accuracy with which an experienced workman can thus obtain the desired alloy is truly astonishing, the more especially since this homogeneous alloy is confined within very narrow limits.

It is stated by founders that the alloy of 60 per cent. of copper and 40 per cent. of zinc will present almost precisely the same homogeneity of fracture, whether the test ingot prepared from it be cooled slowly, by exposure to the air, or rapidly, by plunging it into cold water, while alloys containing either more or less than 60 per cent. of copper are liable to assume different structures, according as they are cooled with greater or less rapidity. Two ingots are, therefore, sometimes cast on each trial of the alloy, one of which is cooled in water and the other in air, for comparison. This double test is, however, deemed superfluous by skilful workmen.

I may remark, in this connection, that I have repeatedly obtained crystals, by the method of partial cooling, from portions of melted yellow-metal taken from the founder's pots at the moment it had afforded them a satisfactory test. These crystals are in no wise different from those obtained by myself from alloys of almost identical composition.

Although, as has been stated, the tendency to form fibres seems to have ceased at the alloy containing 60 per cent. of copper, I cannot but think that the limit of its influence is less clearly defined than the "test" of yellow-metal founders would seem to indicate. In the circumstances under which this test is applied, it is doubtless true that no fibres are formed; but it is a matter of experience with manufacturers of yellow-metal, that the texture of the large ingots from which the sheets of sheathing are rolled is no longer so homogeneous as that of the small test ingot; they affirm also that this texture may vary greatly, according to the conditions in which the ingot is allowed to cool. It is evident, therefore, that during the processes of hot and cold rolling, and of annealing, to which the alloy is subsequently subjected, its texture may undergo various changes; while it is certain that the comparative durability of the sheathing, when exposed to the action of sea-water, must in great measure depend upon its relative compactness. If it be open and porous, as would be the case if a trace of the fibrous structure were present, it is clear that the sheathing would soon be destroyed,—not only because the salt water would come in contact with its interior portions, but also since the individual crystalline fibres of the alloy would, doubtless, resist its action more completely than the amorphous matter attached to them or contained in their interstices; from this a galvanic action would be produced, which could not fail to promote the corrosion of the alloy.

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[1] The temperature to which the alloy is brought before pouring is considered to be a point of much importance by founders. In their opinion, a very inferior alloy would be formed if it were not heated much more strongly than would be necessary to maintain it in the liquid state. Compare Bolley, *Zur Kentniss der Moleculareigenschaften des Zinks, Ann. Ch. u. Pharm.*, xcv. 302. This subject is evidently connected with the phenomena of tempering described in another part of this memoir.

One of the most common complaints against yellow-metal arises from a tendency which some specimens of it exhibit to become so friable, after an exposure of longer or shorter duration to sea-water, that the sheets may readily be broken in pieces, sometimes even between the fingers. Attention has recently been called to this subject by Bobierre (Thèses présentées à la Faculté des Sciences de Paris. "Thèse de Physique: Des Phénomènes électro-chimiques qui caractérisent l'Altération, à la Mer, des Alliages employés pour doubler les Navires," p. 61. Nantes: Imp. Busseuil. 1858,) who would refer such cases more particularly to chemical conditions depending, as he thinks, upon the too great proportion of zinc which is used in the preparation of the alloy, yellow-metal, as well as to the changes of composition produced by hot-rolling.

I cannot agree with this conclusion. In my own opinion, the other alternative which Bobierre has suggested, namely, peculiar arrangement of the molecules of which the alloy is composed, furnishes the true explanation of the difficulty. It is, however, possible that at times, when the temperature of the reverberatory, in which the alloy is heated before passing to the rollers, is not properly regulated, zinc may be burned off from the exterior portions of the sheet, and that the alloy, richer in copper, which would thus be formed, may subsequently be pressed into the body of the sheet during the operation of rolling. An alloy destitute of homogeneity would result from this treatment, which could hardly be durable in any event. Instances of this sort must nevertheless be rare, for no part of the process is watched by the manufacturers more scrupulously than this.

It must also be borne in mind, that, of the enormous quantity of yellow-metal which is now used by the merchant vessels of Great Britain and of this country,—all of which is composed of 60 parts of copper and 40 parts of zinc, and rolled hot,—only a comparatively small portion passes into the friable condition to which I have alluded.

In most cases the alteration which the sheathing undergoes is gradual and regular, and the portion which remains after the wear of several years is still malleable. Indeed, the absolute amount of sheathing which becomes friable is entirely out of proportion with the annoyance to which it subjects ship-owners;[1] for it rarely happens, even in the worst instances, that more than one-third of the sheets upon a vessel become friable, the remainder being in good condition.

The friability is, therefore, a purely accidental occurrence, and by no means a necessary consequence either of hot-rolling or of the presence of 40 per cent. of zinc in the alloy, as has been implied by Bobierre (Thèses, p. 77). I am strongly of opinion that it might be in every instance entirely obviated by methodically annealing or tempering the sheets of alloy in such a manner that no fibres could form in them, and that their structure should be homogeneous.

Bobierre, in his very able thesis, to which I have already alluded, has urged that it would be well to discontinue the use of the alloy containing 60 per cent. of copper, which admits of being rolled hot, and to substitute for it sheathing prepared from alloys containing about 66 per cent. of copper, which can only be prepared by a most laborious process of cold rolling. It is true that the fibres which I have described would in this case probably never be encountered. Still, there are several important objections to the proposition of M. Bobierre. Not only would the method of cold-rolling consume a much greater amount of time and labour, but it would be exceedingly difficult, if not impossible, to procure any alloy the composition of which could be maintained so nearly constant as is the case with yellow-metal.[2] It is not probable that a test like the one applied to this alloy could be found anywhere else in the whole series.

It is frequently stated in chemical text-books that yellow-metal is always prepared from "best-selected" copper, and one is led to infer that a metal of peculiar purity is alone used in its manufacture. This may once have been the case; but since the immense increase in the use of this alloy, it would no longer be practicable to obtain a sufficient quantity of copper of uniform character, or, indeed, of any one kind, from which to prepare the large quantity of sheathing which is used. In an establishment where expense would be a secondary consideration, as, for example, in a Government workshop, it might still be possible to prepare an alloy constantly from the same sorts of copper and of zinc, in which case, although the amount of zinc lost would probably be subject to considerable variations, one might, nevertheless, soon be able to control the process, and to prepare an alloy of the composition proposed by Bobierre, so that only trifling variations should occur in the composition of the product. But in ordinary practice manufacturers are compelled to make use of the most varied kinds of copper, not only because the supply of the best sorts is limited, but particularly from the fact, that, owing to the competition which exists between the various foundries,—or, rather, by force of the laws which regulate supply and demand,—they are obliged to re-melt larger quantities of old copper sheathing,[3] the origin of which is unknown to them, and which may have been originally prepared from copper of inferior quality.

(To be continued.)

The Adulteration of Food Act in the City.

The following extracts are taken from Dr. H. Letheby's "Quarterly Report on the Sanitary Condition of the City of London:"—

In the matter of this Act, I have had but four applications for analysis. These are one for milk, two for bread, and one for mustard. The milk was delivered in the ordinary way to a householder in Martin's Lane, Cannon Street, and it was adulterated with water. One of the samples of bread was said to have been purchased of Mr. Negus, 158, Bishopsgate Street: it was adulterated with rice and alum, and contained about 44 per cent. of water. The other sample was said to have been purchased of Mr. Clunie, at No. 4, Finsbury Pavement: it was genuine bread, of good wheaten flour. The sample of mustard was said to have been obtained at Mr. Colman's, in Cannon Street; it consisted of mustard meal, wheaten flour, and turmeric; but considering that the proportion of wheaten flour was not excessively large, and that it is a common and necessary practice to dilute the meal of mustard, in order that it may be used as an agreeable condiment, I do not think that the mixture was an adulteration, and therefore I certified that it did not contain anything injurious to health.

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[1] Whenever the destruction of a portion of the sheathing requires hat a ship be hauled up for repairs, it is customary, since this operaion is an expensive one, to re-sheathe the vessel entirely; for, as the riable sheets are interspersed among the others, and since all the heets are somewhat worn, it would be bad economy to attempt any artial repairs. Of course the owner of the vessel regards the entire uit of sheathing as being worn out, and forms his opinion of the urability of yellow-metal in accordance with this view.

[2] An alloy of constant composition might, however, possibly be prepared, by adding known quantities of melted copper to determined volumes of molten yellow-metal which had been prepared by the usual method.

[3] The popular notion that a better—"more compact"—product is obtained by *re-melting* any alloy, may also have some connection with this custom.

I am very anxious that the practical working of this Act should be of advantage to the public, and should be brought fully and fairly into operation within the City. This, however, must be effected by the energy of the citizens themselves; for, however desirable it may be to expose the frauds and dangers of adulteration, the Act has not contemplated that the exposure should be brought about by the offices of the common informer. In every case, therefore, the public must initiate the inquiry by a fair and open purchase of the article suspected to be adulterated; and as far as my duties are concerned, as the Analyst appointed for the City, I shall endeavour so to conduct every analytical investigation, as, on the one hand to expose fraud, and on the other to protect the interests of the honest dealer. Whether it may be thought desirable, hereafter, to undertake an extensive series of systematic examinations of the various articles of food and drink, for the purpose of exposing the fraudulent practices of trade, is a matter that will require serious consideration. At present, however, the effectual working of the Act must rest entirely with the public; and with the view of indicating the right mode of proceeding, I have reproduced in the Appendix an abstract from my recent report, on the practical working of the Act.

Of the many classes of adulteration that deserve attention, there are none so serious in their consequences as those which affect the stamina and health of the people. The adulteration of bread, for example, with materials that lessen its nutritive value, is not merely a fraud on the pocket, but is also a fraud on the constitution of the consumer; for that which diminishes the flesh-forming constituents of so important an article of diet, must necessarily be a means of impairing the growth of the animal fabric, and of weakening the force of labour.

In some cases of adulteration, the practice of it has a far more extensive relation to the affairs of the community than at first sight appears. To take the sophistication of beer and porter as an instance: it is well known that the publicans, almost without exception, reduce their liquors with water after they are received from the brewer. The proportion in which this is added to the beer at the better class of houses is nine gallons per puncheon, and in second-rate establishments the quantity of water is doubled. This must be compensated for by the addition of ingredients which give the appearance of strength, and a mixture is openly sold for this purpose. The composition of it varies in different cases; for each expert has his own particular nostrum. The chief ingredients, however, are a saccharine body, as foots and liquorice to sweeten it; a bitter principle, as gentian, quassia, sumach, and terra japonica, to give astringency; a thickening material, as linseed, to give body; a colouring matter, as burnt sugar, to darken it; cocculus indicus, to give a false strength; and common salt, capsicum, copperas, and Dantzic spruce to produce a head, as well as to impart certain refinements of flavour. In the case of ale, its apparent strength is restored with bitters and sugar-candy. Now, apart from the question of how much injury is done to the health of the consumer by this practice, or how large is the fraud on the pocket of the purchaser, it is a matter of interest to know what is the effect of it on the revenue. There are, I believe, about 6200 publicans and beer retailers in this metropolis. Supposing that each, on an average, has a sale for only six barrels of beer and porter per week, and that the degree of dilution is but to the extent of five gallons of water per barrel, in the aggregate there would have been used 186,000 gallons of water per week. This, if properly and honestly brewed into beer, would have required about 1722 quarters of malt; and as each quarter of malt pays a duty of 22s. 6d. to the revenue, there is a fraud of rather more than £1937 sterling per week, or nearly £100,800 per annum. In like manner almost every such fraudulent practice of trade has a wider relationship to the public interests than the mere act itself would imply. I will not, however, pursue this further at present; and, in leaving it, I venture to express a hope, that as the public interests are so largely concerned in the general application of this Act of Parliament, its execution will be faithful and vigorous.

Abstract of the Report on the Practical Working of the Act.—According to the provisions of the Act it is necessary that the different states of the investigation shall be conducted in a systematic manner.

First. *In respect of the Purchasing of the Article.*—In all cases the purchaser of an article suspected to be adulterated, shall, directly he has purchased it, and before he removes it from the shop, give notice to the dealer, or the person serving him, that he intends to have the article analysed; so that the dealer may have the opportunity of accompanying the purchaser to the analyst, or of securing the article in such a manner as to prevent it from being tampered with. This will be necessary, in order that proof may be furnished of the identity of the article examined.

Second. *As regards the Analysis.*—Wherever an analyst is appointed, any purchaser of an article of food or drink, shall be entitled, on payment to the analyst of a sum not less than two shillings and sixpence, nor more than ten shillings and sixpence, to have the article analysed, and to receive from him a certificate of the result thereof. Considering, however, that it is not easy or practicable to draw up a scale of charges for the various kinds of investigations contemplated by the Act, and that the fee named will rarely cover the expenses of the analysis, it will, I think, be expedient to fix an uniform charge, intermediate in the scale, say five shillings, for each analysis. This will prevent a great deal of complexity, and will, at the same time, show that the object of the charge is not to cover the expenses of the inquiry, but rather to guard the dealer against unnecessary annoyance and groundless complaint. The poor, however, who are the chief sufferers from adulteration, will, I fear, be unable to pay the fee named; and, unless something be done to assist them in the matter, they will be shut out, most completely, from the benefits of the Act. I would suggest, therefore, that you give to the analyst a discretionary power to conduct such inquiries for the poor without fee. It is necessary, however, that a complaint should first come to him from a poor person, and the complainant should be entitled to leave with the analyst a sample of the suspected article, together with the name and address of the dealer; and if, in the judgment of the analyst, the matter is sufficiently serious to demand inquiry, he shall be at liberty to instruct the inspector to purchase a sample of the suspected article at the place named, and then to submit it to analysis free of charge.

Lastly. *In respect of the Certificate.*—I place before you the form of certificate, which, with your sanction, I will use. It is constructed so as to indicate the object of the inquiry, and to express the result of the analysis. It also expresses the opinion of the analyst—whether the article is adulterated or not, and whether the article is so adulterated as to be injurious to health. These indeed are the several points to which the Act has directed attention; and in giving expression to them, it is necessary on the one hand to be very explicit, so as to expose fraud, and on the other not so emphatic as to furnish a puff for the rivalries of trade.

PHARMACY, TOXICOLOGY, &c.

Remarks on Polygalic Acid, by WILLIAM PROCTER, *Jun.*

AMONG the questions proposed last year for investigation, the following was accepted by me:—

"What is the most eligible process for obtaining polygalic acid from senega? In what proportion does it exist in that root? And what are the forms for prescribing it as substitutes for the preparations of seneka?"

Seneka snake-root, as one of the most efficient American contributions to the "Materia Medica," early attracted the attention of European pharmaceutists and chemists, and at least half-a-dozen analyses of it had been published more than twenty years ago, when it was very fully investigated by Quevenne,[1] in his inaugural essay sustained at the School of Pharmacy, at Paris.

According to this analysis, the constituents of seneka are as follows, viz.:—

Organic.	Inorganic.
Polygalic acid.	Carbonate, phosphate, and sulphate of potassa.
Virgineic acid.	Chloride of potassium.
Pectic acid.	Carbonate, sulphate, and phosphate of lime.
Tannic acid.	Alumina, magnesia, silica, and iron.
Bitter yellow colouring matter.	
Gum, albumen, &c.	
Cerin, and fixed oil.	

Polygalic acid, so called by Quevenne, was previously made known, in a less pure state, as *senegin*, by Gehlin. It is the active constituent of the root, although some influence may be due to the virgineic acid and colouring principle. It may also be noticed that both Dulong and Foenuille found malic acid in this root, united with lime and potassa.

It may not be out of place to state the characteristics of this substance, as it is certainly deserving of a better destiny than has thus far been accorded to it. Polygalic acid is a white, amorphous powder, without any tendency to crystallise, and is unalterable in the air. It is inodorous when pure, but is apt to retain traces of virgineic acid. Its taste, at first but slight, soon becomes pungent and acrid, and produces a painful constriction of the throat. When its dust is inhaled through the nostrils, irritation and violent sneezing occur. It is not volatile; when burnt in a tube it affords no nitrogen, and is wholly combustible. Polygalic dissolves slowly in cold water, but readily in hot water, forming a solution which reddens litmus-paper, and froths strongly by agitation. It is soluble in boiling alcohol, but the larger part separates on cooling. It is soluble in diluted alcohol, but the solution has a tendency to deposit it on standing. It is absolutely insoluble in ether, and in acetic ether, the fixed and volatile oils. It is soluble in alkaline solutions, with the production of a greenish-yellow coloration. It is precipitated in its free state by subacetate of lead, and proto-nitrate of mercury, but by no other salts; various salts throw it down from solution when it is combined with an excess of potassa or soda. When heated with nitric acid it affords oxalic and picric acids, and sulphuric acid decomposes it with a rose and violet colour. The acid character of this substance is so slight that it will not decompose the carbonates, yet it forms amorphous salts, with alkaline and metallic bases. Quevenne suspected a close relationship between polygalic acid and saponin. More recently,[2] M. Bolley has resumed the inquiry, and believes that both saponin and polygalic acid, are glucosides, like salicin, resolved by hydrochloric acid into sugar and their derivative body *sapogenin*, and that their formula is the same.

Preparation of Polygalic Acid.—The particular object of this paper is the preparation and administration of this substance. The original process of Quevenne required precipitation by acetate of lead, and the action of sulphuretted hydrogen; but he afterwards greatly simplified the process, but retained some complications not necessary to obtain the acid sufficiently pure for medicinal use. The following process I have tried several times; and it has the merit of great simplicity:—

Take of senega, in powder (50 meshes to the inch) 10,000 grains; alcohol, ether, and water, of each a sufficient quantity. Moisten the senega with half-a-pint of a mixture of two parts of alcohol and one part of water, pack it in a large glass funnel; cover the surface with a piece of muslin, or paper, and pour on the same liquid until three pints have slowly passed by drops, or until the root is nearly exhausted. Evaporate this tincture to 12 fluid ounces, which removes nearly the whole of the alcohol, and then agitate it with repeated portions of ether, until the latter ceases to acquire colour, decanting each portion before adding the next, which requires about a pint and a-half. The syrupy liquid is now mixed with a pint and a-half of strong alcohol, and a half-a-pint of ether, well agitated several times, and set aside to repose. The polygalic acid being nearly insoluble in this menstruum, precipitates as a bulky, light, fawn-coloured precipitate. If on adding a small portion of the clear liquid to a similar mixture of ether and alcohol, no precipitate occurs, the whole of the polygalic acid capable of being thus thrown down has been separated. The supernatant liquid is now carefully decanted from the precipitate, which is thrown on to a filter and carefully washed with a mixture of two parts of alcohol and one of ether, till the adhering coloured liquid has all been displaced. The filter is then spread on an absorbent surface of paper, pressed strongly, the mass broken into small fragments, dried, and powdered. The resulting powder weighs 550 grains, 5½ per cent. of the weight of the senega; making allowance for wastage, senega may be said to contain $\frac{1}{16}$th of its weight of this substance, and, consequently, 30 grains will equal an ounce of the root.

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[1] *Journ. de Pharm.*, 1836, p. 449.

[2] *Annal. der Chemie*, and *Amer. Journ. of Pharm.*, vol. xxvii, p. 43.

In this state polygalic acid is not pure, but is sufficiently so for all medical purposes. It retains traces of colouring matter, and of some substances which are separated by the ether, but in very small amount. By re-dissolving it in hot water, and precipitating with the mixture of ether and alcohol, and afterwards dissolving it in hot alcohol, with animal charcoal, and filtering hot, it is obtained quite white.

The ethereal washing liquids first obtained were evaporated till no ether remained. The residue consisted of a dark amber-coloured fixed oil, admixed with crystals of virgineic acid, which formed a crust on its surface, and a dense aqueous solution of the same acid, which strongly affected litmus-paper.

The liquid from which the polygalic acid precipitated was allowed to stand a month, when the sides and bottom of the vessel were spangled with a crystalline matter, in small quantity. This substance, is soluble in water, when heated; less soluble in alcohol; gritty between the teeth; has no taste of senega; fuses by heat, blackens, and is almost entirely consumed. Concentrated SO_3 dissolves it with heat without the evolution of either ammonia or HCl. Its aqueous solution is not precipitated by either chloride of barium or nitrate of silver. A crystal placed on moistened litmus-paper produced a reddish spot. It is possible that this substance is an acid salt of malic or some analogous acid. The small quantity of ash it leaves is alkaline. The investigation was not pushed further.

The liquid, from which the crystals deposited, by evaporation, yielded an extract tasting strongly of senega.

As regards the pharmaceutical applications of polygalic acid, several experiments were made:—

1. Five grains of the acid in a fluid ounce of water, kept in a vial for two months, acquired a fetid odour, whilst a portion of matter precipitated, yet the taste of the solation was strongly that of senega.

2. Five grains of the acid were dissolved in an ounce of cold water, with five grains of bicarbonate of potassa, and kept the same length of time. This solution also spoiled, but retained its senega taste.

The best form of prescribing polygalic acid, in liquid form, is with syrup of gum arabic, to blunt its acrimony. It is necessary to dissolve it in a little hot water, and add it warm to the syrup of gum, and mix them. It may also be added to syrup of ipecacuanha, to increase its expectorant power. But the form best adapted to administer this principle is in pills and powders. In doses of one or two grains (which represent 15 to 30 grains of senega), the full effect of a tablespoonful and a fluid ounce of decoction of senega is obtained. Where a more gentle effect is desired, the dose must, of course be greatly reduced,—to $\frac{1}{2}$ or $\frac{1}{4}$ of a grain,—and can be associated with ipecacuanha, tartar emetic, and other medicine with which senega is given. When the form of powders is adopted, a mixture of powdered gum and sugar should always be added, to blunt the acrimony of the polygalic acid. Sugar of milk triturated with the acid, in the proportion of one grain to nine grains, is an eligible form for use.

In cough mixtures, when not contra-indicated or incompatible, the addition of an equal weight of bicarbonate of potassa facilitates its solution. It is not advisable to keep a permanent liquid preparation of polygalic acid, leaving that form for the officinal syrup, but it undoubtedly offers an admirable means of using senega in the forms above indicated, when the physician can regulate the dose and associate it with other remedies in the nicest manner.—*Proceedings of the American Pharm. Association*, 1859.

Lead in the Calcareous Deposits of Kettles, *by* HENRY OSBORN, *M.D.*

IN making an examination of water for the presence of lead, it is seldom requisite to reduce the water by evaporation, but simply to test it by means of the ordinary tests, and should there be no indication of lead, we infer that the water is free from the poison. Whenever water is capable of acting upon lead, we generally find the earthy matter, which is held in solution, so small that no concretion takes place in the kettles or boilers; and the carbonic acid, which the water contains, exists in a free state, or nearly so. Until a very recent period I was induced to believe that whenever the carbonic acid held carbonate of lime in solution it was incapable of taking up the lead. I was aware, however, of the liability of such water to corrode the metal into holes, even when a sufficiency of sulphates, &c., were present to act as protecting agents. In carrying my investigations a step further, I found, by evaporating to dryness one gallon of water (which, on previously testing, did not appear to contain lead) and analysing the residue, a small quantity of lead was present. I then, on examining the calcareous deposits taken from a number of kettles and boilers, by treating the earthy matter with acetic acid, and found an appreciable quantity of carbonate of lead to exist in a few grains of the crust. The proportion, however, varied in different samples, owing to the length of the one-inch leaden pipes which transmitted the water from the main, and the length of time in which the water is permitted to rest in contact with the lead. The next point was to determine whether this accumulation of carbonate of lead, combined with carbonate of lime, organic matter, &c., was liable to be re-dissolved by boiling water in the encrusted kettles; but I failed to prove that the lead was re-dissolved, on applying the tests, without concentration. Hence, we may conclude that there is but little risk of being poisoned by such accumulations, though it is advisable to avoid the insoluble lead as much as possible, because the particles of the crustaceous matter may become detached by ebullition and suspended by the water. It is evident, then, that water may take up so small a quantity of lead as to escape detection by our most delicate colour-tests prior to evaporation; but, by boiling such water, the carbonic acid escapes, and minute quantities of carbonate of lead are daily thrown down, and may thus accumulate to a considerable extent.

The house in which I now reside is supplied with water from two sources,—one from a pump, and the other from the town water-works. The former is most delicious water, containing but a very small quantity of solid contents, and leaving no deposit in the kettles. Its free carbonic acid and purity, however, render it capable of forming carbonate of lead; and, if I were to permit its use for domestic purposes, my constitution would soon succumb to its enervating influence. The latter water, supplied from the town, is the water which yields the deposit now under consideration; but, as it passes through a small iron tube from the main, instead of lead, I find no insoluble carbonate of lead in the concrete matter in the kettles. Whilst engaged in making this investigation, my attention was drawn to a singular and simple method of preventing the incrustation from taking place. I was informed that a schoolboy's marble, when placed in a kettle, would prevent the crust from forming, and the interior of the vessels never required cleansing. Being unable to account for the action of a marble in preventing such a formation, I was referred to a person who had kept a marble in her tea-kettle for five years. On inspection, I found a very little deposit, except in the spout of the vessel, which part only was thickly encrusted. On examining the marble, I found that it had increased in size, but the surface of it was perfectly smooth, and so hard that I could scarcely scrape it with a knife; but when cut it presented fine concentric layers, resembling the most compact calculus. When split asunder the marble was found in the centre, measuring three-quarters of an inch in diameter, and the concrete matter enclosing it, only one-eighth of an inch in thickness, which had been about five years forming. The interesting part of this formation is the resemblance which it bears to a calculus, and shows that an artificial calculus may be formed in a tea-kettle as well as in the bladder of the human body, though not composed of the same constituents.—*Medical Times.*

A Means of Detecting the Presence of Castor-oil in the Volatile Oils, by HARRY NAPIER DRAPER, *F.C.S.I.*

As a general rule, the volatile oils which are employed in pharmacy and perfumery are, at least in our markets, to be obtained pure; but it does occasionally happen that they are sophisticated with some fluid of low price. Turpentine of wood, alcohol, and the fixed oils are the most common adulterants. It is a very simple matter to detect alcohol, and the admixture of turpentine so much deteriorates the proper odour of the oil that but slight experience is required for its recognition. The insolubility of nearly all the fixed oils in alcohol, and the consequent facility of ascertaining their presence by simple treatment of the suspected sample with this re-agent is so well known, that this mode of adulteration is seldom attempted. Castor-oil forms, however, an exception to the general behaviour of this class of bodies in this respect. Soluble not only in alcohol, but in all menstrua which dissolve the volatile oils, its recognition by the means just pointed out would be impracticable, and its miscibility with alcohol can even be taken advantage of to introduce the latter into the volatile oil in such proportion as to imitate the natural fluidity of a pure specimen. Having recently had occasion to examine an oil which was suspected to be adulterated in this manner, it occurred to me that the reaction by which ricinus oil when treated with nitric acid gives rise to œnanthylic acid ($C_{14}H_{13}O_3 + HO$) might be taken advantage of for its detection.

This body is a product of the oxidation of the castor-oil, and is formed when the warm oil is treated with an excess of nitric acid. A violent action ensues during which much nitrous acid is disengaged, and there is found floating in the acid liquid, when the residue is mixed with water, a soft, unctuous mass, which is, I believe, the palmine of Boudet, accompanied by fatty acids. If the acid liquid be now neutralised with carbonate of soda, so as to entirely remove the odour of nitrous acid, the smell of the œnanthylic acid can be most clearly recognised. It is upon this fact that I have based the method of detecting the presence of castor-oil which is now to be described. The *modus operandi* is as follows:—Twenty drops of the suspected oil are placed in a porcelain capsule, and heated on a sand-bath until the odour of the oil is no longer perceived. To the residue—if there be any—add five or six drops of nitric acid, and as soon as the action has subsided, dilute with a solution of carbonate of soda. If castor-oil be present, the odour of the œnanthylic acid will now be at once perceived, and once smelled, is not likely to be mistaken for any other. As, however, it can hardly be compared to any other familiar odour, any one who has occasion to use this test will do well to make the experiment with castor-oil alone, and to acquaint himself with the peculiar smell.

In this way I have been able to detect the admixture of five per cent. of castor-oil in volatile oils, and have no doubt even a much smaller proportion could be recognised.—*Dublin Medical Press.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. FARADAY, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

LECTURE III. (Jan. 1, 1861.)

Products: Water from the Combustion—Nature of Water—A Compound—Hydrogen.

I dare say you well remember that when we parted we had just mentioned the word "products" from the candle. For when a candle burns we found we were able, by nice adjustment, to get various products from it. There was one substance which was not obtained when the candle was burning properly, which was charcoal or smoke, and there was some other substance that went upwards from the flame which did not appear as smoke, but took some other form and made part of that general current which, ascending from the candle upwards, becomes invisible and escapes. There were also other products to mention. You remember that in that rising current having its origin at the candle, we found that a part was condensible against a cold spoon, or against a clean plate, or any other cold thing, and part was incondensible.

We will first take the condensible part and examine it, and, strange to say, we find that that part of the product is just water—nothing but water. I last time spoke of it incidentally, merely saying that water was produced among the condensible products of the candle; but, to-day, I wish to draw your attention to water that we may examine it carefully, especially in relation to this subject, and also with respect to its general existence on the surface of the globe.

Now, having previously arranged an experiment for the purpose of condensing water from the products of the candle, my next point will be to show you this water; and perhaps one of the best means that I can adopt for showing its presence to so many at once, is to exhibit a very visible action of water, and then to apply that test to what is collected as a drop at the bottom of that vessel. I have here a chemical substance, discovered by Sir Humphry Davy, which has a very energetic action upon water, which I shall use as a test of the presence of water. If I take a little piece of it—it is called potassium, as coming from potash—if I take a little piece of it, and throw it in that basin, you see how it shows the presence of water by lighting up and floating about, burning. I am now going to take away the candle which has been burning underneath the vessel containing ice and salt, and you see a drop of water—a condensed product of the candle—hanging from the under surface of the dish. I will show you that potassium has the same action upon it as upon the water in that basin in the experiment we have just tried. See! it takes fire and burns in just the same manner. I will take another drop upon this glass slab, and when I put the potassium on to it you see at once, from its taking fire, that there is water present. Now, that water was produced by the candle. In the same manner, if I put this spirit-lamp under that jar, you will soon see the latter become damp from the dew which is deposited upon it—that dew being the result of combustion; and I have no doubt you will shortly see, by the drops of water which fall upon the paper below, that there is a good deal of water produced from the combustion of the lamp. I will let it remain, and you can afterwards see how much water has been collected. So, if I take a gas-lamp, and put any cooling arrangement over it, I shall get water,—water being likewise produced from the combustion of gas. Here, in this bottle, is a quantity of water—perfectly pure, distilled water, produced from the combustion of a gas-lamp—in no point different from the water that you distill from the river, or ocean, or spring, but exactly the same thing. Water is one individual thing, it never changes. We can add to it by careful adjustment, for a little while, or we can take it apart and get other things from it, but water, as water, remains always the same, either in a solid, liquid, or fluid state. Here again [holding another bottle] is some water produced by the combustion of an oil-lamp. A pint of oil, when burnt fairly and properly, produces rather more than a pint of water. Here, again, is some water, produced by a rather long experiment, from a wax candle. And so we can go on with almost all combustible substances, and find that if they burn with a flame, as a candle, they produce water. You may make these experiments yourselves; the head of a poker is a very good thing to try with, and if it remains cold long enough over the candle, you may get water condensed in drops on it; or a spoon, or ladle, or anything else may be used, provided it be clean, and can carry off the heat, and so condense the water.

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[1] Reported *verbatim* by special permission.

And now—to go into the history of this wonderful production of water from combustibles, and by combustion,—I must first of all tell you that this water may exist in different conditions, and although you may now be acquainted with all its forms, they still require us to give a little attention to them for the present; so that we may perceive how the water, whilst it goes through its Protean changes, is entirely and absolutely the same thing, whether it is produced from a candle by combustion, or from the rivers or ocean.

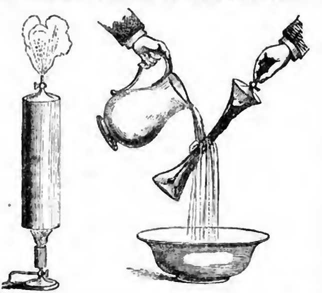
First of all, water when at the coldest is ice. Now, we philosophers,—I hope that I may class you and myself together in this case,—speak of water as water, whether it be in its solid, or liquid, or gaseous state—we speak of it chemically as water. Water is a thing compounded of two substances, one of which we have derived from the candle; and the other, which we shall find elsewhere. Water may occur as ice; and you have had most excellent opportunities lately of seeing this. Ice changes back into water; and on our last Sabbath we had a strong instance of this change, by the sad catastrophe which occurred in our own house, as well as in the houses of many of you. Ice changes back into water when the temperature is raised: water also changes into steam when it is warmed enough. The water which we have here before us as ice, is in its densest state, and although it changes in weight, in condition, in form, and in many other qualities, it still is water; and whether we alter it into ice by cooling, or whether we change it into steam by heat, it increases in volume,—in the one case very strangely and powerfully, and in the other case very largely, and strangely, and wonderfully. For instance, I will now take this tin cylinder, and pour a little water into it, and seeing how much water I pour in, you may easily estimate for yourselves how high it will rise in the vessel: it will cover the bottom about two inches. I am now about to convert the water into steam, for the purpose of showing to you the different volumes which water occupies in its different states of water and steam.

Let us now take the case of water changing into ice; we can effect that by cooling it in a mixture of salt and pounded ice,—and I shall do so to show you the expansion of water into a thing of larger bulk when it is so changed. These bottles [holding one] are made of strong cast-iron, very strong and very thick—I suppose they are the third of an inch in thickness; they are very carefully filled with water, so as to exclude all air, and then they are screwed down tight. We shall see that when we freeze the water in these iron vessels, they will not be able to hold the ice, and the expansion within them will break them in pieces as these [pointing to some fragments] are broken, which have been bottles of exactly the same kind. I am about to put these two bottles into that mixture of ice and salt, for the purpose of showing that when water becomes ice, it changes in volume in this extraordinary way.

In the meantime look at the change which has taken place in the water to which we have applied heat; it is losing its fluid state. You may tell this by two or three circumstances. I have covered this glass flask—in which water is boiling—over with a watch-glass. Do you see what happens? It rattles away like a valve chattering, because the steam rising from the boiling water sends the valve up and down, and forces itself out, and so makes it clatter. You can very easily perceive that that flask is quite full of steam, or else it would not force its way out. You see also that the flask contains a substance very much larger than the water, for it fills the whole of the flask over and over again, and there it is blowing away into the air; and yet you cannot observe any great change in the bulk of the water, which shows you that its change of bulk is very great when it becomes steam.

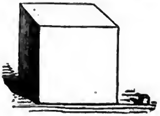
I have put our iron bottles containing water into this freezing mixture that you may see what happens. No communication will take place, you observe, between the water in the bottles and the ice in the outer vessel. But there will be a conveyance of heat from the one to the other, and if we are successful—we are making our experiment in very great haste—I expect you will by-and-by, so soon as the cold has taken possession of the bottles and their contents, hear a pop on the occasion of the bursting of the one bottle or the other, and, when we come to examine the bottles, we shall find their contents masses of ice partly enclosed by the covering of iron which is too small for them, because the ice is larger in bulk than the water. You know very well that ice floats upon water; if a boy falls through a hole into the water, he tries to get on the ice again to float him up. Why does the ice float? Think of that, and philosophise. Because the ice is larger than the quantity of water which can produce it, and therefore the ice weighs the lightest and the water is the heaviest.

To return now to the action of heat on water. See what a stream of vapour is issuing from this tin vessel. You observe, we must have made it quite full of steam to have it sent out in that great quantity. And now, as we can convert the water into steam by heat, we convert it back into liquid water by the application of cold. And if we take a glass, or any other cold thing, and hold it over this steam, see how soon it gets damp with water; it will condense it until the glass is warm—it condenses the water which is now running down the sides of it. I have here another experiment to show the condensation of water from a vaporous state back into a liquid state, in the same way as the vapour, one of the products of the candle, was condensed against the bottom of the dish and obtained in the form of water; and to show you how truly and thoroughly these changes take place, I shall take this tin flask, which is now full of steam, and I shall close the top. We shall see what takes place when we cause this water or steam to return back to the fluid state by pouring some cold water on the outside. [The Lecturer poured the cold water over the vessel, when it immediately collapsed.] You see what has happened. If I had closed the stopper and still kept the heat applied to it, it would have burst the vessel; yet, when then the steam returns to water, the vessel collapses, there being a vacuum produced inside by the condensation of the steam. I show you these changes for the purpose of pointing out that in all these occurrences there is nothing that changes the water into



another thing; it still remains water, and so the vessel is obliged to give way and is blown inwards, as in the other case, by the further application of heat, it would have been blown outwards.

And what do you think the bulk of that water is when it assumes the vaporous condition? You see that cube [pointing];



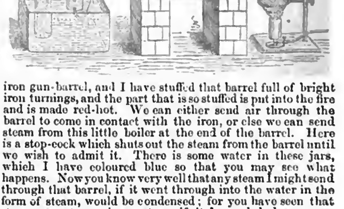
it is a cubic foot. There, by its side is a cubic inch: it is square, exactly the same shape as the cubic foot, and that bulk of water [the cubic inch] will make that bulk [the cubic foot] of steam, and the application of cold will contract that large quantity of steam into that small quantity of water. [One of the iron bottles burst at that moment.] Ah! There is one of our bottles burst, and here you see is a crack down one side an eighth of an inch in width. [The other now exploded, sending the freezing mixture in all directions.] This other bottle is now broken; although the iron was nearly half-an-inch thick, the ice has burst it asunder. These changes always take place in water; they do not require to be always produced by artificial means, we only use them here because we want to produce a small winter round that little bottle instead of a large one. But if you go to Canada, or to the North, you will find the temperature there out of doors will do the same thing as has been done here by the freezing mixture.

To return to our quiet philosophy. . We shall not in future be deceived, therefore, by any changes that are produced in water. Water is the same everywhere, whether produced from the ocean or from the flame of the candle. Where, then, is this water which we get from a candle? I must anticipate a little, and tell you. It evidently comes, as to part of it, from the candle, but is it within the candle beforehand? No. It is not in the candle; and it is not in the air round about the candle which is necessary for its combustion. It is neither in one nor the other, but it comes from their conjoint action, a part from the candle, a part from the air; and this we have now to trace so that we may understand thoroughly what is the chemical history of a candle when we have it burning on our table. How shall we get at this? I myself know plenty of ways, but I want *you* to get at it from the association in your own minds of what I have already told you.

I think you can see a little in this way. We had just now the case of a substance which acted upon the water in the way that Sir Humphry Davy showed us, and which I am now going to recall to your minds again by making an experiment upon that dish. It is a thing which we have to handle very carefully, for you see if I bring a little splash of water near this mass it sets fire to part of it; and if it set fire to a part, and there was free access of air, it would set fire to the whole. Now this is a metal—a beautiful and bright metal—which rapidly changes in the air, and as you know, rapidly changes in water. I will put a piece on the water, and you see it burns beautifully, making a floating lamp, using the water in the place of air. Again, if we take a few iron filings or turnings and put them in water we find that they likewise change. They do not change so much as this potassium does, but they change somewhat in the same way, they become rusty, and show an action upon the water, though in a different degree of intensity, to what this beautiful metal does; but they act upon the water in the same manner generally as this potassium. I want you to unite these different facts together in your minds. I have another metal here, and when we examined it with regard to the solid substance produced from combustion we had an opportunity of seeing that it burnt; and I suppose, if I take a little strip of this zinc and put it over the candle, you will see something half way, as it were, between the combustion of potassium on the water and the action of iron,—you see there is a sort of combustion. It has burnt, leaving a white ash or residuum, and here also we find that that metal has a certain amount of action upon water.

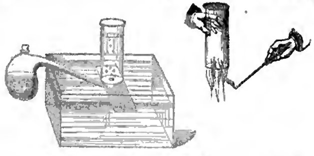
By degrees we have learned how to modify the action of these different substances, and to make them tell us what we want to know. And now, first of all, I take iron. It is a common thing in all chemical reactions, where we get any result of this kind, to find that it is increased by the action of heat; and if we want to examine minutely and carefully the action of bodies one upon another, we often have to refer to the action of heat. Now you know, I think, that iron filings burn beautifully in the air; and I am about to show you an experiment of this kind, because it will impress upon you what I am going to say about iron in its action on water. If I take a flame which I make hollow,—you know why, because I want to get air to it and in it, and therefore I make it hollow,—if I take a few iron filings, and drop them into the flame, you see how well they burn. That combustion proceeds from the chemical action which is going on when we ignite those particles. And so we proceed to consider these different effects, and ascertain what iron will do when it meets with water. It will tell us the story so beautifully, so gradually and regularly, that I think it will please you very much.

I have here a furnace with a pipe going through it like an iron gun-barrel, and I have stuffed that barrel full of bright iron turnings, and the part that is so stuffed is put into the fire and is made red-hot. We can either send air through the barrel to come in contact with the iron, or else we can send steam from this little boiler at the end of the barrel. Here is a stop-cock which shuts out the steam from the barrel until we wish to admit it. There is some water in these jars, which I have coloured blue so that you may see what happens. Now you know very well that any steam I might send through that barrel, if it went through into the water in the form of steam, would be condensed; for you have seen that steam cannot remain as steam if it be cooled down; you saw it here [pointing to the tin flask] crushing itself into a small bulk, and causing the flask containing it to collapse.



So that if I were to send steam through that barrel it would be condensed—supposing the barrel were cold,—it is, therefore, heated to perform the experiment I am now about to shew you. I am going to send the steam through the barrel in small quantities, and you shall judge for yourselves when you see it issue from the other end, whether it still remains steam. Steam is condensible into water, and when you lower the temperature of steam you convert it back into fluid water; but I have lowered the temperature of the gas which I have collected in this jar, by passing it through water after it has passed through the iron barrel, and still it does not change back into water. I will take another test and apply to this gas. (I hold the jar in an inverted position, or else I should lose my substance.) If I now apply a light to the mouth of the jar it ignites with a slight noise. That tells you that it is not steam, steam puts a fire out, it does not burn; but you saw that what I had in that jar burnt. We may obtain this substance equally from water produced from the candle flame as from any other source. When it is obtained by the action of the iron upon the aqueous vapour, it leaves the iron in a state very similar to that in which these filings were when they were burnt. It makes the iron heavier than it was before. So long as the iron remains in the tube and is heated, and is cooled again without the access of air or water, it does not change in its weight; but after having had this current of steam passed over it, it then comes out heavier than it was before, having taken something out of the steam, and having allowed something else to pass forth, which we see here. And now, as we have another jar full, I will show you something most interesting. It is a combustible gas; and I might at once take this jar and set fire to the contents, and show you that it is combustible; but I intend to show you more if I can. It is also a very light substance. Steam will condense; this body will rise in the air, and not condense. Suppose I take another glass jar, empty of all but air; if I examine it with a taper I shall find that it contains nothing but air. I will now take this jar full of the gas that I am speaking of, and deal with it as if it were a light body; I will hold both upside down, and turn the one up under the other; and that which contains, or did contain, the gas obtained from the steam, what does it contain now? You will find it now only contains air. But look! Here is the combustible substance [taking the other jar] which I have poured out of the one jar into the other. It still preserves its quality, and condition, and independence, and therefore is the more worthy of our consideration, as belonging to the products of a candle.

Now, this substance which we have just got by the action of iron on the steam or water, we can also get by means of those other things which you have already seen act so well upon the water. If I take a piece of potassium, and make the necessary arrangements, it will produce this gas; and if I take a piece of zinc, I find, when I come to examine it very carefully, that the main reason why this zinc cannot act upon the water continuously as the other metal does, is because the result of the action of the water envelopes the zinc in a kind of protecting coat. We have learned in consequence, that if we put into our vessel only the zinc and water, they, by themselves, do not give rise to much action; and we get no result. But suppose I proceed to dissolve off this varnish,—this encumbering substance,—which I can do by a little acid; the moment I do that I get the zinc acting upon the water, exactly as the iron did, but at the common temperature. The acid in no way is altered, except in its combination with the oxide of zinc, which is produced. I have now poured the acid into the glass, and you would think I was applying heat to cause this boiling up. There is something coming off from the zinc very abundantly, which is not steam. There is a jar full of it; and you will find that I have exactly the same combustible substance remaining in the vessel, when I hold it upside down, that I produced from the experiment with the iron barrel. This is what we get



from water, and this is the substance which is contained in the candle.

Let us connect these two points clearly and distinctly together. This is hydrogen—a body classed among those things which, in Chemistry, we call elements, because we can get nothing else out of them. A candle is not an elementary body, because we can get carbon out of it, we can get this hydrogen out of it, or at least out of the water which it supplies. And this gas has been so named hydrogen, because it is that element which, in association with another, generates water.[2] Mr. Anderson having now been able to get two or three jars of gas, we shall have a few experiments to make, and I want to show you the best way of making these experiments. I won't be afraid to show you, for I like you to make experiments, if you will only make them with care and attention, and the assent of those around you. As we advance in Chemistry we are obliged to deal with substances which are rather injurious if in their wrong places; the acids, and heat, and combustible things we use, might do harm if used carelessly. If you want to make hydrogen, you can make it easily from bits of zinc, and sulphuric, or muriatic acid. Here is what in former times was called the "philosopher's candle." It is a little phial with a cork and a tube or pipe passing through it. And I am now putting a few little pieces of zinc into it. This little instrument I am going to apply to a useful purpose in our demonstrations, for I want to show you that you can prepare hydrogen, and make some experiments with it as you please, at your own homes. Let me here tell you why I am so careful to fill this phial nearly, and yet not quite, full. I do it because the evolved gas which, as you have seen, is very combustible, is explosive to a considerable extent, when mixed with air, and might lead to harm if you were to apply a light to the end of that pipe before all the air had been swept out of the space above the water. I am now about to put in the sulphuric acid. I have used very little zinc and more sulphuric acid and water because I want to keep it at work for some time. I, therefore, take care in this way to modify the proportions of the ingredients so that I may have a regular supply,—not too quick, and not too slow. Supposing I now take a glass and put it upside down over the end of the tube, because the hydrogen is light I expect that it will remain in that vessel a little while. We will now test the contents of our glass to see if there be hydrogen in it,—I think I am safe in saying we have caught some [applying a light]. There it is, you see. I will now apply a light

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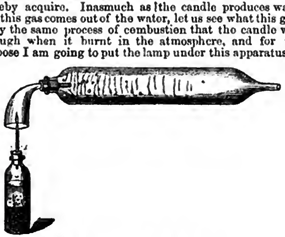
[2] ὑδωρ, "water," and γενναω, "I generate."

to the top of the tube. There is the hydrogen burning. There is our philosophical candle. It is a foolish, feeble sort of a flame, you may say, but it is so hot that scarcely any common flame gives out so much heat. There is the flame and the heat of the flame, and you see it goes on burning regularly, and I am now about to put that flame to burn under a certain arrangement in order that we may examine its results and make use of the information which we may thereby acquire. Inasmuch as the candle produces water, and this gas comes out of the water, let us see what this gives us by the same process of combustion that the candle went through when it burnt in the atmosphere, and for that purpose I am going to put the lamp under this apparatus, in order to condense whatever may arise from the combustion within it. In the course of a short time you will see moisture appearing here in the cylinder, and you will get the water running down the side, and the water from this hydrogen flame will have absolutely the same effect upon all our tests, being obtained by the same general process as in the former case. This hydrogen is a very beautiful substance. It is so light that it carries things up; it is far lighter than the atmosphere, and I dare say I can show you this by an experiment which, if you are very clever, some of you may even have skill enough to make. Here is our generator of hydrogen, and here are some soap-suds. I have an india-rubber tube connected with the hydrogen generator, and at the end of the tube is a tobacco-pipe. I can thus put the pipe into the suds and blow bubbles by means of the hydrogen. You observe how the bubles fall downwards when I blew them with my warm breath; but notice the difference when I blow them with hydrogen. [The Lecturer here blew bubbles with hydrogen, which rose to the roof of the theatre.] It shows you how light a thing this must be in order to carry with it not merely the ordinary soap-bubble, but the larger portion of a drop hanging to the bottom of it. I can show its lightness in a better way than this; larger bubbles than these may be so lifted up; indeed, in former times balloons used to be filled with this gas. Mr. Anderson will fasten this tube on to our generator, and we shall have a stream of hydrogen here with which we can charge this balloon made of collodion. I need not even be very careful to get all the air out, for I know the power of this gas to carry it up. [Two collodion balloons were inflated and sent up, one being held by a string.] Here is another larger one made of thin membrane, which we will fill and send up; you will see they will all remain floating about until the gas escapes.



What, then, are the comparative weights of these substances? I have a table here which will show you the proportion which their weights bear to each other. I have taken a pint and a cubic foot as the measures, and have placed opposite to them the respective figures. A pint measure of this hydrogen weighs three-quarters of our smallest weight, a grain, and a cubic foot weighs one-twelfth of an ounce; whereas a pint of water weighs 8750 grains, and a cubic foot of water weighs almost 1000 ounces. You, therefore, see what a vast difference there is between the weight of a cubic foot of water and a cubic foot of hydrogen.

Hydrogen gives rise to no substance that can become solid, either during combustion or afterwards as a product of its combustion; but when it burns it produces water only, and if we take a cold glass and put it over the flame of it, i tbecomes damp, and you have water produced immediately in abundance; and nothing is produced by its combustion but the same water which you have seen the flame of the candle produce. It is an important point for you to consider that this hydrogen is the only thing in the world that by combustion gives such a substance as this as its sole product.

And now we must endeavour to find out some fresh proof of the general character and composition of this water, and for this purpose I will keep you a little longer, so that at our next meeting we may be better prepared for the subject. We have the power of arranging the zinc which you have seen acting upon the water by the assistance of an acid, in such a manner as to cause all the power to be evolved in the place where we require it. I have behind me a voltaic pile, and I am just about to show you at the end of this lecture, its character and power, that you may see what we shall have to deal with when we next meet. I hold here the extremities of the wires which transport the power from behind me, and which I shall use to act on the water.

We have previously seen what a power of combustion is possessed by the potassium, or the zinc, or the iron filings; but none of them show such energy as this. [The Lecturer here made contact between the two terminal wires of the battery when a bright flash of light was produced.] This light is, in fact, produced by a forty-zinc power of burning, it is a power that I can carry about in my hands through these wires at pleasure, although if I applied it wrongly to myself it would destroy me in an instant, for it is a most intense thing, and the power you see here put forth while you count five [bringing the poles in contact and exhibiting the electric light] is equivalent to the power of several thunder-storms, so great is its force. And that you may see what intense energy it has I will take the ends of the wires which convey the power from the battery behind me, and with it I dare say I can burn this iron file. Now this is a chemical power, and one which, when we next meet, I shall apply to water, and show you what results we are able to get.

[The Fourth Lecture will appear in our next Number.]

NOTICES OF BOOKS.

Etude sur l'Origine du Diamant. By M. D. Rossi, of Toulon. (Reprinted from the *Bulletin de la Société Scientifique de Draguignan.*) Draguignan: P. Grimbert. 1860.

This is an interesting little work upon the diamond, considered in a scientific point of view, and more especially as to its chemical origin in Nature.

In the first place, the author exposes the opinions of ancient and modern philosophers regarding the nature of the diamond. The opinion of Pliny, who considered it as *pure water solidified*, and that of Linnæus, who called it *alumen lapidosum, pellucidissimum, solidissimum, hyalinum*, will suffice as examples of the extent of knowledge possessed by our ancestors on the nature of this precious stone.

When in later days it was found, by direct experiment, that the diamond was pure, crystallised carbon, it became interesting to endeavour to produce it artificially, and to account for its formation in Nature. Concerning the latter point, M. D. Rossi endeavours to refute the opinion professed by the late M. D'Orbigny, that "the diamond appears evidently to be the result of a crystalline transformation of vegetable *detritus*;" and to prove from geological and other deductions that it has been formed before any plants existed, by means of the natural carburets of hydrogen or other compounds of carbon existing on the surface of the globe.

He brings forward arguments, also, to prove that the diamond was not formed by fusion, remarking that we have never been able to fuse carbon, and by applying much heat, have only transformed it into graphite. According to our author, Sir David Brewster has shown that diamonds sometimes contain small cavities filled with gas.

On the other hand, M. Rossi details many experiments made with the view of forming the diamond artificially. Among them are some that certainly deserve repeating:—In favour of his opinion that the diamond may have resulted from the decomposition of some carbon compound existing before plants appeared upon the globe, M. Rossi cites an experiment made by Mr. J. Joyce, in which the latter endeavoured to fire a mixture of carbonic acid and hydrogen by the electric spark. It was supposed that the oxygen of the carbonic acid would unite with the hydrogen and deposit pure carbon, perhaps in a crystalline state. "At the end of one operation," says M. Rossi, "it was imagined that diamonds could be distinguished with a magnifying-glass."

Many other experiments made with the same hopes turned out unsuccessful. Indeed, the late M. Cagniard de la Tour thought he had obtained some small, flat crystals of diamond by fusion, and M. Despretz by the action of electricity upon certain compounds of carbon. But M. Rossi brings forward another experiment in which diamonds appear really to have been picked up after the operation. It was performed by M. Gannal. Eight ounces of water and eight ounces of sulphide of carbon were introduced into a flask, and eight ounces of phosphorus in sticks were added; in four months' time some crystals were formed, and separated by pressing the liquid through chamois leather. The crystals (twenty in number) thus obtained, when submitted to examination, were found to scratch steel; no metal could act upon them; they were perfectly transparent, and possessed much brilliancy; finally, some of them appeared crystallised in dodecahedrons. They were not submitted to chemical analysis.

We should not do justice to M. Rossi's little work if we did not mention that it contains more than its title seems to indicate; the author having included an historical sketch of the diamond, and some remarks upon its physical and chemical properties, which well deserve perusal by those who feel interested in this mineral.

CORRESPONDENCE.

Restoration of Exhausted Manganese.

To the Editor of the Chemical News.

Sir,—My attention has been drawn to an enquiry, in a late number of the Chemical News, as to whether any practical means exist of recovering manganese.

Messrs. Tennant's mode has been alluded to, but there is also another patented by myself; the practical operation having been carried on for a whole summer.

The most marked features in my process are, the absence of expensive machinery; the economy of the operations; and an oxide of manganese produced containing 52 per cent. of peroxide—the same if required can be brought up to 70 per cent. of peroxide. Both of these products are almost free from iron.—I am, &c.

Edward Haeffely.

Farnworth, near Manchester.

Action of Iodine on Turpentine.

To the Editor of the Chemical News.

Sir,—Your correspondent, Mr. Dearden, is in error in supposing that no mention is made in Chemical works, of the action of iodine upon oil of turpentine; if he will refer to "Slater's Hand-book of Analysis," page 225, he will find a notice upon the subject.—I am, &c.

F. P.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Hydrogen, Oxygen, and Chlorate of Potash on Perchloride of Phosphorus.—When pure, dry hydrogen, says M. Baudrimont (*Comptes Rendus*, t. li. p. 823), is passed through a glass tube heated to redness, into which the vapour of perchloride of phosphorus is passed at the same time, hydrochloric acid gas and liquid protochloride of phosphorus are formed. But the action of the hydrogen does not end there: ordinary phosphorus is set at liberty, which becomes converted into the red amorphous, and a gas is disengaged which burns with a beautiful emerald green flame, and produces white vapours of phosphoric acid. This gas appears to be a mixture of phosphuretted hydrogen with an excess of free hydrogen. Although not spontaneously inflammable, it fumes in the air as soon as formed, and soon deposits a light yellowish film which appears to be solid phosphide of hydrogen. The author thinks that the hydrogen acts at once on both the chlorine and phosphorus of the PCl_5.

At a dull red heat pure and dry oxygen also acts on vapour of PCl_5, and during the reaction a beautiful phosphorescent light illuminates the tube. Considerable quantities of chlorine are evolved, and the tube becomes covered with downy flocculi of anhydrous phosphoric acid, while a liquid, coloured yellow by chlorine, condenses in the receiver. This liquid shaken up with mercury, and then distilled, is obtained colourless. It boils at 110°. It is decomposed by water, giving hydrochloric and phosphoric, without a trace of phosphorous acid; it is then chloroxide of phosphorus, PCl_3O_2. The author considers the foregoing reaction the first example of the direct substitution of free oxygen for combined chlorine.

When three equivalents of PCl_5, and one equivalent of $KO\ ClO_5$, both in powder, are mixed together, the mixture liquefies immediately, and the temperature rises a good deal. At the same time hypochlorous, or hypochloric acid gas, is evolved, which often explodes, but soon chlorine is produced. What remains in the vessel is pure chloride of potassium (quite free from phosphate) suspended in a liquid formed of the chloroxide of phosphorus holding chlorine in solution. The change is represented by the following equation:—

$$3\,(PCl_5) + KO,\ ClO_5 = 3\,(PCl_3O_2) + KCl + 6\,Cl.$$

The formation of the explosive gas the author supposes to depend on the action of moisture on the PCl_5, by which hydrochloric and phosphoric acids are formed, which act first on the chlorate of potash. The action of PCl_5 on $KOClO_5$, the author considers another example of the substitution of oxygen for chlorine.

Biniodide of Potassium.—The same author (*Ibid.* p. 827) dissolved an equivalent of iodine in iodide of potassium, which, it is generally admitted, forms a biniodide of potassium. He then shook up the solution with sulphide of carbon, and found that the latter removed all the iodine, and left a colourless solution of KI. The removal of the iodine by a simple solvent, the author thinks, proves that it is not combined with the KI, but only held in solution.

II. ORGANIC CHEMISTRY.

New Vegetable Principle.—M. Ad. Chatin announces (*Ibid.* p. 810) the discovery of a colourless, neutral, nitrogenised and non-coagulable principle, which he says exists in a notable proportion in all vegetable tissues in process of formation, and which for the present he designates A. This matter exists in all vegetables, and is held in solution in a juice distinctly acid. Most vegetable acids and dilute mineral acids preserve it from change, but it is quickly turned brown by alkalies. M. Chatin was led to the discovery by observing that certain tissues which are always colourless in a fresh plant, are regularly coloured brown in old specimens; and that tissues in process of formation, and those which take the most active part in the phenomena of vegetation, show the colouration. He supposed, therefore, that the nourishing juices of vegetables must contain a principle which is colourless in living tissues, but which, on the death of the organs, and from other causes, undergoes a change indicated by the alteration of the colour, and is the exclusive cause of the brown appearance of autumnal and dead leaves in general. A, after it has turned brown, may be extracted from autumn leaves by treating them with a mixture of ether and water, the former of which will dissolve the green matter and the latter the brown, but the author does not tell us how we may separate the original colourless A.

Oxalate of Peroxide of Iron.—Dr. Phipson (*Ibid.* p. 831) forms the above salt by dissolving ferric hydrate in a boiling solution of oxalic acid. By evaporation, emerald green oblique prismatic crystals are obtained, which are very soluble in water. When exposed to solar light, they blacken like chloride of silver; and, if they are then treated with water, they dissolve with a crackling noise, and leave a deposit of yellow oxalate of the protoxide. By a prolonged exposure to light all the green persalt is decomposed. The green crystals lose five equivalents of water at 100° C., and turn white; when decomposed by heat they lose ten more equivalents of water; for one equivalent of the peroxide they contain five equivalents of oxalic acid, hence their composition may be represented by the formula,—

$$Fe_2O_3\,5C_2O_3 + 10HO + 5Aq.$$

Decolorisation of Iodide of Starch by Heat.—This, says M. Baudrimont (*Ibid.* p. 827), depends upon the volatilisation of the iodine, and the re-coloration of the liquid on cooling is caused by the ré-solution of the vapour of iodine which will remain on the surface. This is proved by heating some iodide in an open capsule, and blowing on the surface, after which the solution does not become coloured again on cooling. But if the iodide be boiled in a small flask, from which the vapour cannot so easily escape, the solution does become coloured on cooling. Further, when heated in a sealed tube, without air, the solution does not decolorise.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered at the Royal Institution during the current week:—Tuesday, January 22, at 3 o'clock, Professor Owen "On Fishes." Thursday, January 24, at 3 o'clock, Professor Tyndall, "On Electricity." Friday, January 25, at 8 o'clock, Dr. G. C. Wallich "On the Nature of the Deep-sea Bed and the Presence of Animal Life at Vast Depths in the Ocean." Saturday, January 26, at 3 o'clock, Professor Frankland "On Inorganic Chemistry."

How to Keep the Water in Burst Pipes.—We suppose that many, probably most, of our readers have already had experience of the annoyance caused by the bursting of the water-pipes during the short interval of the last thaw, and are anticipating a similar inundation when the next comes. Our own experience of the evil has been of rather a severe kind. On one occasion the cistern exhausted itself wholly, and another partially, into our laboratory, and the damage caused led us to consider whether something could not be done to prevent the recurrence of a similar catastrophe, and the device we have adopted is one which it may be of use to some of our readers to be made acquainted with. Having ascertained the points at which the pipes showed signs of weakness, we caused the fractured portions to be cut away, and in place of these we substituted a piece of vulcanised india-rubber tubing, the diameter of the hollow core being a little less than the outer diameter of the lead pipe. The orifice being enlarged by stretching, about an inch and a-half of the end of the lead pipe was inserted, a little copper-wire binding caused the tube to embrace the pipe with such force that there was no risk of the pressure of the water forcing it off when the ice melted. The same operation being performed with the other end we had a continuous pipe, and can wait with tranquillity until the plumber shall be sufficiently at liberty to restore it in a more permanent manner; and in the meanwhile having nothing to fear from the spontaneous irruption of the cool element into our house through irregular channels, or of our supply being cut off. With the view of preventing the future bursting of the principal pipes exposed to the cold, we propose to have the lead ones removed, and strong vulcanised india-rubber ones substituted for them. We need hardly say that for the frost to burst this is out of the question, since the elasticity of the material will yield to the expansive force of the water passing into the condition of ice. Whether such a substitution will be found to answer in a pecuniary point of view remains to be proved; but the difference must of necessity be trifling either way. Moreover, we have the satisfaction of knowing that we cannot have property of ten times its value, destroyed through its bursting—a contingency to which we should be liable at every thaw if we retained the old lead pipe.

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

⁂ All *Editorial Communications* are to be addressed to Mr. Crookes and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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D. Logan.—The numbers were sent by post as soon as the letter arrived.

Ozonized Cod-liver Oil.—A correspondent wishes to be referred to any article on, or account of, the preparation, nature, or properties of ozonized cod-liver oil

G. F. R.—The subject of your paper is so purely theoretical, and unsupported by experiment, that we think it would be scarcely applicable for insertion in our columns. Amber-dust or fragments can readily be disposed of at the varnish-makers', amber varnish being largely used in photography.

F. C. H.—Received with thanks.

Books, &c., Received.—"Illustrated Catalogue of Chemical Apparatus," J. J. Griffin, Bunhill-row. "Infant Alimentation; or, Artificial Feeding, as a Substitute for Breast-milk, considered in its Physical and Social Aspects." By M. A. Baines. A paper from the *Lancet* of January 12, 1861.

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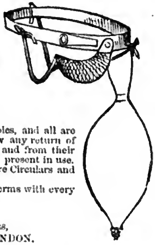

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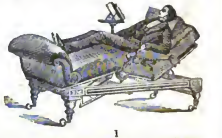

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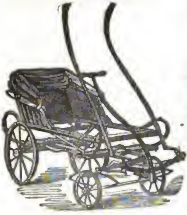

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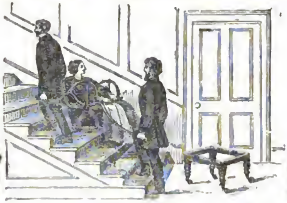

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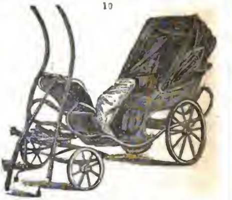

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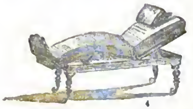

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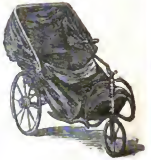

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THE CHEMICAL NEWS

Vol. III. No. 60.—January 26, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

An Account of Guy's Hospital Well, by William Odling, *M.B., F.R.S.*

(*Continued from page* 37.)

Some of the constituents of these waters are highly interesting in a scientific point of view. The presence of potash in deep-well water was first ascertained in the case of the Artesian water of Grenelle, near Paris, which, moreover, is quite free from soda salts. The fixed constituents in a gallon of this water, as estimated by M. Payen,[1] are shown below:—

Constituents.	Grains per Gallon.
Chloride of potassium	0·763
Bicarbonate of potash	2·072
Sulphate of potash	0·860
Carbonate of lime	4·760
Carbonate of magnesia	0·994
Silica	0·399
Organic matter	0·015
Total residue	9·863

Mr. Graham was unable to detect potash in Messrs. Combe and Delafield's well, nor could Mr. Brande recognise its presence in the water of the Mint. It has, however, been found in the Trafalgar-square water, by Messrs. Abel and Rowney, by Mr. D. Campbell, and by Dr. Dupré and myself. Moreover, potash has been detected by several chemists in various other deep well waters of the London basin; there was no difficulty in recognising its existence in the Guy's Hospital water. I am quite unable to account for the irregularity of its presence in waters derived from the same strata, and at situations comparatively near to one another. It does not appear to depend upon the different depths of the borings.

The presence of phosphoric acid in deep-well water was first ascertained by Mr. Graham in his examination of the water of Messrs. Combe and Delafield's well in 1845. This acid has since been invariably found, when sought for, in the deep waters of London. Its presence is not surprising, for phosphoric acid is a compound of which minute quantities are very widely distributed in nature. It exists in the deep-well waters in variable, but always in very small, proportions. The amount of phosphoric acid which I found in Guy's Hospital water is considerably less than that which other chemists have found in the waters of other deep wells. The estimation, however, was made very carefully, and corroborated in several ways. When nitrate of silver is added to an aqueous solution of the fused residue of an evaporated deep-well water, an abundant precipitate is produced, the yellow colour of which is at times very decided. This yellow colour has been considered to result from the phosphoric acid existing in the water. In my opinion, the quantity of phosphoric acid present is, in no case, anything like sufficient to account for the coloration, which, I believe, is really dependent upon the carbonate of soda. In adding nitrate of silver solution to the solution of an alkaline carbonate, whenever the drop of nitrate of silver is temporarily in excess, we always obtain a yellow precipitate, as is, indeed, well known. Phosphoric acid is also found in the water of shallow wells, or land-springs of the London gravel; but, in this case, it is, doubtless, derived from contamination of the water with products of animal decomposition, and is associated with obvious quantities of other compounds of similar origin,—ammonia, nitric acid, &c.

The source of the carbonate of soda which exists so uniformly, and to so considerable an extent, in the deep waters of London, has been the subject of much speculation, and is even now undecided. The general opinion at one time entertained was, that it resulted from the admixture, and mutual reaction, of infiltrated sea water, rich in soda salts, with proper chalk water, containing much bicarbonate of lime. The principal arguments in favour of this view were considered to be the following:—Water from chalk wells, situated above the level of Trinity high-water mark,—the Watford water for instance,—is comparatively free from soda salts, which exist so largely in sea water; whereas, these salts are found to a considerable extent in the water of chalk wells descending greatly below high-water mark, such as the deep-well water of the London basin. Again, the high chalk water, though poor in soda salts, is rich in carbonate of lime, whereas, the deep chalk water is comparatively rich in soda salts, and poor in carbonate of lime; and this difference in character has been thought to indicate mutual decomposition of the soda salts of the sea, with the carbonate of lime of the proper chalk water, whereby carbonate of soda is produced in the deep-well water. Moreover, it has been stated that the proportion of soda salts in the deep-well water of London was gradually increasing, a circumstance which, it was conceived, could scarcely occur unless that water had some communication with the sea. The above view has been controverted, and, as I think, successfully, by Mr. Dugald Campbell.[2] He has shown that the alleged increase of soda salts in the deep-well water is a mistake; that the shallow chalk water contains an obvious amount of nitric acid which is not to be found in the deep-well water: and finally, that no admixture of proper chalk water and sea water could account for the composition of the deep-well water as a whole. The following table of the principal constituents of the different waters, calculated, No. I. from Mr. D. Campbell's analysis of the Watford water; No. II. from my own analysis of the Guy's Hospital water; and No. III. from Mr. Schweitzer's analysis of the water of the English

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[1] *Annales de Chemie et de Physique*, 3ème sér., t. i. p. 381.

[2] *Quarterly Journal of the Chemical Society*, vol. ix. p. 22.

Channel, illustrate Mr. Campbell's position. If the sodium of the deep-well water, for example, had been derived wholly from the sea, it ought to have been associated with at least 25·63 grains, instead of with only 9·75 grains of chlorine:—

Constituents.	I.	II.	III.
Chlorine	0·84	9·75	1366·75
Sulphuric acid	0·39	9·24	164·99
Carbonic acid	8·00	6·90	1·01
Calcium	7·71	1·05	29·86
Magnesium	—	0·69	97·32
Sodium	0·77	16·61	744·69
Potassium	—	0·54	28·03

In reference also to the above view, it is worthy of observation that the deep chalk water of Southampton, which appears to be contaminated to a considerable extent with sea water, is entirely free from carbonate of soda.[1] In addition to other constituents of the sea, this water contains 62·8 grains of common salt in a gallon, whereas the Trafalgar Square water contains only 19·0, and the Guy's Hospital water only 16·0. Mr. Campbell has also shown the improbability of a modification of the above view, namely, that the deep-well water results from an admixture of the proper chalk water with sea water, which has undergone a change by percolating through an immense bed of chalk.

In the present imperfect state of our knowledge, the most probable explanation of the source of the carbonated alkali in the deep water of London, seems to me to consist in recognising the decomposition of an alkaline silicate, chiefly silicate of soda, existing in the strata above the chalk. The presence of carbonated alkali in the deep water of Grenelle, which is free from salts of the sea, and the presence of salts of the sea in the deep water of Southampton, which is free from carbonated alkali, seem to negative the idea that the existence of carbonated alkali in certain deep-well waters is dependent in any way upon contamination with sea water. That it may be derived from alkaline silicates is rendered probable by the following circumstances, namely, that alkaline silicates are among the most common constituents of minerals, and are known to suffer slow decomposition by the action of water containing carbonic acid, rain-water for example;[2] and that carbonate of soda is not confined to the deep-water from the chalk, but exists also in the water of the porous strata intervening between the London clay and the chalk. In a preceding part of this communication, the analyses of two waters from Mr. Burnett's well at Vauxhall are given. This well is so constructed, that the water of the gray sand above the chalk can be obtained separately from the water of the chalk itself. No. V. is the analysis of the chalk water, and No. VI. that of the sand water. It will be observed that the general characters of the two correspond closely, the principal difference being that the chalk water contains more carbonate of soda, and less sulphate of soda, than does the sand water. This is a difference which may be readily explained, by supposing the sulphate of soda of the sand water to undergo a partial decomposition with the bicarbonate of lime, which characterises a pure chalk water. Moreover, it is quite possible that the formation of carbonated alkali, begun by the decomposition of alkaline silicates in the plastic clay and gray sand, may be continued by the decomposition of alkaline silicates in the chalk. My results with the gray sand water are not singular; Mr. D. Campbell examined a specimen of well water from the plastic clay, and found it to correspond closely in its characters with deep-well water from the chalk. Messrs. Clarke and Medlock again, in their examination of the Westbourne Park well water, observe, "the almost complete identity of water from the sand and that from the chalk." I am inclined to think that the principal source of the deep-well water of London is rain water, a considerable proportion of which has drained through the highly porous sand formations beneath the mottled clay, into the chalk, which constitutes its principal reservoir. The porous eocene beds, intervening between the London clay and the chalk, have a very extensive superficial distribution for the reception of rainfall. On the south, they are found in Kent, at Woolwich, Blackheath, and Bromley, &c.; and in Surrey, beyond Croydon, and at the Addington Hills. To the north of London, they appear at the surface in parts of Hertfordshire, and of Middlesex, particularly in the neighbourhood of Bushey. This view of the source of the carbonate of soda in deep-well water does not account in any way for the presence of the chloride of sodium. It is not contended, however, that the well water is entirely free from sea-contamination, but only that sea-contamination is not the cause of its alkalinity. From the fact of portions of the chalk, and of the much more porous strata above it, cropping out as we proceed eastwards along the course of the Thames to the sea, it seems not improbable that the deep water of London should be contaminated to some extent with sea water. The deep water of the Paris basin, indeed, is free from sea salt; but then the direct distance of the chalk under Paris from the sea, the altered relations to the sea of the littoral chalk itself, which, in some parts eastwards, rests upon a bed of Kimmeridge clay, forming a portion of the sea-bottom, and the non-communication of any porous strata above the chalk with the sea, render the case of the Paris basin very different from that of London, as regards its liability to sea-contamination. Moreover, at Grenelle, the boring has pierced through the chalk, and entered the lower green-sand formation, from which, I believe, the supply of water is principally derived.

On the Detection of Phosphorus,
by Dr. FRED. HOFFMAN.

THE author has instituted a series of more than 150 experiments with Mitscherlich's method for detecting phosphorus. He has mixed the phosphoric mass of matches in different preparations, with food cooked in various ways, with beverages commonly used, and with medicines in a liquid, soft, and plastic state, and kept the mixtures for a variable length of time. The presence of phosphorus, it will be remembered, is established by the phosphorescence imparted to the aqueous vapours on distilling the liquid or liquified mixture, and the distillation of the phosphorus is increased by the addition of sulphuric acid, chloride of sodium, sugar, &c., whereby the boiling-point is made to approach that of phosphorus.

In the place of Mitscherlich's apparatus, the author has constructed a more simple one, which is easily prepared and readily taken apart. It consists of an ordinary flask, connected with a receiving-bottle by means of a glass tube, which passes about 18 inches through a glass cylinder filled with cold water. A long, straight tube conducts the gaseous products from the bottle. The lamp and flask are surrounded with dark paper, and about three-fourths of the glass cylinder. The operation is best performed in a dark room.

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[1] Robson, *Quarterly Journal of the Chemical Society*, vol. iv. p. 7.

[2] A portion of the sand stratum overlying the chalk, obtained from the borings of Guy's Hospital well, was found to contain a considerable proportion of a readily-decomposible silicate of soda.

The phosphorescence of the liquid increases in intensity with the consistence of the liquid and the quantity of the phosphorus. The gas-bubbles are luminous, rise in the mixture, and apparently burn upon its surface with a bright flame. With the temperature the light is increased: a photosphere fills the flask, rises in the tube, and moves up and down within the cooled part. Sometimes only a column or a luminous ring appears stationary at the point where the vapours are cooled, and a luminous fog or sparks gradually sink into the receiver, or a sudden, frequently-repeated lightning is observed. If the heat is raised too high, or the cooling is insufficient, the luminescence passes through the long gas-tube, at the mouth of which the gases take fire, if the volatile oils from cruciferæ (mustard, &c.) have been present.

Coffee, mustard, smoked meat, highly-seasoned food and beverages, and medicines containing odorous gum-resins, volatile oils, musk, castor, camphor, chlorine, &c., have the property of covering the odour of a small portion of phosphorus; minute quantities, which yet produce the symptoms of gastro-enteritis, may not be recognised by the odour in the contents of the stomach. The reaction is not prevented, except by the presence of much alcohol, volatile oils, and mustard. If the quantity of phosphorus be not too insignificant, the phosphorescence is observed, either momentary or constant, at the beginning of the distillation, and after the alcohol has passed over.

The reaction is not interfered with by the presence of ipecacuanha, tartar emetic, magnesia, hydrated oxide of iron, musk, castor, opium, albumen, neutral acid, or basic salts and double salts, volatile organic acids, chlorides, iodides and sulphides, and by free acids; but iodine, chloride and bichloride of mercury in considerable proportion, and metallic sulphides in the presence of free sulphuric acid, and particularly oleum cinæ (artemisiæ), interfere with or prevent the reaction.

Numerous experiments, by distilling the brain of various animals, blood, albumen, casein, fibrin, legumin, and other protein compounds, with diluted sulphuric acid, yielded not the least photospheric reaction.—*Archiv. de Pharm.*

On Combinations of Iodine with Iodide of Potassium, by B. Piffard.

Having been lately engaged in making a series of experiments for the purpose of determining whether iodine, when dissolved in a solution of iodide of potassium, is *simply held in solution* or *chemically combined with it;* and having come to the conclusion that it is a definite chemical compound, a short statement of some of the principal facts I have arrived at may not be out of place, as I see that an opposite view is occasionally held, from some remarks I read in the Chemical News for January 16, 1861.

In the first place, I prepared a saturated solution of iodide of potassium, and in it dissolved as much iodine as it would hold, which, according to Baup, is two equivalents;[1] and I also prepared another similar solution, and to it added several times its bulk of water, which precipitates one equivalent of iodine. From these two solutions I then precipitated iodide of lead, by means of the acetate of lead.

Now, these iodides of lead, prepared in this manner, neither resembled one another nor the ordinary iodide of lead (PbI); for that precipitated from the first-named solution speedily turned from yellow to a deep-vermilion colour, and that from the second solution, after going through various shades of yellow, green, and blue, finally became nearly black. Both of these precipitates I separated from the liquid and dried without their undergoing any further change. This black iodide of lead has the following properties:—

1. It does not part from iodine at a temperature of 160° F., when even long continued—I have not tried a higher temperature—although uncombined iodine rises in vapour at ordinary temperature.
2. It does not part with its iodine to chloride of ammonium, nitrate of ammonia, or chloroform, in all of which uncombined iodine is soluble.

In the second place, I prepared a saturated solution of bromide of potassium, saturating it afterwards with bromine, and from it, with acetate of lead, precipitated a brilliant yellow bromide of lead; and from a similar solution of bromine in bromide of potassium, after diluting it with water, I precipitated with the acetate of lead, as before, a brickdust-coloured bromide of lead.

In the third and last place, through a saturated solution of chloride of potassium in water I passed a stream of chlorine gas, which was rapidly absorbed, and from it, when saturated, I precipitated with acetate of lead a reddish-brown chloride of lead.

By precipitating from a diluted solution of the above I did not find, however, that the colour of the precipitate was changed.

Now, I think that in all probability my two iodides of lead are different and definite combinations of iodine and lead, and that my bromides are also different and definite combinations of bromine and lead. Assuming this to be the case, and also that they are not produced by the action of free iodine and bromine on the ordinary iodide and bromide of lead,—which, after repeated attempts, I have not succeeded in doing,—I think I may safely come to the two following conclusions:—

1. That iodine and bromine combine respectively with iodide and bromide of potassium, with the aid of water.
2. That they each combine in two definite proportions.

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[1] See Graham's "Elements of Chemistry," vol. i. p. 529.

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc, by Frank H. Storer.

(Continued from page 38.)

Now, since many of the impurities of copper are eliminated when it unites with zinc, being thrown up on the surface of the melted metal as a crust or scum, which is removed by the workmen,[1] and as no two samples of copper contain the same kind or quantity of impurity, the amount of it removed, or, what is the same thing, the amount of copper left to unite with a given weight of zinc,[2] must continually vary.

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[1] This explains the remark of Karsten (*loc. cit.* s. 386), that perfectly pure copper can take up from 1·5 to 2·5 per cent. more zinc than impure, and still afford a product of better colour, more tenacious, and more malleable.

[2] Much zinc also is lost in this case, both by alloying with the foreign metal and by uniting with any oxygen which may have been combined with the latter. As the workmen say, impure copper "burns up" a great deal of zinc.

From this it may easily be seen how very difficult, if not impracticable, it would be to obtain anything like constant results, unless some test were found by which the workman might ascertain, at least approximately, the composition of his alloy.[3]

That it is of the first importance that every alloy used for sheathing shall possess some one constant composition, so that it may endure equally well on all parts of the vessel, and that no galvanic action may occur between the metal of different sheets, is a point too obvious to be mentioned.

It might still be urged against yellow-metal, that the admixture of other metals—of kinds capable of mingling with it—which may have been derived from the copper used in its preparation, is a serious objection to its use. This is true, and the remark applies with equal or even greater force to all the alloys used for sheathing; it must always depend upon the condition of copper metallurgy. It would apply more forcibly, for example, to the low-grade brass which Bobierre has proposed to substitute for the ordinary sheathing-metal; for since the "test" by means of which founders are enabled to prepare the alloy (yellow-metal) of constant composition cannot be obtained if metals are present which are incapable of mixing in all proportions with this alloy, in which event small particles of the foreign substances would be found irregularly disseminated upon the fractured surface of the test ingot, manufacturers are enabled to ascertain at once whether or no a given sample of copper is suitable for the preparation of yellow-metal. Whenever it is found that satisfactory tests cannot be obtained, the copper is immediately rejected by the founders, and applied to some other use. No similar means of controlling the purity of the brass in question are known.

It does not appear—at all events, it has never been proved—that any serious injury results in practice from the use of the most varied kinds of copper, so long as they afford the desired *homogeneous* alloy containing 40 per cent. of zinc.

I do not wish to deny that hurtful impurities may at times occur in yellow-metal, in spite of the test to which I have so frequently alluded. Most probably, the very rare cases in which this alloy wears out irregularly, portions of the sheet being much corroded, while other parts are scarcely at all acted upon and have preserved their original colour and malleability, may be referred to the irregular diffusion of such impurities throughout the mass of alloy of which the sheet was formed;[4] but their influence must be regarded as being of very slight importance in comparison with the changes of structure which have already been discussed.

The following is a list of the alloys which I have prepared. Crystals were obtained from all of them, with the exception, perhaps, of one or two of those lowest in the series, where the tendency to assume a pasty state during solidification renders the crystallisation obscure.

The amount of copper contained in both the crystals and the portion of fluid alloy poured off from above them was determined directly by assay in each instance. The copper used in the preparation of the alloys was a very pure article from Lake Superior; it contained only a certain amount of suboxide of copper and traces of silver.

The zinc was from La Vieille Montaigne, containing as its principal impurity a small amount of lead. Both metals were granulated for convenience in weighing. The lead and other impurities have been neglected in stating my results.

Number of the Experiment.	Per cent. of Copper put into the Alloy.	Per cent. of Copper found in the Crystals.	Per cent. of Copper found in the portion of Alloy poured off.
1	97·38	99·14	98·68
2	96·50	95·57	97·40
3	94·38	95·06	95·04
4	94·38	94·30	93·82
5	91·38	90·77	91·49
6	88·38	88·86	88·78
7	85·38	86·10	85·74
8	79·38	83·72	83·61
9	82·38	82·89	82·52
10	79·38	80·34	80·73
11	76·38	77·32	78·92
12	73·38	75·03	74·27
13	74·38	75·71	74·94
14	72·38	74·52	73·61
15	70·00	71·48	71·68
16[5]	—	—	69·20
17	67·40	68·93	67·37
18	65·94	67·12	—
19[5]	—	66·94	—
20[6]	—	65·56	—
21[5]	—	—	62·30
22	60·29	61·16	61·99
23[6]	—	60·92	61.36
24[6]	—	60·77	60·88
25[7]	—	60·42	60·24
26	56 69	57·82	58·66
27	53·09	54·22	54·45
28	51·00	52·62	52·11
29	49·00	49·51	49·55
30	47·00	49·43	48·26
31	45·00	47·49	46·79
32	43·00	44·41	43·63
33[5]	—	43·64	44 01
34	41·00	41·48	42·19
35	39·00	38·57	38·77
36	37·00	38·09	37·71
37	35·00	35·33	36·71
38	33·00	32·98	32·79
39	27·31	29·67	29·07
40[5]	—	29·07	—

In determining the amounts of copper contained in these alloys, I have employed a method which has been used for a long time by many of the copper assayers of this country, by whom it is highly esteemed. It is essentially the old Swedish method[8] of precipitating the copper from its solution in acid, by means of metallic iron, with, however, several more modern modifications and improvements.[9] Since several of the details of this process have never, to my knowledge, appeared in chemical literature, I shall make no apology for describing it at length in this connection.

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[3] It must be well understood, however, that, as a rule, manufacturers of alloys of copper and zinc always use the best copper they can obtain, since it is generally more economical for them to do so. For the purer this metal is, so much the greater will be the total amount of alloy obtained by the use of a given weight of it, or, in other words, the loss, mentioned in the preceding notes, which would occur from the elimination of impurities, will be smaller.

[4] It is not impossible that the small amount of silver which, as is well known, is precipitated from sea-water upon the metallic sheathing of ships, can accumulate to such an extent in yellow-metal which has been repeatedly used and re melted, that the durability of the latter may be seriously impaired thereby. It is, however, equally probable, and perhaps more so, that the greater part of this silver is eliminated in the preparation of the alloy, when the old metal is fused with zinc.

[5] The instances in which the amounts of metal used in preparing the alloy are not given refer to crystallised products which were obtained accidentally during the course of the research.

[6] Yellow metal, from founder's pots, said, from appearance of test ingot, to require a little more zinc.

[7] Yellow-metal which had afforded a satisfactory test.

[8] *Vid.* Bergman, "Physical and Chemical Essays," translated from the original Latin by E. Cullen, M.D. (London, 1788), vol. ii. p. 443. Described more fully by Kersten, in Karsten u. von, Dechen's *Archir für Mineralogie, u. s. w.*, xii. 567; also by Kerl, in Bodemann's *Probirkunst* von Bruno Kerl (Clausthal, 1857), s. 216.

In choosing samples of the alloy for analysis, I have been careful to select those portions of the cup which were best crystallised, avoiding always the upper crust, which had lost zinc by volatilisation while cooling; from the sheets into which the fluid portion of the alloy was cast, I have taken pieces near the edge, which were free from any admixture of fragments of the crust.

(To be continued.)

On Coal Gas,[10] *by the Rev.* W. R. Bowditch.

A distinguished Fellow of the Royal Society discovered coal-gas, when Rector of Crofton, about two miles from my present parish, and nearly all our knowledge of this complex substance is derived from the labours of chemists who have been, or are, Fellows of the Society. I feel assured, therefore, that an attempt to extend the knowledge of the reaction of coal-gas with various substances will be favourably received, and that the application to practice of the facts made known, will not render a memoir less acceptable to the Society which rewarded alike the abstract researches of Leverrier and the practical ones of Arnott.

Six years ago I introduced the use of clay into gas-works, for the purpose of improving the purification of coal-gas, and now,—after so long an experience, the purification of many hundreds of millions of feet of gas, and the use of many thousand tons of the refuse as manure,—I venture, for the first time, to submit the grounds upon which my process is based.

Coal-gas may conveniently be considered under the heads of carbon compounds required for the production of heat and light, which generate water and carbonic acid by their combustion; and sulphur and nitrogen compounds which are not necessary for heat and light, and ought to be removed from gas on account of the injurious nature of the substances produced by their combustion.

The former of these classes will be treated of incidentally; the latter class forms the principal subject of this paper. When speaking of *gas*, I always refer to that which has undergone the ordinary condensation of gas-works, wherefore no mention is made of the complex compounds removed by condensation.

When coal is distilled, its nitrogen is evolved in some forms of combination which are generally familiar, while others are almost unsuspected. Under certain conditions of distillation, much nitrogen leaves the retort and passes the condenser as ammonia or some of its salts. These are all removed from gas by clay, so that no trace of ammonia can be discovered after gas has passed through purifiers charged with an adequate quantity of clay, and with lime or some equivalent substance to remove sulphide of hydrogen. Clay is thus entitled to be classed with acids and some metallic salts as a purifier of gas, for these, of course, remove ammonia and its salts. But between clay and acids there is an important difference, in regard to the action which takes place upon the most valuable light-giving constituents of the gas; acids remove a large quantity of these, clay does not. We have experimental proof that clay does not remove the valuable hydrocarbon vapours from gas, in the fact that strong spirit of wine digested upon foul clay for days, does not thereby become much more luminous than it was before being so treated. The very slight light-giving power which it has obtained is due to tar; for if the spirit be evaporated, and the tar so obtained be re-dissolved in fresh spirit, the same kind of flame will be obtained as before; whereas the addition of a small portion of coal-oil to spirit gives a flame of considerable illuminating power. To this I may add, that long and extensive experience shows that the employment of clay in the purifying process improves the light-giving power of gas, by removing substances which are not otherwise removed, and which, if allowed to be burnt with the gas, lessen its illuminating power. These light-damaging compounds are produced during the later portion of the distillatory process, as I have proved by experiment. The same retort was charged twice with the same weight of the same coal. The gas produced by one charge was purified by lime only, that produced by the other charge was purified by lime and clay. The illuminating power of the gas passing at each half-hour's end was determined, and it was found that the purification made no difference for the first three or four half-hours. About the middle of the charge, that purified by my process had slightly the advantage, and at the close the difference in favour of that purified by the addition of clay has been found as much as ten or twelve per cent. Thus it is shown that the compounds removed by clay from gas produced during the early stages of distillation—however objectionable on other accounts—do not lessen the light-giving power of gas, whereas those removed during the later periods of distillation reduce the light-giving powers considerably.

If conjecture be allowable, I would venture an opinion that cyanogen compounds, and other nitrogenised substances with which foul clay abounds, are those which lessen light. My own investigations lead directly to this inference, and, I think, explain an old Table by Dr. Henry in this sense. In the *Philosophical Transactions* for 1808, he shows that the gas produced from 112lbs. of Cannel-coal contained, after purification, the following quantities of nitrogen:—

Hours from commencement of distillation.	100 measures of purified gas contain measures of nitrogen	
½ an hour	20	Due chiefly to atmospheric air.
1 hour	4¾	Probably the time when ammonia was principally produced.
3 ,,	5	
5 ,,	15	Probably vapour of water was present in very small quantity, and cyanogen and related compounds were produced in increasing quantity.
7 ,,	15	
9 ,,	15	
10½ ,,	20	
12 ,,	20	

Without assuming the absolute accuracy of these figures, we may regard them as valuable indicators, pointing, I think, in the direction I have ventured to conjecture.

A beautiful reaction furnishes experimental proof of the damage done to gas by acids. Clean deal sawdust is well moistened with pure sulphuric acid, diluted with five or six volumes of water, so that the sawdust may not be discoloured, and gas is passed through it in a slow stream. With rich gases, which give the light of from 20 to 25 sperm candles for a consumption of five feet an hour, the sawdust instantly changes to a most beautiful pink colour, and the tint gradually deepens until the whole becomes of a dark mahogany. With poor gases, which give the light of from 10 to 12 candles, this coloration is exceedingly faint at first, and deepens very slowly. The differences of coloration are so considerable and constant, that I have no doubt of the possibility of thus determining the value of gas as an illuminant. By using a standard acid, the same kind of sawdust, a uniform volume of gas, and the same sized U-tubes, notation of time and depth of colour would give a close approximation to the illuminating value of the gas. At all events, the sources of error are not greater than those of photometry in the hands of all but the most experienced, and the process is quite as close an approximation to truth as an ultimate analysis of gas, containing, as it does, impurities which render skill and precaution useless. A comparison of the analysis of coal-gas given in "Bunsen's Gasometry," with the substances now known to exist in gas, will convince us that at present we cannot attach any value to such analyses.

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[9] For information concerning these I am indebted to Mr. G. J. Dickinson, chemist, of the copper works at Point Shirley, near Boston.

[10] From the *Proceedings of the Royal Society*, vol. xi. p. 25.

To determine the substances in gas which produce this coloration, some of its chief illuminating constituents were prepared and passed separately through the acid sawdust.

Olefiant gas made in the usual manner, and carefully purified, reddens the acid sawdust. Ether vapour does not affect it, and therefore need not be removed from the gas for this experiment.

Propylene, produced by passing the vapour of fusel-oil through a red-hot combustion-tube filled with cast-iron nails, but kept at so low a temperature that a small portion of oil passed over without decomposition, reddened the acid sawdust.

Commercial benzole, with the exception of one specimen, reddened the acid sawdust.

I have not yet had leisure to prepare and test acetylene.

The coloration of fir-wood, moistened by hydrochloric acid, has been mentioned by Williams as characteristic of pyrrole.

To show that the colour was produced by illuminating matter abstracted, some sawdust was treated with acid strong enough to char it slightly;[1] and gas, which instantly reddened the clean sawdust and dilute acid, was passed first through the black and then through the clean acid sawdust. No colour was produced in the latter, though the flow of gas continued for an hour.

Hydrochloric may be substituted for sulphuric acid, so far as that gas colours sawdust moistened with it; but it is liable to a considerable disadvantage. If gas contain ammonia, the vapour of the acid unites with it in the tube before the gas comes into contact with the sawdust; and the result is a deposit of chloride of ammonium on the surface of the sawdust where the colour commences, which renders the observation less precise and easy. Olefiant gas likewise does not redden this acid sawdust, and therefore cannot be estimated by it.

Nitrogenised compounds in coal-gas present the greatest difficulty in the way of efficient purification, and the almost impossibility of obtaining them in a state fit for examination, renders their investigation laborious and unsatisfactory. Much nitrogen is contained in gas as cyanogen, which can be separated from the clay used in purification. Probably not much less exists as sulphocyanogen, which can be separated from the foul clay with ease; and the presence of further quantities in combination with sulphuretted hydrocarbons and tar, can be demonstrated. The bodies formed by this combination of elements are, I believe, unknown at present.

By placing clay in a purifier through which crude gas passes from the condenser of a gas-works, and treating the saturated clay with spirit, a solution is obtained, of a brown colour, which has no effect upon litmus, turmeric, or lead paper, which decolorises a solution of iodine, and from which nitrate of silver throws down a white or brownish-white precipitate, and acetate of lead a white precipitate. The aqueous solution possesses the same properties, and, like the solution in spirit, is always neutral. Litmus paper, immersed in either of the solutions, and exposed to the air, becomes quickly, strongly, and permanently reddened. Soluble sulphides have been tested for repeatedly with nitroprusside of sodium, as well as with acetate of lead, but have never been found; yet a sulphur compound exists in solution which possesses the power of forming a sulphide with metallic mercury. The spirit solution, digested on mercury, with occasional shaking, produces the black sulphide of mercury, while the aqueous solution, similarly treated, produces the red sulphide. Insoluble sulphides, however, exist in the foul clay, and evolve sulphide of hydrogen on the addition of an acid. These insoluble sulphides are oxidised rapidly by exposure of the clay to atmospheric action.

A solution of clay in spirit was treated with an excess of powdered acetate of lead, and the white precipitate filtered off. The brown filtrate was supersaturated with ammonia and filtered. The clear brown filtrate, diluted with twelve times its bulk of water, became milky, and with much difficulty was obtained clear by filtration. Part of the spirit was then distilled off, to ascertain whether it would bring over a volatile sulphur compound, but the spirit was quite free from sulphur. The remaining fluid was then acidified with nitric acid, which caused brisk effervescence and a strong smell of hydrocyanic acid. Nitrate of silver was added as long as it continued to produce a precipitate; the precipitate, dried and heated, gave off cyanogen, which burnt with its characteristic flame. The clear filtrate, slowly evaporated to dryness, left a pale yellow crystalline mass, which did not change colour by several days' exposure to light. Part of this, burnt in a porcelain crucible, gave off nitrous fumes, and left a considerable residue blackened by oxide of silver. Water was added to this residue, and the oxide of silver filtered off, and an abundant precipitate of sulphate of baryta obtained, with a salt of baryta. The remainder of the yellow salt was re-dissolved in water, with a view to separate a granular portion which was mixed with the more perfectly crystalline salt, but an accident unfortunately spoilt the remainder, and rendered any further progress impossible.

(To be continued.)

PHARMACY, TOXICOLOGY, &c.

Report on Adulterations, by the American Pharmaceutical Association.

The Committee upon the subject of Home Adulterations, appointed at the annual meeting of this Association, held in Boston last year, respectfully report:—

The subject of Home Adulterations is one worthy of the careful consideration of every member of society, whether pharmaceutist, physician, or consumer,—and *all* belong to the latter class,—because of the pernicious and often dangerous results which ensue from the use of articles mixed with deleterious substances, or deficient in power, owing to inert substances used to enhance the profit of the unscrupulous tradesman.

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[1] This acid was of the same strength as that used in some gas-works.

Your Committee feel deeply the responsibility of their duty; they feel that much ought to be said and done which they dare not take the responsibility, as individuals, of saying and doing. They trust that the time will come when State legislators, or the General Government, will cause such penalties to be affixed to the selling of adulterated articles that the business will cease to "pay," and that "honesty" will be practised because it *is* "the best policy." During the past year your Committee have had many articles of drugs, medicines, and food submitted to their examination, and they have felt more and more the necessity of some means being devised for checking this abominable practice, which, in the language of another, "is undermining the very foundation of trade,—viz., faith in commercial integrity."

We refer to our published report in our last year's "Proceedings" for the definition of an adulteration as given by Dr. Hassall, of London, who has done more than any other to draw public attention to the alarming facts of this species of fraud. He says an adulteration consists in the intentional addition to an article, for the purpose of gain or deception, of any substance or substances, the presence of which is not acknowledged in the name under which the article is sold. The change of an article entirely, and selling it under a false name in place of another, constitutes a *substitution*, and not an adulteration.

Thus briefly referring to the introductory of our last year's report, we would say that all the views we there express we still sustain. We have, during the past year, seen nothing to cause us to change our opinion as to the dangerous and hurtful tendency of this unlawful practice; on the contrary, all our experiences confirm us in the views we there express, and tend to convince us more fully that the public do not know the extent to which they are cheated, nor would any intelligent community submit to the imposition were they once fully aware of the danger and risk they are exposed to.

Adulterations are practised for three principal reasons, as briefly stated by the author of "Falsification of Food:"—

1. For the purpose of making the substance more saleable, by improving its appearance by the addition of some body, either innocuous or otherwise.

2. To depreciate the quality by adding some substance which will diminish the *real*, without altering the *apparent* strength or general appearance. This is generally a very deadly fraud.

3. To depreciate quality and "extend" the quantity, by the addition of some simple substance, as water, or, if a solid body, as sand, gypsum, &c.

The matter of adulteration of articles of food is quite properly within the scope of a report on Home Adulterations; and your Committee propose entering somewhat largely into this part of the subject, trusting that the Association will grant them indulgence.

It is a sad and alarming fact that "death lurks in the pot;" that the community are exposed to injury, disease, and death, by the very food they live upon, and the tea, coffee, and water they drink. It is often urged in deference to many of these nefarious practices, such as the addition of some of the salts of copper and lead, or cocculus Indicus, to articles of daily consumption, that they are "used in *very small* quantities." We grant the statement to be true, and are deeply, we trust truly, grateful, as we ought to be, that we are not murdered outright, bu
inches.

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No substance, however
decidedly poisonous and p
producing, finally, disease,
physician can tell you of cases w
and lingering disease have been p
insidious poison. All of you are, d
disease often arises from the use of wa
pipes; and who can say that of the
"unknown disease," some of them may no
caused by poison taken in the small but o ced
doses of the daily food.

We are aware that many—and among them even those of scientific reputation—scout the idea of any trouble arising from water through lead; but your Committee feel that it is of such great importance that they cannot pass it by without a brief notice. It is a question upon which scientific men have differed; but, with the present knowledge of the subject that we have, your Committee can but recommend, in every practical case, the use of some other substance than lead for the conveyance of water to be used for drinking purposes.

Where so large a possibility of injury exists, prudence would dictate an entire avoidance of the danger.

One of your Committee has been fully convinced of the great danger attending the use of water through lead pipe. He has known lead to be taken from a portion of the liver of a person who died with every symptom of acute lead poisoning, and who was considered by the attending physician to be a victim of lead disease.

The susceptibility of individuals to this poison varies very greatly; many persons apparently suffer no inconvenience, although subjected to precisely the same exposure which proves fatal to others.

One of the most striking cases of extreme susceptibility to this species of poisoning which has come to the knowledge of your Committee is the following:—

A man, aged thirty-five, who was a house-servant in the city at the time of his sickness, had never been exposed to the influence of lead, as far as could be ascertained, previous to entering his place of service. He was employed by a private family, consisting of persons both older and younger than himself, and who had drank the water through the lead pipe for several years, in fact, ever since the introduction of the water into the city, without the slightest apparent injury. The man referred to was from the country, and perfectly healthy and strong upon his arrival in the city; he was a fleshy and robust man, and never had noticed any indications of paralysis. He had been, in his situation as servant, exposed to lead poisoning in no other way than drinking the same water that the family used, only three months, before he was attacked with violent symptoms of lead paralysis, without the premonitory symptoms of colic. He was completely paralysed, excepting the muscles of respiration and the heart. Even the muscles of the eye-lids were useless, and the "blue line" upon the gums was apparent, which is the well-known and usual attendant upon cases of lead-poisoning. Under treatment of iodide of potassium he completely recovered.

If such serious results *can* occur, is not the matter one worthy the attention of those who have in charge the public health?

The singular mode in which the lead is sometimes introduced into the system also calls for care and watchfulness, to prevent the unknown, and, therefore, unsuspected danger.

One of the members of your Committee was called upon by a physician who had a patient, a farmer, suffering apparently from lead poison, to make an analysis of some cider. From what source the lead was taken was a mystery. The water used by the family was all drawn by an old-fashioned "well-sweep" and bucket; no possibility seemed to exist that lead could be the cause, and yet all the symptoms were those of a plainly-marked case of lead-poisoning. The attending physician finally thought of the cider which the farmer was in the habit of drinking, and, as a matter of extreme precaution, decided to have a portion of it analysed. From less than two quarts of that cider, metallic lead was obtained in substance sufficiently large to hold in the hand and prove its nature beyond question. This caused an examination of the cask of cider, and from the bottom of the cask, upon draining off the cider, was taken a pint bowlful of mixed sediment and white lead, This, of course, readily accounted for the farmer's lead disease. The cask used had formerly been filled with oil, and, in putting it together, the edges of the staves had been plentifully coated with lead, ground in oil. before driving the hoops, to prevent the possibility of leakage; afterwards, when used for cider, the lead was dissolved out, and the serious consequences followed.

These instances, taken from many, are given to show that those who speak and feel warmly on this subject of lead-poisoning have some reason for their opinions and feelings.

We are aware that this is not strictly an adulteration, but certainly, after all that has been said and written upon this subject, those who, being aware of the facts in the case, and who could avert the danger, go on deliberately using lead pipes for drinking-waters, place themselves in the situation of the man who sets spring guns and gets shot himself. When lead pipes are used in preference because they are "cheaper" than gutta percha, block tin, or iron, does not *this* constitute an adulteration of the water?

We are lead to speak of this subject of lead-water, perhaps going beyond the limits which the Association have laid down for our Committee, by having met with and known many instances where sad trouble has been produced by this often despised cause.

The next article we would ask your attention to is milk. During the past year it has been the duty of one of your Committee to analyse a large number of specimens of milk. As many of you are aware, there exists a statute in some States regarding the sale of adulterated and unwholesome milk, and by virtue of that statute an Inspector of Milk is appointed by the authorities.

During the past year a large number of specimens have been analysed at the request of the Inspector of Milk for the city of Boston. With but one single exception, every specimen was found to be adulterated, many of them very largely.

The plan adopted has been to compare the amount of food contained in any milk examined with that which, by the analysis of a very large number of specimens of pure cow's milk, was found to be the *average* amount. The amount of food being ascertained in any given specimen, it is easy, by comparison, to find the relative worth of the sample to the standard, and thus ascertain the dilution with water.

The amount of milk-sugar and salts was also estimated, and the difference between the weight of the milk started with and the sum of the weights of the food, milk-sugar, and salts, gave the weight of the water in any sample analysed. The amount of water which any milk contained was not made the basis of comparison at all; the addition of water to milk increases in a much larger degree the worth of the milk as a food yielding fluid than it increases the *per centage* amount of water, and as the amount of *food* really constitutes the value of the milk, the analyses were placed on this alone.

Water is the principal adulteration of milk; but, to conceal this fraud and destroy the "skim milk" appearance, recourse must be had to several other adulterations, as the addition of colouring-matter, salt, &c., to increase the weight and improve the flavour, both of which it does, the latter quite remarkably.

Sometimes poor milk, from cows that should not be milked at all, owing to their condition. is mixed with that sent to market. It is only by microscopical examination that this unwholesome and unclean milk can be detected. Specimens of this kind have been met with during the past year.

From the experience one of your Committee has had, he does not hesitate to say that the adulteration of milk is largely and systematically carried on. Who can judge of the amount of sickness caused to children by the use of this unwholesome, and oftentimes poisonous food,—poisonous, because decomposition has already commenced.

Much of the sickness among young children in many large cities may be fairly attributed to the bad and unhealthy milk which they almost of necessity receive; and to the poorer classes, those less able by other reasons to contend with such a diet, the greater portion of this milk is given.

The milk oftentimes is not served to consumers until the second day after it is taken from the cow, owing to its being transported long distances, and when diluted with water the process of decomposition is much more rapidly set up.

On Santonate of Protoxide of Mercury,
by Dr. John Muller, *of Berlin.*

The solutions of equal parts of protonitrate of mercury and santonate of soda, in distilled water, are mixed; after 24 hours the precipitate is separated by a filter, well washed with distilled water, dried at a moderate heat, and preserved in bottles excluded from the light.

The protosantonate of mercury is a whitish powder, somewhat crystalline under a magnifier, inodorous, and of a slight metallic taste, which is afterwards bitterish and very persistent. It is insoluble in water and alcohol, unaltered at 212°, charred at a high temperature, and decomposed into mercury and red oxide of mercury; lime-water decomposes it into soluble santonate of lime and black oxide of mercury.

A solution of sulphide of potassium decomposes it at 212°, forming black sulphide of mercury and soluble santonate of potassa. A warm solution of iodide of potassium yields with it greenish iodide of mercury and santonate of potassa.

Exposed with alcohol to the temperature of boiling water, a turbidity is produced on the addition of water, but no precipitate. Thereby the salt is distinguished from santonin, which, under the same circumstances, is precipitated. When treated with diluted nitric acid, protonitrate of mercury is dissolved and pure santonin left behind.

Other acids show the same behaviour as nitric acid, and alkalies decompose it like lime-water.—*Arch. der Pharm.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. Faraday, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

Lecture IV. (Jan. 3, 1861.)

Products: Water from the Combustion—Nature of Water—A Compound—Hydrogen.

I see you are not tired of the candle yet, or I am sure you would not be interested in the subject in the way you are. When our candle was burning we found it produced water exactly like the water we have around us; and by further examination of this water we found in it that curious body, hydrogen—that light substance of which there is some in this jar. We afterwards saw the burning powers of that hydrogen, and that it produced water. And I think I introduced to your notice an apparatus which I very briefly said was an arrangement of chemical force, or power, or energy, so adjusted as to convey its power to us in these wires; and I said I should use that force to pull the water to pieces, to see what else there was in the water besides hydrogen; because, you remember, when we passed the water through the iron tube, we by no means got the weight of water back which we put in in the form of steam, though we had a very large quantity of gas evolved. We have now to see what is the other substance present. That you may understand the character and use of this instrument let us make an experiment or two. Let us put together, first of all, some substances, knowing what they are, and then see what that instrument does to them. There is some copper (observe the various changes which it can undergo), and here is some nitric acid, and you will find that this being a strong chemical agent will act very much when I add it to the copper. It is now sending forth a beautiful red vapour; but as we do not want that vapour, Mr. Anderson will hold it near the chimney for a short time, that we may have the use and beauty of the experiment without the annoyance. The copper which I have put into the flask will dissolve: it will change the acid and the water into a blue fluid containing copper and other things, and I purpose then showing you how this voltaic battery deals with it; and in the meantime we will arrange another kind of experiment for you to see what power it has. This is a substance which is to us like water—that is to say, it contains bodies which we do not know of as yet, as water contains a body which we do not know as yet. Now this solution of a salt I will put upon paper and spread about, and apply the power of the battery to it, and observe what will happen. Three or four important things will happen which we shall take advantage of. I place this wetted paper upon a sheet of tinfoil, which is convenient for keeping all clean, and also for the advantageous application of the power; and this solution, you see, is not at all affected by being put upon the paper or tinfoil, nor by anything else I have brought in contact with it yet, and which, therefore, is free to us to use as regards that instrument. But first let us see that our instrument is in order. Here are our wires. Let us see whether it is in the state in which it was last time. We can soon tell. As yet, when I bring them together, we have no power, because the conveyers—what we call the electrodes—the passages or ways for the electricity—are stopped; but now Mr. Anderson by that [referring to a sudden flash at the ends of the wires] has given me a telegram to say that it is ready. Before I begin our experiment I will get Mr. Anderson to break contact again at the battery behind me, and we will put a platinum wire across to connect the poles, and then if I find I can ignite a pretty good length of this wire we shall be safe in our experiment. Now you will see the power. [The connection was established, and the intermediate wire became red hot.] There is the power running beautifully through the wire, which I have made thin on purpose to show you that we have those powerful forces; and now, having that power we will proceed with it to the examination of water.

I have here two pieces of platinum, and if I lay them down upon this piece of paper [the moistened paper on the tinfoil] you will see no action; and if I take them up there is no change that you can see, but the arrangement remains just as it was before. But now see what happens: if I take these two poles and put either one or the other of them down separately on the platinum plates, they do nothing for me, both are perfectly without action; but if I let them both be in contact at the same moment, see what happens [a brown spot appeared under each pole of the battery]. Look here at the effect that takes place, and see how I have pulled something apart from the white—something brown; and I have no doubt, if I were to arrange this, and were to put one of the poles to the tinfoil on the other side of the paper, why, I got such a beautiful action upon the paper, that I am going to see whether I cannot write with it—a telegram if you please [the Lecturer here traced the word "juvenile" on the paper with one of the terminal wires]. See there how beautifully we can get our results.

You see we have here drawn something, which we have not known about before, out of this solution. Let us now take that flask from Mr. Anderson's hands, and see what we can draw out of that. This, you know, is a liquid which we have just made up from copper and nitric acid, whilst our other experiments were in hand, and though I am making this experiment very hastily, and may bungle a little, yet I prefer to let you see what I do rather than prepare it beforehand.

Now see what happens. These two platinum-plates are the two ends (or I will make them so immediately) of this apparatus; and I am about to put them in contact with that solution, just as we did a moment ago on the paper. It does not matter to us whether the solution be on the paper or whether it be in the jar, so long as we bring the ends of the apparatus to it. If I put the two platinums in by themselves they come out as clean and as white as they go in [inserting them into the fluid without connecting them with the battery]; but when we take the power and lay that on [the platinums were connected with the battery and again dipped into the solution], this, you see [exhibiting one of the platinums], is at once turned into copper, as it were; it has become like a plate of copper; and that [exhibiting the other piece of platinum] has come out quite clean. If I take this coppered piece and change sides, the copper will leave the right hand side and come over to the left side; what was before the coppered plate comes out clean, and the plate which was clean comes out coated with copper; and you thus see that what copper we put into this solution we can also take out of it by means of this instrument.

Putting that solution aside, let us now see what effect this instrument will have upon water. Here are two little platinum-plates which I intend to make the ends of the battery, and this, (C) is a little vessel so shaped as to enable me to take it to pieces, and show you its construction. In these two cups (A and B), I pour mercury, which touches the ends of the wires connected with the platinum-plates. In the vessel (C), I pour some water containing a little acid (but which is put only for the purpose of facilitating the action, it undergoes no change in the process), and connected with the top of the vessel is a bent glass tube (D), which may remind you of the pipe which was connected with the gun barrel in our furnace experiment, and which now passes under the jar (F). I have now adjusted this apparatus, and we will proceed to affect the water in some way or other. In the other case, I sent the water through a tube which was made red hot; I am now going to pass the electricity through the inside of this vessel. Perhaps I may boil the water; if I do boil the water I shall get steam; and you know that steam condenses when it gets cold, and you will therefore see by that, whether I do boil the water or not. Perhaps, however, I shall not boil the water, but produce some other effect. You shall have the experiment and see. There is one wire which I will put to this side (A), and here is the other wire which I will put to the other side (B), and you will soon see whether any disturbance takes place. Here it is seeming to boil up famously; but does it boil? Let us see whether that which goes out is steam or not. I think you will soon see the jar

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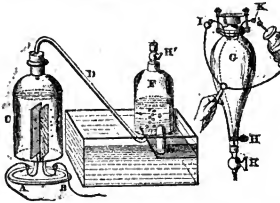
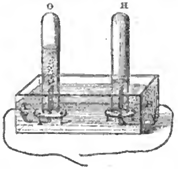
[1] Reported *verbatim* by special permission.

(F), will be filled with vapour, if that which rises from the water is steam. But can it be steam? Why, certainly not; because there it remains, you see, unchanged. There it is standing over the water, and it therefore cannot be steam, but must be a permanent gas of some sort. What is it? Is it hydrogen; is it steam; is it anything else? Well, we will examine it. If it is hydrogen it will burn. [The Lecturer then ignited the gas collected, which burnt with an explosion.] It is certainly something combustible, but not combustible in the way that hydrogen is. Hydrogen would not have given you that noise, but the colour of that light when the thing did burn was like that of hydrogen; it will, however, burn without contact with the air. That is why I have chosen this other form of apparatus, for the purpose of pointing out to you what are the particular circumstances of this experiment. In place of an open vessel, I have taken one that is closed; (our battery is so beautifully strong, that we are even boiling the mercury, and getting all things right,—not wrong, but vigorously right); and I am going to show you that that gas, whatever it may be, can burn without air; and in that respect differs from a candle, which cannot burn without the air. And our manner of doing that is as follows:—I have here a glass vessel (G) which is fitted with two platinum wires (I K), through which I can apply electricity; and we can put the vessel on the air-pump and exhaust the air, and when we have taken the air out we can bring it here and fasten it on to this jar (F), and let that gas into the vessel which was formed by the action of the voltaic battery upon the water, and which we have produced by changing the water into it,—for I may go as far as this, and say we have merely, by that experiment, changed the water into that gas. We have not only altered its condition, but we have changed it really and truly into that gaseous substance; and all the water is there which was decomposed by the experiment. As I screw this vessel (G H) on here (H'), and make the tubes well connected, and when I open the stop-cocks (H H H'), if you watch the level of the water (in F), you will see that that gas will rise. Now, I will close the stop-cocks, as I have drawn up as much as that vessel can hold, and being safely conveyed into that chamber, I will pass into it an electric spark from this Leyden jar (L), and the vessel, which is now quite clear and bright, will become dim. There will be no sound, for the vessel is strong enough to confine the explosion. [A spark was then passed through the jar, when the explosive mixture was ignited.] Did you see that brilliant light? If I again screw the vessel on to the jar, and open these stop-cocks, you will see that the gas will rise a second time. [The stop-cocks were then opened.] Those gases [referring to the gases first collected in the jar, and which had just been ignited by the electric spark] have disappeared, as you see: their place is vacant, and fresh gas has gone in. Water has been formed of them; and if we repeat our operation [repeating the last experiment], I shall have another vacancy, as you will see by the water rising. I always have an empty vessel after the explosion, because the vapour or gas into which that water has been resolved by the battery, explodes under the influence of the spark, and changes into water; and by-and-by you will see in this upper vessel some drops of water trickling down the sides and collecting at the bottom.



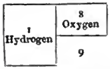
We are here dealing with water entirely, without reference to the atmosphere. The water of the candle had the atmosphere helping to produce it; but in this way it can be produced independently of the air. Water, therefore, ought to contain that other substance which the candle takes from the air, and which, combining with the hydrogen, produces water.

Now, you saw that one end of this battery took hold of the copper, extracting it from the vessel which contained the blue solution. It was effected by this wire; and surely we may say if the battery has such power with a metallic solution which we made and unmade, may we not think that it is possible that it can split asunder the component parts of the water, and put them into this place and that place? Suppose I take the poles—the metallic ends of this battery—and see what will happen with the water in this apparatus (Fig. 2) where we have separated the two ends far apart. I place one here (at A), and the other there (at B), and I have little shelves with holes which I can put upon each pole, and so arrange them that whatever escapes from the two ends of the battery will appear as separate gases; for you saw that the water did not become vaporous, but gaseous. The wires are now in perfect and proper connection with the vessel containing the water, and you see the bubbles rising; let us collect these bubbles and see what they are. Here is a glass cylinder (O), I fill it with water and put it over one end (A) of the pile, and I will take another (H) and put it over the other end (B), of the pile. And so now we have a double apparatus, with both places delivering gas. Both these jars will fill with gas. There they go, that to the right (H) filling very rapidly; the one to the left (O) filling not so rapidly; and though I have allowed some bubbles to escape, yet still the action is going on pretty regularly, and were it not that one is rather smaller than the other, you would see that I should have twice as much in this (H), as I have in that (O). Both these gases are colourless; they stand over the water without condensing; they are alike in all things—I mean in all *apparent* things; and we have here an opportunity of examining these bodies and ascertaining what they are. Their bulk is large, and we can easily apply experiments to them. I will take this jar (H) first, and will ask you to be prepared to recognise hydrogen.



Think of all its qualities—the light gas which stood well in inverted vessels, burning with a pale flame at the mouth of the jar, and see whether this gas does not satisfy all these conditions. If it be hydrogen it will remain here while I hold this jar inverted. [A light was then applied and the hydrogen burn.] What is there now in the other jar? You know that the two together made an explosive mixture. But what can this be which we find as the other constituent in water, and which must therefore be that substance which made the hydrogen burn? We know that the water we put into the vessel consisted of the two things together. We find one of these is hydrogen: what must that other be which was in the water before the experiment, and which we now have by itself? I am about to put this lighted splinter of wood into the gas. The gas itself will not burn, but it will make the splinter of wood burn. [The Lecturer ignited the end of the wood and introduced it into the jar of gas.] See how it invigorates the combustion of the wood, and how it makes it burn far better than the air would make it burn, and now you see by itself that every other substance which is contained in the water, and which, when the water was formed by the burning of the candle, must have been taken from the atmosphere. What shall we call it, A, B, or C? Let us call it O—call it "Oxygen;" it is a very good distinct-sounding name. This, then, is the oxygen which was present in the water, forming so large a part of it.

We shall now begin to understand more clearly our experiments and researches; because when we have examined these things once or twice we shall soon see why a candle burns in the air. When we have in this way analysed the water—that is to say, separated, or electrolysed its parts out of it, we got two volumes of hydrogen, and one of the body that burns it. And these two are represented to us on this diagram, with their weights also stated, and



Oxygen	88·9
Hydrogen	11·1
Water	100·0

we shall find that the oxygen is a very heavy body by comparison with the hydrogen. It is the other element in water.

I had better, perhaps, tell you now how we get this oxygen abundantly, having shown you how we can separate it from the water. Oxygen, as you will immediately imagine, exists in the atmosphere; for how should the candle burn to produce water without it? Such a thing would be absolutely impossible, and chemically impossible without oxygen. Can we get it from the air? Well, there are some very complicated and difficult processess by which we can get it from the air; but we have better processes. There is a substance called the black oxide of manganese; it is a very black-looking mineral, but very useful, and when made red hot it gives out oxygen. Here is an iron bottle which has had some of this substance put into it, and there is a tube fixed to it, and a fire ready made, and Mr. Anderson will put that retort into the fire, for it is made of iron, and can stand the heat. Here is a salt called chlorate of potassa, which is now made in large quantities for bleaching, and chemical and medical uses, and for gunpowder and other purposes. I will take some and mix it with some of the oxide of manganese (oxide of copper, or oxide of iron would do as well), and if I put these together in a retort far less than a red heat is sufficient to evolve this oxygen from the mixture. I am not preparing to make much, because we only want sufficient for our experiments; only, as you will see immediately, if I use too small a charge the first portion of the gas will be mixed with the air already in the retort, and I should be obliged to sacrifice the first portion of the gas because it would be so much diluted with air; the first portion must, therefore, be thrown away. You will find in this case that a common spirit lamp is quite sufficient for me to get the oxygen, and so we shall have two processes going on for its preparation. See how freely the gas is coming over from that small portion of the mixture. We will examine it and see what are its properties. Now, in this way we are producing, as you will observe, a gas just like the one we had in the experiment with the battery, transparent, undissolved by water, and presenting the ordinary visible properties of the atmosphere. (As this first jar contains the air, together with the first portions of the oxygen set free during the preparation, we will carry it out of the way, and be prepared to make our experiments in a regular, dignified manner.) And inasmuch as that power of making wood, wax, or other things burn, was so marked in the oxygen we obtained by means of the voltaic battery from water, we may expect to find the same property here. We will try it. You see there is the combustion of a lighted taper in air, and here is its combustion in this gas [lowering the taper into the jar]. See how brightly and how beautifully it burns;—you can also see more than this,—you will perceive it is a heavy gas, whilst the hydrogen would go up like a balloon, or even faster than a balloon, when not encumbered with the weight of the envelope. You may easily see that although we obtained from water twice as much in volume of the hydrogen as of oxygen, it does not follow that we have twice as much in weight; because one is heavy and the other a very light gas. We have means of weighing gases or air; but without stopping to explain that, let me just tell you what their respective weights are. The weight of a pint of hydrogen is three-quarters of a grain; the weight of the same quantity of oxygen is nearly twelve grains. This is a very great difference. The weight of a cubic foot of hydrogen is one-twelfth of an ounce; and the weight of a cubic foot of oxygen is one ounce and a third. And so on we might come to masses of matter which may be weighed in the balance, and which we can take account of as to hundred-weights and as to tons, as you will see almost immediately.

Now as regards this very property of oxygen supporting combustion, which we may compare to air, I will take a piece of candle to show it you in a rough way,—and the result will be rough. There is our candle burning in the air: how will it burn in oxygen? I have here a jar of this gas, and I am about to put it over the candle for you to compare the action of this gas with that of the air. Why, look at it; it looks something like the light you saw at the poles of the voltaic battery. Think how vigorous that action must be! And yet during all that action nothing more is produced than what is produced by the burning of the candle in air. We have the same production of water; and the same phenomena exactly, when we use this gas instead of air, as we have when the candle is burnt in air.

But now we have got a knowledge of this new substance, we can look at it a little more distinctly, in order to satisfy ourselves that we have got a good general understanding of this part of the product of a candle. It is wonderful, you see, how great the supporting powers of this substance are as regards combustion. For instance, here is a lamp which, simple though it be, is the original, I may say, of a great variety of lamps which are constructed for divers purposes,—for lighthouses, microscopic illuminations, and other uses; and if it was proposed to make it burn very brightly, you would say, "If a candle burnt better in oxygen, will not a lamp do the same?" Why, it will do so. Mr. Anderson will give me a tube coming from our oxygen-reservoir, and I am about to apply it to this flame, which I will previously make burn badly on purpose. There comes the oxygen: what a combustion that makes! But if I shut it off, what becomes of the lamp? [The flow of oxygen was stopped, and the lamp relapsed to its former dimness.] It is wonderful how, by means of oxygen, we get combustion accelerated. But it does not affect merely the combustion of hydrogen, or carbon, or the candle; but it exalts all combustions of the common kind. We will take one which relates to iron for instance, as you have already seen iron burn a little in the atmosphere. Here is a jar of oxygen, and this is a piece of iron wire; but if it were a bar as thick as my wrist, it would burn the same. I first attach a little piece of wood to the iron, I then set the wood on fire, and let them both down together into the jar. The wood is now alight, and there it burns as wood should burn in oxygen; but it will soon communicate its combustion to the iron. The iron is now burning brilliantly, and will continue so for a long time. As long as we supply oxygen, so long can we carry on the combustion of the iron, until the latter is consumed.

We will now put that on one side, and take some other substance; but we must limit our experiments, for we have not time to spare for all the illustrations you would have a right to, if we had more time. We will take a piece of sulphur: you know how sulphur burns in the air; well, we will put it into the oxygen, and you will see that whatever can burn in air can burn with a far greater intensity in oxygen, leading you to think that perhaps the atmosphere itself owes all its power of combustion to this gas. The sulphur is now burning very quietly in the oxygen; but you cannot for a moment mistake the very high and increased action which takes place when it is so burnt, instead of being burnt merely in common air.

I am now about to show you the combustion of another substance—phosphorus. I can do it better for you here than you can do it at home. This is a very combustible substance, and if it be so combustible in air, what might you expect it would be in oxygen? I am about to show it to you not in its fullest intensity, for if I did so we should almost blow the apparatus up; I may even now crack the jar, though I do not want to break things carelessly. You see how it burns in the air. But what a glorious light it gives out when I introduce it into oxygen! [Introducing the lighted phosphorus into the jar of oxygen.] There you see the solid particles going off which cause that combustion to be so brilliantly luminous.

Thus far we have tested this power of oxygen and the high combustion it produces, by means of other substances. We must now, for a little while longer, look at it as respects the hydrogen. You know when we allowed the oxygen and the hydrogen derived from the water to mix and burn together we had a little explosion. You remember also that when I burnt the oxygen and the hydrogen in a jet together, we got very little light but great heat; I am now about to set fire to oxygen and hydrogen mixed in the proportion in which they occur in water. Here is a vessel containing one volume of oxygen and two volumes of hydrogen. This mixture is exactly of the same nature as the gas we just now obtained from the voltaic battery; it would be far too much to burn at once; I have therefore arranged to blow soap bubbles with it and burn those bubbles, that we may see by a general experiment or two how this oxygen supports the combustion of the hydrogen. First of all we will see whether we can blow a bubble. Well, there goes the gas [causing it to issue through a tobacco-pipe into some soap-suds]. Here I have a bubble. I am receiving them on my hand, and you will perhaps think I am acting oddly in this experiment, but it is to show you that we must not always trust to noise and sounds, but rather to real facts. [Exploding a bubble on the palm of his hand.] I am afraid to fire a bubble from the end of the pipe because the explosion would pass up into the jar and blow it to pieces. This oxygen then will unite with the hydrogen, as you see by the phenomena and hear by the sound, with the utmost readiness of action, and all its powers are then taken up in its neutralisation of the qualities of the hydrogen.

So now I think you will perceive the whole history of water with reference to oxygen and the air, from what we have before said. Why does a piece of potassium decompose water? Because it finds oxygen in the water. What is set free when I put it in the water, as I am about to do again? It sets free hydrogen, and the hydrogen burns; but the potassium itself combines with oxygen; and this piece of potassium, in taking the water apart,—the water, you may say, derived from the combustion of the candle,—takes away the oxygen which the candle took from the air, and so sets the hydrogen free; and even if I take a piece of ice, and put a piece of potassium upon it, the beautiful affinities by which the oxygen and the hydrogen are related are such, that the ice will absolutely set fire to the potassium. I show this to you to-day, in order to enlarge your ideas of these things, and that you may see how greatly results are modified by circumstances. There is the potassium on the ice, producing a sort of volcanic action.

It will be my place when next we meet, having pointed out these anomalous actions, to show you that none of these extra and strange effects are met with by us—that none of these strange and injurious actions take place when we are burning, not merely a candle, but gas in our streets, or fuel in our fire-places, so long as we confine ourselves within the laws that Nature has made for our guidance.

[*The Fifth Lecture will appear in our next Number.*]

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 8, 1861.

E. W. Binney, *F.R.S., F.G.S., &c., Vice-President, in the Chair.*

Mr. Ransome exhibited and explained the means of obtaining a photograph, which, although appearing a confused series of light and shade, yet, when reflected from a polished cylinder, was a beautiful picture.

Dr. Crace Calvert brought under the notice of the Society an interesting communication made to him by Professor Arnaudon, of Turin, to the effect that oxalate of ammonia completely modifies the action of yellow prussiate of potash when mixed in solution with a salt of peroxide of iron. Thus, if oxalate of ammonia be added to this metallic salt, it will give no prussian blue when a solution of yellow prussiate of potash is added; but on the addition of an acid, prussian blue is immediately produced. The knowledge of this fact may be interesting to calico printers, as it will give them the means of easily producing prussian blue on their fabrics. To attain this desirable end, the printer will simply have to pad his fabric through a mixture of persalt of iron and oxalate of ammonia, dry, and paint an acid where he wishes to produce the blue.—After Dr. Calvert had made a few more remarks on this subject, he stated that he had lately examined several varieties of Snuffs, which he had found to be more or less impregnated with lead compounds, especially the black rappee, and he found on further investigation that the presence of lead was due to the corrosive action of the snuff upon the lead foil used for packing it. He also stated that it was his intention to examine several other substances usually packed in lead foils, and that he would lay the results of his observations before the Society, as he thought it highly desirable to make the public aware of such sources of injury and to induce manufacturers to adopt means to avoid inflicting this serious evil on their consumers. Dr. Calvert concluded by stating that he had been engaged for the last few months in investigating the action of the Manchester Waterworks water on various kinds of leaden pipes, and that he was arriving at such results as would show the necessity for serious consideration on the part of the inhabitants of this city with respect to the evils arising from the introduction of the water into their dwellings through leaden pipes.

Being requested by the Chairman to give his opinion, Dr. Angus Smith said that he had never found any Manchester water which had passed through lead pipes to be entirely free from lead. At the same time, the quantity is in almost all cases so small that, as far as we know, it can produce no bad effects, and is practically equal to nothing. There is, however, a great difficulty in knowing what is hurtful; medical men had not settled the point. Persons said to be suffering from lead paralysis were known to have taken water with as little as one-hundredth of a grain of oxide of lead per gallon, whereas it was considered generally not to be hurtful until it contained one-fortieth of a grain. We have little idea of the extreme susceptibility of some persons, and it is better to avoid lead as much as possible. Short lead pipes may be used without fear in Manchester, especially if the water which has stood over night be thrown away. Long lead pipes should be avoided, and lead cisterns are extremely dangerous, especially with soft waters, including Manchester water. (Here instances were given.) Soft water dissolves lead more readily than many hard waters, and if the hardness be due only to the earthy carbonates, the lead becomes coated instead of being dissolved. But if the hardness be due to chlorides or nitrates, the water dissolves lead much more rapidly than pure water (referring to what he had written on this). He gave an instance of water from a cesspool obtaining, by oxidation of its impurities in a porous soil, much nitric acid, which, along with the chlorides always found in such cases, caused the adjacent water, when draw up by a lead-pump, to have a very strong taste of lead salts. It is remarkable that this water was drunk for some years, but ultimately caused the death of two or three persons. It is a mistake to suppose that pure water dissolves lead more than all impure waters. Some very pure natural waters dissolve lead simply because they contain chlorides, although in small quantities. Such waters sometimes come from clay slates and similar formations. As to peaty water, it was of two kinds, occasionally acid, with some action on lead, but in most cases alkaline, the peaty matter not dissolving lead. Some lead pipes were more easily affected than others. Dr. Smith gave an instance of a lead pipe, nearly an inch in thickness, with holes pierced through the sides in various places by the action of the water; others are much more equally corroded. He had spent a long time in obtaining a suitable coating for lead to protect the water from its action, and had not quite succeeded; but he had given his results to a friend, who had gone further, and, having obtained great success, patented the process. However, he was told that no one would buy lead pipes of the kind, as they cost half-a-crown per cwt. more than the ordinary ones. People complain of evils which they refuse to escape from. It is, however, better to avoid lead than to avoid pure water because of its action on lead.

A paper was read by Mr. Edward Hull, F.G.S., "*On the Nature and Objects of Geological Surveys, with Special Reference to the Progress of the Geological Survey of*

Lancashire and Cheshire." After describing the various sources, both natural and artificial, on which the geological surveyor depends for his conclusions, and according to which he is enabled to trace the boundaries of the formations, the author went on to observe that so generally is the value of such surveys recognised, that the Governments of nearly all the most civilised nations had undertaken their support. Amongst others, France, Belgium, Germany, Russia, the States of North America, the British Colonies, as Canada, New Columbia, India, Australia, and New Zealand. The Author then explained some details regarding the maps of Lancashire so far as they had been completed by the Geological Survey of Great Britain. Specimens of these maps, both of the one-inch and six-inch scales, were exhibited to the Society in the course of the evening.

MICROSCOPICAL SECTION.

December 17, 1860.

Letters were read from Sir Leopold McClintock, Mr. J. W. Read, of the Admiralty, and Dr. Wallich, who accompanied the former in the *Bull Dog*, in the late expedition to the North Seas. Dr. Wallich kindly presented to the Section a few copies of his pamphlet on "Life in the Deep Sea," now circulating amongst the members.

A Letter was also read from Captain M. F. Maury, of the U.S. Navy, promising to supply envelopes for soundings amongst the sperm whalers and other vessels trading to the Pacific Ocean, &c.

Specimens of incrustations from the boilers of the steamer *Edinburgh*, trading from Glasgow and Liverpool to New York; from the steamer *Rhone*, from Liverpool to Venice, Trieste, &c.; and from the steamer *Minho*, from Liverpool to Lisbon and Oporto, were received from Mr. W. A. Hayman, of Liverpool. The incrustations are as hard as marble, breaking with a crystalline fracture, and showing, by different coloured strata, the crust obtained from harbours and from the open sea. Mr. Dale stated that the component parts of the incrustations are sulphates of lime, magnesia, &c.; he recommended maceration in bicarbonate of ammonia to obtain calcareous shells, and in weak acids of muriate of barytes to obtain silicious shells. Various members took specimens for examination.

A Letter was read from Captain Andersen, of the Cunard steamer *Canada*, from Livepool to New York, accompanying specimens of the soundings taken during his last voyage across the Atlantic. Captain Andersen was kind enough to send the soundings by post from Queenstown, by which means they arrived just before the meeting.

Mr. W. H. Heys, of Hazel Grove, exhibited his newly invented Kaloscope, by means of which he obtains refracted and reflected light of different colours at the same time upon objects under the microscope, producing beautiful effects in some cases.

NOTICES OF BOOKS.

On Coal Gas. By the Rev. W. R. Bowditch, B.A., F.C.S. Van Voorst. 1860.

The substances obtained by the destructive distillation of vegetable matters, such as coal, peat, and wood, and not only the products proper, but the by-products, and even the impurities, have received, within the last few years, and are still constantly receiving, the most extensive and varied applications. Mr. Bowditch, of Wakefield, has given us some interesting details concerning the purification of the most important of all these products of destructive distillation, namely, illuminating gas. The amplitude of the subject may be guessed from the fact that Mr. Bowditch's remarks, originally given in the form of a lecture delivered before some directors and managers of gas-works, though occupying about sixty pages, relate only, or at all events chiefly, to the detection and removal of one particular impurity invariably present in coal-gas. The presence of bisulphide of carbon in coal-gas had been ascertained before, but the author of the little work before us has shown us how to secure its absence by a happily-devised plan, at once simple and cheap, a great contrast to the tedious and costly processes which had been previously proposed. We intend to give some account of Mr. Bowditch's various experimental results, premising, however, that our remarks will also refer to the specifications which the author has given of his patent processes "for improvements in the purification of coal-gas," and to the corroborative testimony of several chemists who have reported upon the efficiency of his methods. As we proceed we shall point out one or two discrepancies between these different accounts which we have not been able to reconcile.

The book opens with a notice of Dr. Clayton's discovery of coal-gas (termed by him "Spirit of Coals"), in the year 1739. Next, we are told what substances are found in the gas which passes to the purifiers of a gas-works, after adequate condensation, these substances being divided into three classes,—illuminating substances, diluents, and impurities. The author says, "Of the impurities, I omit sulphuretted hydrogen and carbonic acid, because they are removable by the simplest means; and, except in the case of accident, cannot be found in gas supplied to consumers without most culpable negligence, or still more culpable parsimony." This is no doubt true as a general rule, but, unfortunately, in small country towns where the consumption of gas is not large, the former impurity may be often detected.

Ammonia is the first impurity to which the author devotes special notice. After stating that no impurity is more easily removed from coal-gas than ammonia, he goes on to describe the methods adopted:—"Where attempts are made to remove ammonia, the substances employed to that end are,—water, acids taken up by sawdust, metallic and earthy salts, and clay. I will say a word or two upon the advantages and disadvantages of each." He then states that water dissolves considerable quantities of the illuminating constituents of gas, and stops others by a mechanical action. "I am acquainted with a gas-works where five scrubbers are employed to wash the gas, four being placed between the condenser and the purifiers, and one between the purifiers and the gas-holders. Wigan coal is used and yet the illuminating power of the gas is but twelve candles. I asked the intelligent manager if he did not find a considerable quantity of hydro-carbon vapours removed even by the last scrubber, and he admitted that a considerable quantity could generally be obtained from the surface of a small well through which the water flowed from this scrubber on its passage to the store-tank." The hydro-carbons thus dissolved or mechanically separated by the water are, in reality, those of highest value, so far as illuminating power is concerned. Among them may be mentioned olefiant gas, propylene, and other members of the same series as well as those hydro-carbons which present the composition and vapour density of the alcohol radicals (propyle, &c.,) and which, if not identical with these bodies are isomeric with them. Thus it would seem to be with good reason, that Mr. Bowditch condemns the use of water as a purifying agent. And not only does it impoverish the gas, but it fails to separate the tar and sulphuretted carbon compounds present. Acids, and it would seem, metallic salts also present the same disadvantages as water. As illustrations of the disadvantage attendant upon the use of acids, the following instructive experiments are described. If through a tube containing deal sawdust, moistened with sulphuric acid, which has been diluted with six times its weight of water, gas be passed, a pink colouration, gradually deepening in intensity, will be observed. With a gas giving the light of 14 candles for a consumption of 5 feet per hour, the colour is produced with moderate rapidity, but is not intense; and it is found that the better the gas the more intense is the colouration, and the more quickly does it take place. Mr. Bowditch has experimented with olefiant gas, propylene and benzole, and in each case finds that if only a sufficient quantity of acidified sawdust be employed, the vapour of these valuable illuminating ingredients of gas, while they redden the sawdust are completely absorbed by it. He proposes the employment of the acidified sawdust as a method of ascertaining the light-giving power of different specimens of gas, by means of the depth of colour produced. The rationale of this absorption may be as follows. It is quite possible that the large surface of dilute sulphuric acid may exert the same absorbing effect on olefiant gas and its homologues and on benzole which strong sulphuric acid under ordinary conditions does. If so, the sawdust after being used should yield the acids commonly known as sulphethylic, sulphopropylic, and sulphobenzolic.

Mr. Bowditch says, "Clay removes ammonia and its salts as completely as acids do. In this respect it is merely their equal, and if this were the limit of its power, it would not be entitled to any preference. *But it possesses properties and powers peculiar to itself, and exerts a beneficial influence which nothing else known to chemists can exert.*" Mr. Bowditch then relates a most important experiment. He takes gas which contains no sulphuretted hydrogen and which does not blacken lead-paper, passes it through a cold purifier containing clay, and then shows how powerful a darkening effect is produced on the test-paper then. Of course nothing is easier than to remove, by the ordinary lime process, the sulphuretted hydrogen now present, in which form the sulphur, previously existing as bisulphide of carbon, bisulphide of ethylene, &c., now appears in virtue of the action of the clay. The words which we have italicised above, where Mr. Bowditch states that clay is the only material capable of changing bisulphide of carbon and other similar sulphur compounds into the easily-removable sulphuretted hydrogen, though they re-appear frequently in different parts of the work before us, and are affirmed with emphasis, yet in his specification, filed before the publication of the work on gas, the same virtues are assigned to other substances, and especially to slaked lime at particular temperatures. More than this, the latter part of the book itself contains a complete account of the energetic action of monohydrate of lime upon the various injurious sulphur compounds of gas and we have seen the opinions and testimonials of Drs. Brande, Letheby, and Frankland, and of Messrs. Warrington and Keates, all of whom speak in the highest terms of the practical efficiency of that process invented by our author in which monohydrate of lime is employed at temperatures varying between 300° and 400° F., while we look in vain for a syllable concerning the process in which clay is employed, and to the description of which by far the greater part of Mr. Bowditch's lecture, and also of his specification is devoted.

The author finds that all the sulphur of gas appears as sulphuretted hydrogen when the gas is conducted over hydrate of lime at the particular temperatures specified above. He suggests applications both of the cold clay and of the hot lime process for domestic use, and gives all the necessary details for their employment on the large scale. The expense is quite trifling, and becomes quite unworthy of consideration when the enormous benefits conferred by the adoption of the plan are taken into account. The lime or clay-process may be most advantageously adopted in the purification of coal-oils. The vapour of these liquids is to be passed over hydrate of lime heated to about the boiling-point of the liquid used, and the product afterwards washed with a dilute alkaline solution, to remove the dissolved sulphuretted hydrogen produced in the process.

The author makes the startling assertion that "naphthalin is a hurtful, sulphur-bearing, non-illuminating compound." We think, however, that we should like to have further experimental proofs of this statement than Mr. Bowditch affords. His explanation of the offensive state of the earth in contiguity with the gas-mains in streets is worthy of consideration, and is most probably correct. The sulphuretted hydrogen which blackens the London soil "results from the sulphur-compounds which the clay (in the soil) breaks up and re-arranges."

We hardly think Mr. Bowditch's method of detecting bisulphide of carbon in coal-gas (p. 35) an infallible one, for this sulphur-compound is not the only liquid possibly present in gas, which, while soluble in alcohol, is precipitated to the bottom of the containing vessel on the addition of six or seven volumes of water, a cloudiness or turbidity being at first produced. A confirmatory test should certainly be applied. The detection of sulpho-cyanogen compounds in coal-gas is not new, and it has long been known that bisulphide of carbon, in the presence of water, ammonia, and sulphuretted hydrogen (or of sulphide of ammonium in solution), yields a large quantity of sulpho-cyanide of ammonium.

In a volume such as that to which we are now directing our readers' attention, and which contains the history of a most important discovery, we are sorry to find some errors and oversights which the assistance of a person tolerably versed in the great principles of chemical science, in reading the proof sheets, would have enabled the author to avoid. At the same time, of course, we cannot but perceive how extensive and clear are Mr. Bowditch's views on the great practical questions on which he treats. It is likely that the book will soon reach a second edition, and it would undoubtedly be improved by one or two alterations in such points as the following. On page 19, the formula HCS_3 in accordance with the old and usually-received notation, is assigned to sulpho-carbonic acid, while on page 38, in an extract from a paper by Dr. Hofmann, the formula given to xanthate of potash in the original memoir, viz. $K(C_2H_5)\ CS_2O$, is passed over without notice, although the value assigned to the symbols C and S is not the same in both cases. We have not the original paper at hand, but we can hardly imagine that its author could have identified the terms "xanthate" and "sulpho-carbonate" as we find to be the case on page 38. An extract from Dr. Thompson's evidence on the Metropolitan Gas Bill is given on pages 47 and 48, and one of his erroneous statements is there corrected. But another passes unnoticed, for the statement that 9½ grains of bisulphide of carbon contain or correspond to 4 grains of sulphur, remains unaltered, although it does not require a very profound knowledge to see that they correspond to exactly double this amount.

We have been given to understand that Mr. Bowditch's process is highly appreciated by the great gas companies, and will be at once adopted. We trust, however, that in the present case no attempt will be made to overthrow the validity of this gentleman's patent. We have heard with regret that the larger gas companies possess a common fund devoted solely or mainly to contest patent rights, and consequently, in many cases, to get rid of just claims. We can scarcely imagine that they would be successful in combating Mr. Bowditch's rights, for we are compelled to regard his discovery as totally new in theory, as it is admirably successful in practice.

CORRESPONDENCE.

Manufacture of Sulphuric Acid.

To the Editor of the Chemical News.

Sir,—While absent from England, I have heard that one of your correspondents has inquired for an apparatus to produce from one to seven tons of sulphuric acid per week.

Having devoted several years to gradually bringing to practical perfection an apparatus for this purpose, totally unlike the usual leaden chambers, and seeing great advantages in it, I shall be happy to communicate further with the party, either through your columns or (what would, perhaps, be less like an indirect advertisement) privately by letter, if he thinks well.

The basis of my system is the drawing the gases through coarse, rounded silicious sand, or equivalent material, in stoneware columns, all being put together with a cement which is indestructible by the acids used. The general principle has attracted the notice of several inventors and manufacturers; possibly, in some cases, independently of my patents; but its useful application requires arrangements which have not been so readily seen.

The great benefits of this system in my application of it are, that it presents a means of effecting the requisite chemical actions with great rapidity, or in a very small space, and very perfectly; and that the apparatus is indestructible, not needing the usual heavy expenses for repairs and renewals, nor any of the usual care lest it be destroyed by negligent working with too much nitre, or by lying unused for any time or at any intervals with the acids in it. It is well known that the usual leaden chambers, and their coke apparatus, or "scrubbers," are greatly at fault in meeting any of the aforesaid requirements.—I am, &c.

W. Petrie.

Nouvelle Montagne Zinc Works, Engis, near Liege, Belgium.

The Adulteration of Food Act in the City.

To the Editor of the Chemical News.

Sir,—It is with feelings of surprise I read in the Chemical News the report of Dr. H. Letheby on Mustard. He admits the mustard was adulterated with flour and turmeric. Now, what is the intention of a manufacturer, when he adulterates any article, but to lessen the cost of producing the article in question? Now, here is a large manufacturer reducing the mustard with flour, and bringing up the colour with turmeric, both articles lower in price than the article vended.

The grocer is mulcted in the penalty of £100 if he mixes chicory with coffee, and yet chemists admit that chicory in itself does not contain anything injurious to health.

Surely the Act, and also the analysation, is a farce, for the only question for the manufacturer will be,—This article costs me 6d. per pound; I want something to adulterate it, but I must find some harmless substance to adulterate it with, so that it may be certified that the article is not injurious to health.—*Circumspice!*—I am, &c.

George J. Johnson.

A Curious Fact connected with Sounds, Smells, and Tastes of Various Intensities.

To the Editor of the Chemical News.

Sir,—It is very remarkable that faint sounds are different in kind, if human judgment is correct in this particular, to strong ones of the same origin, and that the same thing may be said of every taste and smell. A gentle rap with the knuckle upon a plate of iron produces a sound apparently different in kind to that which arises from a more forcible stroke, similarly given, and this experience characterises every description of sound. And so the faint smell of a rose, and the taste of diluted brandy and gin, appear respectively to be different in kind from the strong smell and flavours. These are illustrations, not, I apprehend, the most striking, of a universal law.

It may properly be thought and inquired what is the criterion of difference in kind and degree? and this is a question to which it is impossible, in the present state of science, to give a satisfactory reply. If the production of sound is ever thoroughly understood, it will be possible to say when various sounds differ sufficiently to allow of a human distinction into kind and degree, and to refer to the judgment before this is simply to permit every individual to assume—seeing that the proof is impossible—whichever view he pleases. The former remark is of course only applicable to those sounds which are near to the division into kind and degree, inasmuch as,—which applies to the two other sensations, and the optical sensation of black—there are sounds which it is exceedingly probable, if not certain, differ in kind, and may be extended to colour, taste, and smell of every description.

I am, &c. J. Alexander Davies.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Phenomena Attending the Amalgamation of Zinc, Cadmium, and Iron.—M. J. Regnault has observed that the amalgamation of zinc with mercury is always attended with a diminution of temperature. (*Comptes-Rendus*, t. xli. p. 778.) Thus, when zinc and mercury at 10° C. are mixed in a tube, a thermometer placed in the mixture sinks to 7·5°. The amalgamation of iron is also attended with a condensation of heat. But when cadmium is amalgamated the temperature rises—a mixture of the two metals at 10° rising to 13°. The above facts, the author says, explain how it happens that amalgamated zinc is more electro-positive than zinc alone, and amalgamated cadmium more electro-negative than cadmium itself. Zinc becomes electro-positive because it fixes heat in combining with mercury; and cadmium becomes electro-negative because it parts with heat under the same circumstances. Amalgamated soft iron and steel also was found to be electro-positive relatively to the non-amalgamated metal. Tempered steel is proved by experiment to be electro-positive relatively to the same steel untempered, and, applying the above results, the author supposes that the latent heat of tempered steel is less than that of untempered, and this, he thinks, may be the cause of its remarkable mechanical properties.

Influence of Pressure on Chemical Action.—M. Favre has found (*Comptes-Rendus*, t. xli. p. 1027) by experiment that when hydrogen is generated from zinc and diluted sulphuric acid in a vessel hermetically closed, the action of the sulphuric acid diminishes as the pressure increases. This he ascribes to the adhesion of hydrogen to the surface of the metal, which protects it from the action of the acid. The author found also that the electrolysis of water by four of Bunsen's elements with platinum electrodes was not at all affected by a pressure of 70 or 80 atmospheres. No ozone was noticed in the gas which had submitted to the pressure, but when the pressure was removed the liquid from which the gas had been disengaged was found to retain a small quantity in solution, and this, when collected, was found to blue ozone paper and bleach sulphide of lead.

II. ORGANIC CHEMISTRY.

Decomposition of Ethers by Anhydrous Alkalies.—MM. Berthelot and de Fleurieu have studied the action of anhydrous alkalies on compound ethers (*Comptes-Rendus*, t. li. p. 1020), and find that when so acted on they split up and form an acid and a compound corresponding to alcohol in which the equivalent of water is replaced by an equivalent of the alkali: for instance, they took 4 grammes of pure benzoic ether and 6 grammes of anhydrous baryta and placed them in a perfectly dry tube, which they then sealed and kept at a heat between 150° and 180° C. for five hours. After this time the tube was opened, and it was found that the benzoic ether had completely disappeared and no gas had been produced. The contents of the tube, when heated on a water bath, gave off no volatile matter, nor did they yield up anything to anhydrous ether. After being completely deprived of ether, water was added, and the mixture was rapidly distilled; immediately alcohol was obtained, the amount of which approached very nearly to the theoretical quantity. The liquid remaining in the retort was then treated with carbonic acid to remove the excess of baryta, and afterwards evaporated, by which means perfectly pure benzoate of baryta was obtained. The following equation will represent the decomposition:—

$$\underbrace{C_{18}H_{10}O_4}_{\text{Benzoic Ether.}} + 2BaO = \underbrace{C_{14}H_5BaO_4}_{\text{Benzoate of Baryta.}} + \underbrace{C_4H_5BaO_2}_{\text{Alcoholate.}}$$

In performing the above experiment care must be taken to manage the temperature, for if it should pass above 200°C. the benzoic ether is destroyed, and various products are formed which complicate the results.

When acetic ether is acted on by baryta, the temperature must be higher and more prolonged. Maintained at 200° C. for thirty hours, the results correspond exactly with the foregoing.

Acetic ether and lime exposed to 250° C. for a long time yielded no gas nor formed any secondary products. The mass contained neither free alcohol nor ordinary ether, but, under the influence of water, it was immediately resolved into alcohol and acetate of lime. In the presence of anhydrous alcohol the results were perfectly similar. With formic and oxalic ethers and baryta the results were similar to the above, but not quite so perfect, for some traces of gas and secondary products were obtained. The authors conclude that the action of anhydrous alkalies on ethers is comparable to that of the hydrated alkalies, the elements of the anhydrous alkali replacing the elements of water in the regenerated alcohol.

III. TECHNICAL CHEMISTRY.

Manufacture of Ice.—Although there is no lack of ice at the present time, it is worth knowing how to obtain it by a very simple process in the hottest summer. M. Carre (*Cosmos*, t. xvii. p. 686) takes two strong iron retorts (quicksilver bottles, they say, will answer), into one of which he puts a saturated solution of ammonia. When the two are connected, he places the empty one in a vessel of water, and the other over a fire. The heat drives over ammonia which is condensed by pressure in the previously empty retort. When the ammoniacal gas is liquefied, the one retort is removed from the fire and allowed to cool, whereupon the ammonia in the other rapidly assumes the gaseous form, abstracting so much heat from the water by which it is surrounded as to freeze a considerable quantity. The experiment is repeated without disconnecting the retorts. The cost of ice made in this way is said not to exceed a centime per kilogramme—or about fivepence a hundredweight.

MISCELLANEOUS.

Royal Institution.—The following Lectures will be delivered at this Institution during the ensuing week:—Tuesday, January 29, at 3 o'clock, Professor Owen "On Fishes." Thursday, January 31, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 1, at 8 o'clock, Rev. Alexander J. D. D'Orsey "On the Study of the English Language as an Essential Part of a University Course." Saturday, February 2, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the Chemical News, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume will be ready for delivery with the Magazines for February.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*** All *Editorial Communications* are to be addressed to Mr. Crookes and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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Tin Solder may be made by melting together equal weights of tin and lead. A more fusible solder may be formed of 3 equivalents of tin to one of lead; this will melt at 367° Fahr.

B. P.—Your article is received with many thanks. Can you favour us with the specimen and the continuation article referred to in your note?

W. W.—A full abstract of M. Caron's article on the reduction of calcium was given in our first volume. The reference to the original paper accompanied it.

Manufacture of Glue on the large scale.—A correspondent is desirous of knowing the best process for the above manufacture.

D. Clifton.—No fuller account was published than the one which appeared in our columns. If we are able to hear of further details we will give them insertion.

J. D. P.—We have decided to stitch the numbers in future before issuing them, this will get over the difficulty of the two loose pages which the recent increase of size has rendered necessary.

Edgar.—The red colouring matters named are slight modifications of the same body,—the violet colours are the same; the tints of each differ slightly according to method of preparation. Aniline can be made on the small scale by the method recommended in the article by M. Kopp. Methylated spirits will do in most cases where alcohol is mentioned—not in all.

W. E. R.—We will give the prices next week.

R. J.—Gerhart's continuation of Berzelius' "Chemistry" is a very good work for the advanced student. The new edition of Fownes' "Chemistry," which is shortly to appear, will contain a good account of the different systems of chemical nomenclature.

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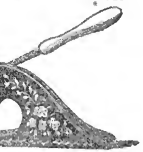

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THE

CHEMICAL NEWS

AND

Journal of Physical Science.

Edited by] [Wm. Crookes, F.C.S.

No. 61.] Saturday, February 2, 1861. Price 4d. Post Free, 5d.

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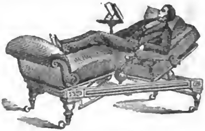

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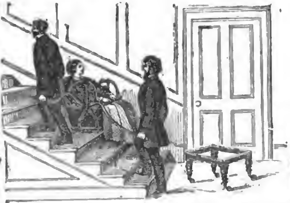

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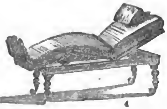

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THE CHEMICAL NEWS

Vol. III. No. 61.—February 2, 1861.

THE ADULTERATION OF FOOD ACT.

Our attention has been drawn to advertisements inviting applications for the appointment of Analyst, under the Adulteration Act, from *medical men* who are willing to take the fees receivable under the Act as their only remuneration. Such an advertisement, we imagine, could only have emanated from individuals who had entirely misunderstood the Act, or who were anxious to render it as inoperative as possible. In the first place, it is not at all necessary that the Analyst should be a medical man. All that the Act requires is, that he should possess "competent medical, chemical, and microscopical knowledge." But the amount of medical knowledge required, in order to know that a poisonous adulteration would be detrimental to health, is extremely small, while the chemical knowledge and experience required to detect the adulteration may be very great. We hold the medical profession in great respect, but, at the same time, we may safely assert that, while every competent chemist possesses sufficient medical knowledge, very few medical men have the chemical skill necessary for effectually carrying out the purposes of the Act. And what would be the effect of appointing an incompetent chemist? A timid Analyst would of course be anxious to err on the safe side. Uncertain about the composition of a substance, he could carefully guard himself by certifying that it "contained nothing detrimental to health," and then no proceedings could be taken, nothing in the Act of Parliament requiring him to state what is the actual composition of the article brought to him for analysis; and, even if he accepted the highest fee allowed by the Act as his only remuneration, he might almost be excused from troubling himself to ascertain it. The framers of the Adulteration Act well understood that the fees they made receivable were very insufficient rewards for the performance of analyses. The Act, therefore, expressly states that the districts appointing an Analyst may pay him "such salary or allowances as they may think fit,"—clearly, in addition to, or in place of the fees. Nor would such a payment out of the rates be at all inconsistent with ordinary practice, as some seem to think. The Analyst must be regarded as a sanitary detective policeman, who, like every other policeman, is employed as much to prevent as to detect crime; and if the knowledge that one had been appointed in a district should tend, as it probably would, to prevent adulteration, the ratepayers would be the gainers by the appointment.

But, in any case, we hope we shall not have to record that any gentleman has expressed his willingness to accept the office for the mere sake of the fees. If he did, the least he could do would be to exact the highest, which, in all probability, would prevent him from receiving any. Supposing, however, articles should be brought, for what analysis is ten shillings and sixpence an adequate remuneration? It is clear that only the most superficial examinations would be made, the most carefully-guarded certificates given, and the object of the Act entirely defeated.

But the more we consider the Adulteration Act, the more valueless does it appear. We may take one glaring illustration. A few years ago an ingenious Frenchman came to London with a secret, which quite enchanted the workhouse authorities, who allowed him to exhibit his skill. He showed them that it was possible to make a sack of flour into nearly twice the number of loaves they had made before, the individual loaves weighing exactly the same. The secret was the addition of a few pounds of rice, which, when made into dough, would, of course, hold a large quantity of water. Whether this sort of bread-and-water diet has been generally adopted for paupers we do not know, but certain it is that the Frenchman's secret has become extensively known to the baking trade, and one of the four articles taken to Dr. Letheby for analysis was some of this bread, which contained 44 per cent. of water. Now, for such a fraud as this the Adulteration Act provides no remedy, it not being, in the sense in which the words are used in the Act, an adulteration injurious to health. But it is a cheat of the most flagrant kind, and deserves the most summary punishment, unless, indeed, the bakers who practise it can be made to understand that they must regulate the price of bread as much by the rates of the Water Companies as the price of corn in Mark Lane.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Action of Citric and Tartaric Acids upon certain Metallic Sulphides, by Frederick Field.

Mr. Spiller, in a paper upon various substances which influence and modify chemical reactions,[1] gave us some very curious and interesting facts regarding the non-precipitation of sulphuric acid by a salt of baryta in the presence of citric acid or a citrate. After a detailed account of his experiments upon this particular head, Mr. Spiller extended his observations to the influence which citric acid or its salts exerted upon the sulphides and oxides of various metals, proving that many of the latter are not precipitated from their solutions by means of potash if a citrate is present; and, further, that the sulphide of manganese, and even of arsenic, are freely soluble in citrate of soda. As regards the metals in the group comprising iron, nickel, cobalt, manganese, and zinc, the author of the paper referred to, tells us that, with the exception of manganese, their precipitation as sulphides is not effected in the presence of a citrate. The action of citric acids upon these sulphides is not especially noticed.

The following experiments may be interesting, as exemplifying in a very remarkable degree the fact pointed out by Rose, many years ago, that the sulphides not precipitated from their slightly-acid solutions by sulphuretted hydrogen are, however, nearly insoluble in dilute acids. This is especially the case with nickel and cobalt, which may be separated from manganese, and even from iron by taking advantage of this peculiarity. The sulphides thrown down by sulphide of ammonium are thoroughly washed and digested in very dilute hydrochloric acid, which dissolves all the manganese, with traces of nickel and cobalt. The precipitate caused by adding sulphide of ammonium to the acid solution is slightly blackish, but after another digestion in acid of equal strength, small black flakes remain undissolved, and the addition of the alkaline sulphide now precipitates the pure, flesh-coloured sulphide of manganese. When iron has to be separated, the method is equally advantageous, although we have necessarily no guide, as far as the *colour* of the precipitate is concerned, to judge of the correctness of the results.

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[1] *Quarterly Journal of the Chemical Society*, vol. x. p. 110.

When sulphide of cobalt is digested in a strong solution of tartaric acid in the cold, little, if any, is dissolved; but, after prolonged boiling, the liquid, on evaporation, has a very faint-pink colour, and becomes brown upon the addition of sulphide of ammonium. Citric acid acts in the same manner, but perhaps with rather more energy, the colour of the solution having a brighter hue, and the shade still darker after the addition of a soluble sulphide. When carbonate of cobalt, however, is dissolved in excess of tartaric acid, and sulphuretted hydrogen passed through the solution, no precipitate takes place, even if the current of gas be continued for many hours. The same phenomenon occurs with citric acid, the liquid remaining perfectly clear and bright. If a small quantity of a solution of zinc be added, an immediate precipitate takes place, of a pure white colour, showing that the sulphide of this metal is insoluble in tartaric and citric acids. The separation of zinc from cobalt and nickel cannot be accurately effected by this means, however, as far as I have been enabled to judge from my own experiments. The carbonates of zinc and cobalt were dissolved in excess of tartaric acid in the cold, and sulphuretted hydrogen passed through the solution. The sulphide of zinc, which appeared perfectly white, was filtered off, but the filtrate, after standing a few minutes, became cloudy and deposited more sulphide. In order to prevent this, gentle heat was employed, and an hour or two allowed to elapse before filtration. On filtering, the sulphide of zinc was no longer white, but of a pale blue, evidently containing cobalt. In another experiment, the flask with the metals was placed upon the sand-bath for many hours after the passage of the gas, and the sulphide of zinc changed from white to blue, and from blue to green. The precipitate was separated by filtration, and had then very much the appearance of carbonate of copper, having as nearly as possible the same tint. That the cobalt was not in the state of sulphide, unless modified to an extraordinary extent, for which it is difficult to account, was evident for two reasons. In the first place, minute traces of sulphide of cobalt impart a blackish or dull-greyish tinge to white precipitates, such as sulphide of zinc; and in the second place, the compound was perfectly soluble in very dilute hydrochloric acid. After the disengagement of the sulphuretted hydrogen, sulphide of ammonium was added, which threw down a brownish compound, consisting of the sulphides of zinc and cobalt. The green colour which sulphide of zinc assumes when precipitated by sulphuretted hydrogen from its solution in tartaric acid, is certainly somewhat curious, especially when it is remembered that a salt of cobalt treated with oxide of zinc gives a green colour exactly similar to that just described. In pursuing these experiments further, it was observed that when oxide of zinc is digested in a neutral solution of nitrate of cobalt, little or no reaction is exercised, but on adding the same oxide to cobalt dissolved in a considerable excess of tartaric acid, nearly the whole of that metal is precipitated as a beautiful peach-coloured powder, which contains a large amount of its precipitant.

The separation of nickel and zinc is equally unsatisfactory, the same phenomena occurring as in the case of cobalt. Iron, also, is similarly affected. No precipitate whatever is produced in a solution of protoxide of iron in tartaric acid, by the passage of sulphuretted hydrogen but when zinc is present the sulphide of that metal, at first white, becomes slightly coloured after digestion in the supernatant liquid, and when placed upon a filter and washed, assumes a pinkish hue, the iron having passed to the state of peroxide by exposure to the atmosphere, and imparted the colour to the zinc. The quantity of iron is certainly very small, but that it exists is evident by the blue tint imparted on addition of ferrocyanide of potassium to the solution of the compound in weak hydrochloric acid. From observing that these metals cannot be entirely separated from each other by this method, I did not examine the filtrates containing the greater part of the cobalt, nickel, or iron, for zinc. As a very small quantity of this metal in tartaric acid affords an immediate precipitate upon the addition of sulphuretted hydrogen, it is probably entirely thrown down by that re-agent carrying with it small traces of the other metals of the group, with the exception of manganese, when they happen to be present, although in what state these latter are precipitated experiment alone can decide.

Estimation of Carbon by Combustion, when Sulphur is Present, by M. L. Carius.

In the elementary analysis of sulphuretted organic combinations we know that the estimation of carbon is often erroneous on account of the disengagement of sulphurous acid. The best means of avoiding this source of error is to burn the sulphuretted substances with chromate of lead, and to place a long column of chromate of lead in the anterior part of the tube, which is heated to a dull red; the sulphurous acid which disengages itself in the posterior portion of the tube, where the chromate is in the fused state, is entirely absorbed in the anterior part, where it is fixed in the state of sulphate.—*Annales de Chemie et de Physique*, 3rd Series, vol. lx. p. 504.

On the Reduction of Potassium Compounds by Sodium, by J. A. Wanklyn, *Demonstrator of Chemistry in the University of Edinburgh.*

The reduction of caustic potash by means of sodium, in presence of heated caoutchine, mentioned by Greville Williams in the Chemical News, No. 58, is very remarkable phenomenon, which may justly lead us to ask why we rank potassium as more electro-positive than sodium.

Some time ago a similar fact came under my own observation. I fused some dry acetate of potash in a glass flask, and dropped into the fused salt pieces of sodium. Violent effervescence occurred, the evolved gas containing free hydrogen. The reaction continued on removing the lamp from beneath the flask. Only just towards the last was there any carbonisation. Long after the materials had grown cold, an examination of the product showed that it contained a quantity of the alloy of potassium and sodium, which is liquid at common temperatures.

Like Greville Williams, I was led to view processes of this kind as an economical source of potassium, and went even so far as to propose the method to a manufacturer. If the alloy of potassium and sodium were wanted in large quantities, the reduction of acetate of potash by sodium would be far preferable to the ordinary plan.

On the Elementary Analysis of Organic Compounds by M. L. Carius.

When readily-oxidisable organic substances, such as tartaric acid, alcohol, and amylic alcohol, are heated in closed tubes with nitric acid density 1·4, the oxidation of these substances takes place at a temperature a little above 100° C., and they are rapidly transformed into water and carbonic acid. The complete oxidation of these matters by the aid of nitric acid, of a density of 1·12 to 1·2, requires a temperature of from 120° to 150° C. Phenylic alcohol and naphtaline are oxidised with difficulty under these conditions. Nevertheless, they may be completely oxidised if heated for six or eight hours at 150° C., and finally at 180° C.

A very exact method for estimating the sulphur, chlorine, and phosphorus contained in organic matters may be founded on these reactions. The method may be thus practically applied:—Weigh the substances to be analysed in small and slight glass bulbs, hermetically sealed. Introduce the liquids, with as little air as possible, into a glass bulb which has been blown in the centre of a glass tube, drawn to a fine point at each end, and turned at right angles. In order to introduce a solid body into a bulb, commence by forming it at the extremity of a tube; then fill it with the pulverised substance; lastly, draw out the tube and close the capillary end. The quantity submitted to analysis may vary from 0·15 to 0·40 grammes. The bulb and its contents are introduced into a glass tube closed at one end, of 10 to 12 millimètres interior diameter, and sufficiently long so that the empty part is of the same capacity as that filled with acid.

The acid employed should have a density of 1·20, its quantity varying from 20 to 60 times the weight of the substance. This should be regulated according to the richness of the substance to be analysed in combustible principles. When the substance and acid are in the tube, thicken the extremity before the blow-pipe, then draw it out, so as to form a capillary tube with thick sides. Then boil the acid, and when the vapours have completely dispelled the air, close the capillary tube. When cold, shake the liquid briskly, so as to break the ends of the bulb. Heat the tube, thus arranged, in an air-bath. For this purpose employ the following apparatus:—A rectangular sheet-iron case, length 0·33, height 0·1 millimètres, one of the long inner sides of which is furnished with a sliding-door. Pierce the front vertical side with four round openings, which can be closed with buttons turning on an axis. Insert in each opening a cast-iron tube, from 20 to 22 millimètres in diameter, closed at one end, and placed obliquely in the case. Small openings made in the sides of the case, between the tubes, admit of the introduction of thermometers. Place this case on the large ring of a retort-stand, and fix it with another ring which grasps the top of it. The apparatus thus disposed, slide the glass tube into one of the cast-iron tubes dipping into the air-bath, and heated with a gas-lamp from 120° to 140° C., the openings of the tubes being turned towards a corner of the laboratory. Under this arrangement, any explosions, should they occur, are perfectly harmless, the pulverised glass being projected against the wall. The operation finished, it is necessary to open the tube, which requires some precaution, on account of the accumulation of gases resulting from the combustion of the organic matters. After having carefully expelled the small quantity of liquid contained in the tapering point of the tube, make it red-hot, when it puffs up, and the gases quietly blow out.

Estimation of Sulphur.—This element is contained under the form of sulphuric acid in the acid liquid which the preceding experiment yielded. The sulphuric acid can be directly precipitated by chloride of barium. The precipitate will contain traces of nitrate of baryta; it must, therefore, be treated with hydrochloric acid and washed in water after calcination.

Sulphuretted organic compounds cannot well be completely oxidised by this process at the above-mentioned temperature. Some there are which, when oxidised by nitric acid, are converted into acid ethers of sulphurous acid, and which are not completely transformed into sulphuric acid at 120° or 140° C.

Ethyl- and naphtyl-sulphurous acids are among the most refractory bodies in this respect. If nitric acid of a density of 1·2 is employed, these bodies cannot be oxidised sufficiently to transform sulphur into sulphuric acid, except by maintaining the temperature, during two or three hours, at 180° to 200° C. But it is not advisable to employ this process when it requires such very high temperatures. To analyse the substances in question, dissolve them in nitric acid of a density of 1·2; neutralise the liquid by carbonate of soda; then add excess of carbonate of soda; evaporate, and melt in a silver crucible. Finish the analysis by the usual processes.

Estimation of Phosphorus and Arsenic.—Phosphorus is found, in the state of phosphoric acid, in the nitric liquid which has been exposed to a temperature of from 120° to 130°. This acid is estimated in the state of ammonio-magnesian phosphate by the ordinary process. The arsenic of organic combinations, under the same conditions, passes to the state of arsenic acid, which is estimated as ammonio-magnesian arseniate, and is weighed after being dried at 100°.

Estimation of Chlorine.—When chlorinetted compounds are oxidised by nitric acid by the above method, one part of the chlorine is converted into hydrochloric acid, but the greatest part remains free at the end of the operation. To estimate the chlorine, turn the cooled tube, so as to have the point downwards, plunge it into a vessel containing a solution of sulphite of soda,[1] and break it carefully. The liquid in the tube is thus forced into the solution in a very fine thread, and the chlorine in contact with the sulphite is immediately converted into hydrochloric acid. When the equilibrium of pressure is established, drive a certain quantity of gas out of the tube by heat; then allow the liquid to re-enter the tube; wash it; then heat it for an hour or two to drive off all the sulphurous acid. As it is extremely acid, it will be convenient to almost entirely neutralise it with carbonate of soda before precipitating it by nitrate of silver.

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[1] From 8 to 10 parts of sulphite of soda to 1 part of the analysed substance are employed, and the salt is dissolved in 40 parts of water to 1 part of the nitric acid used for oxidation.

Estimation of Bromine and Iodine.—Bromine combinations, when heated in a closed vessel with nitric acid, behave like the chlorine compounds. Only the bromine appears to be entirely liberated. This operation ended, open the end of the tube under a solution of sulphite of soda, and terminate the analysis in the manner just indicated.

The iodine of iodised combinations is deposited in an entirely solid state, and crystallises in the tube where the oxidation is effected. Open this tube while the point is red-hot, as has before been pointed out; but before doing so place it in a refrigerating mixture, in order that the disengaged gas may not carry off any considerable quantity of the iodine vapour. Then put the liquid and iodine in a vessel containing a solution of sulphite of soda, the quantity and concentration of which is to be calculated as has been already stated. The iodine dissolves rapidly, and is transformed into hydriodic acid, which can be precipitated as iodide of silver, after being evaporated in a water-bath and neutralised by carbonate of soda.

Estimation of Metals.—The process indicated is easily applicable to the determination of metals contained in organic combinations. When the metals are dissolved and the liquid contains no phosphoric acid, they can be precipitated by appropriate re-agents,—carbonate of soda, for instance. The estimation is rather more difficult when the metals separate under the form of insoluble compounds. In the latter case, the greatest care must be taken to eliminate the pieces of glass caused by the breaking of the capillary tube.—*Annales de Chemie et de Pharmacie*, 3rd Series, vol. lx. p. 497.

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. Bowditch.

(Continued from page 54.)

Sulphocyanide of ammonium may be obtained in considerable quantity from an alcoholic solution of foul clay. Upon one occasion I obtained nearly an ounce, in a fair state of purity, from less than a quart bottle of foul clay; and so tenaciously does clay retain this compound, that from some clay which had been exposed to the full action of the weather in a field for two years, I obtained a considerable coloration with perchloride of iron. Sulphocyanide of ammonium may be obtained from gas which has been purified by oxide of iron, by passing the gas through spirit of wine and evaporating.

When common yellow brick-clay is used in the purification of coal-gas, the solutions from it always contain salts of iron; but they never become of a blood-red colour until a mineral acid is added. When, however, the solutions are evaporated, and the deliquescent residue is exposed to the air, most, and sometimes all of the iron is peroxidised, and yields the well-known reaction.

The nitrogen in tar may be shown from the spirit-solution off foul clay. The spirit, evaporated to dryness and allowed to stand, deposits tar and a mixture of deliquescent crystalline salts. They were allowed to deliquesce, the fluid was removed, and the residual tar well washed with water. Subsequently it was dissolved in hot spirit, precipitated by water, and well washed. When nothing more was removed by washing, the tar was heated, and evolved sulphide of hydrogen and ammonia. Contrary to every other compound in gas with which I have experimented, this tar gave off sulphide of hydrogen before ammonia. In other instances I have found the nitrogen compound evolved at a lower temperature than the sulphur one.

Mineral matter derived from the clay is found in all solutions; but as my object in this paper is to speak only of substances in gas, I purposely omit those united with them derived from the clay. For the same reason I make no mention of the value of the foul clay as a manure.

Sulphur Compounds in Gas purified so as not to affect Basic Acetate of Lead, and their removal.—A recent Royal Commission on lighting picture-galleries has stated the large quantity of sulphur found in some London gas, and has intimated a doubt about the possibility of its removal. Dr. Letheby concludes, from seven years' examination of gas in London, that though quite free from sulphide of hydrogen, it contains, on an average, 200 grains of sulphur in 1000 cubic feet; and Dr. Frankland, in the new edition of "Ure's Dictionary of Arts," Part iv. pp. 730, 731, writes, "It is probable that volatile organic compounds of sulphur are produced by the action of this element with carbon and hydrogen simultaneously, although we have as yet no positive evidence of their presence in illuminating gas. . . . When once generated with coal-gas, all attempts to remove these constituents have hitherto proved ineffectual, and there seems little ground for hope that any practicable process will be devised for their abstraction." I have now the honour to submit evidence of the existence of these sulphurised compounds, and also a practical process for their removal. My attention was specially drawn to this subject by a conversation with the manager of a London gas-works. He informed me that he not unfrequently filled his gas-holders with gas which would not affect acetate of lead, and that after the gas had been stored a few hours it became so foul as to blacken lead-paper the instant it was applied. He sought an explanation of this phenomenon; and as the water of his gas-holder tanks was clean, and there were no accidental sources of sulphide of hydrogen, I concluded that an organic compound containing sulphur and hydrogen had been broken up, and that the sulphide of hydrogen was thus produced. I learnt, also, by other observations, that gas which went to the gas-holders free from ammonia, sometimes became ammoniacal if kept, and joining this fact with the former one, inferred that the compound which thus broke up contained nitrogen as well as sulphur and hydrogen. Subsequently I observed that the saturated clay taken from the purifiers of gas-works contained a quantity of foul naphthalin. This led me to procure a quantity of (so-called) naphthalin which had been taken from the mains of a London gas-works, and which, therefore, must have been deposited by purified gas. Some portions of this naphthalin were white, but others were slightly darkened by the presence of carbonaceous matter, and the whole was in fine powder, aggregated together by the process of deposition. The tendency to form exceedingly small crystals seems a constant characteristic of naphthalin which has been deposited in gas pipes, for by no amount of care and trouble have I been able to obtain it in large crystals, though the solutions from which it has crystallised have been months in evaporating. With naphthalin from tar, on the contrary, I have obtained, from an etherial solution, crystals an eighth of an inch thick, nearly half-an-inch broad, and more than half-an-inch in length. The supposed naphthalin from gas-pipes dissolves wholly in ether and hot alcohol, and crystallises from the spirit on cooling as pure naphthalin does. The solutions are neutral to test-papers. Boiled with an alcoholic solution of potash it evolves no ammonia, and with hydrochloric acid no sulphide of hydrogen. Heated alone, it evolves first ammonia, and then sulphide of ammonium, mixed, I think, with a trace of bisulphide of carbon, and then distils. Several samples began to give off their ammonia at 388° F., and sulphide of hydrogen at 422° F., leading to the hope that here was a compound of definite composition which would admit of correct analysis and perhaps of formulation; but some more of the naphthalin, produced, like the other, from Newcastle coal, but at another gas-works, possessed such different physical properties as to convince me that very much more must be known of this substance before any reliable analyses can be published. The latter sample gave a neutral solution in spirit, like the other, but of a considerably browner colour. When heated alone it gave off ammonia with ebullition at 218° F., and then became tranquil. When the temperature reached 375° F., it began to evolve sulphide of hydrogen, which continued to increase in quantity up to 390° F., when it nearly ceased, and quite ceased at 410° F. The proportion of tar in this sample was much greater than I have seen it in any other. Subsequently I obtained some more naphthalin, which had been deposited in the pipes of another London gas-works, and this, like the former, contained both nitrogen and sulphur, which were evolved upon distillation as sulphide of hydrogen and ammonia.

Having thus obtained one sulphurised hydrocarbon, and determined the temperature at which its sulphur and nitrogen could be obtained as easily-removable compounds, I was prepared to advance towards a better purification of gas with great probability of success. Another well-confirmed observation helped to guide me. Gas freed from every trace of sulphide of hydrogen always blackens lead-paper strongly when passed through clay; and if it be subsequently passed through lime, it affects turmeric though quite free from ammonia when taken for experiment. This process may be repeated through a series of ten or twelve purifiers containing clay and lime placed alternately, the test-papers being less affected at each purifier, until at length they are not discoloured at all. This experiment has been made upon gas produced in various parts of England and Scotland from many kinds of coal, and I think the number of instances sufficient to justify the conclusion that *all* gas, as sold, contains the compound from which clay liberates sulphide of hydrogen. I have not yet been able to separate the compound upon which clay thus acts. I have, however, ascertained that the clay which has liberated sulphide of hydrogen from gas which did not affect test-papers when taken for experiment contains tar, which may be dissolved out by alcohol, and may be obtained alone by evaporating the solvent.

(To be continued.)

On the Preparation of Starch from Cassava, by JOSIAS CORK.

THE growth of cassava in quantity has had my best, my most earnest consideration. If it cannot, in every sense of the word, compete with other staples, now, or which may be grown, my praise of the root must receive qualification.

First, then, cassava is alike at home in the mountains and in the lowlands; it grows readily everywhere. The least moisture in the soil is sufficient to induce vegetation at the time of planting, and, once growing, no drought destroys, it simply for the time retards the crop.

But, if general experience does not fully corroborate this positive statement,—and there are lands of so dry a nature that a long drought does kill the cassava plant, and I will not say there may not be such lands anywhere, as I can only decisively speak of what I have noted in my own experience,—still this evil, if it anywhere exists, is less in the cassava than in any other crop I know.

For, secondly, the propagation, and, therefore, also, the supply of a field partially injured by a drought, is about the easiest thing a planter has to do. To grow corn, pulse, yams, and many other things, a portion of valuable produce must be used for seed. Not so the cassava. All that is needed is, to take a cassava stick, and mark off a length containing three or four "eyes;" pass a knife round and cut just below the skin, and then use a slight breaking power with both hands. Each bit is now a future plant; the eye or two below the soil serve for future roots; the eye or eyes above put forth leaves and stalks. It must be evident, then, that a bundle of cassava sticks, which anyone can carry under his arm, will suffice for a considerable breadth of prepared ground, and the time and labour to cut up and plant is almost the least possible to be conceived for such operations, and the cost is positively nothing. Anybody, anywhere, who has cassava growing, can give readily a few cassava sticks to anybody and everybody, and yet have enough left for himself, and to spare.

Thirdly.—Another fine feature is this:—It is not an exhausting crop. Like other productions grown for a substance which appears to derive its constituents from the atmosphere, or composed, like sugar, of carbon, hydrogen, and oxygen, you may grow and re-grow, with change, of course, or some rest, or a slight manuring, on the same lands *ad infinitum*. In fact, everybody feels that land is always fit for cassava, however unfit it may have become for other productions.

Fourthly.—This is a very accommodating crop. The roots ripen and are fit to be manufactured, or remain and grow, and again ripen, and again remain and grow. Although there is not a positive certainty as to the exact time, for the changes from rain to dry weather are uncertain, still there is a choice of a whole year or more for the grower. If the roots be not dug when they become dry and mealy, they soon acquire what is termed a "fresh spring,"—that is, the roots become somewhat watery, and enlarge, and then ripen. This goes on from the age of about ten months to more than two years. I do not know from experience beyond twenty-four months, but it is evident to me that it is not the limit, or anything like it. All the while the roots have been enlarging, so that there has been, as far as I can judge, no loss from delay. I have known a field of cassava cut down to near the roots in a most ruthless way by a lazy fellow who desired a field of corn and would not prepare fresh land. I have seen the crop of corn, and the crop of cassava again, covering by vigorous stems and leaves the ground, and, after a while, the ripe crop of cassava. Of course, this system is the *acme* of laziness, and to be wholly condemned, but, nevertheless, the fact stands forth, as a proof of the extreme hardihood and wonderfully-accommodating nature of the cassava crop.

Fifthly.—Quantity, as I have proved, or sought to prove, is, as other root crops, very large. I need not, therefore, enlarge on this head, but pass on to manufacture.

There again the cassava says "Try me—try me: you will find me, in parts, and in whole, a crop worth your best care and attention." On this head I observe,—

First.—Each root has to be washed, and the cuticle must be taken off. The plain surface of the root makes the washing easy; the soft nature of the substance renders the skinning process as easy as easy can be. The comparison in favour of cassava, as against arrowroot, is in both cases fully as three to one in favour of cassava.

Secondly.—The grating is also in favour of cassava, as compared to arrowroot, in almost an equal rate. But,

Thirdly.—I would not grate at all; but quote from pages of a known scientific work, and I suppose the site of operations to be the old works of a thrown up sugar estate, and unfortunately many such a spot can too readily be found. The manufacture is arrowroot, but of course the same works would equally do for cassava, and I may just observe that the manufacture is in the Island of St. Vincent, and I have no doubt its completeness is the cause of the superior price obtained for the St. Vincent arrowroot, which is just threefold the price of Jamaica arrowroot in the English market:—

"The skinned roots are thrown into a large cistern, with a perforated bottom, and then exposed to the action of a copious cascade of water. The cleansed roots are next put into the hopper of the mill, and are subjected to the powerful pressure of two pairs of polished rollers of hard brass, the lower pair of rollers being set much closer than the upper. The starchy matter is thus ground into a pulp which falls into the receiver placed beneath, and is thence transferred to large and fixed copper cylinders, tinned inside, and perforated at the bottom with numerous minute orifices like a kitchen drainer. Within these cylinders, wooden paddles are made to revolve with great velocity, by the power of a waterwheel, at the same time that a stream of pure water is admitted from above. The paddle arms beat out the fecula from the fibres and parenchyma of the pulp and discharge it in the form of a milk through the perforated bottom of the cylinder. This starchy matter runs along pipes, and then through strainers into large reservoirs, where, after the fecula has subsided, the supernatant water is drawn off. The drying-house on the Hopewell estate is constructed like the hot-house of an English garden, but instead of plants it contains four dozen of drying-pans made of copper, seven and a-half feet by four and a-half, and tinned inside. Each pan is supported on a carriage, and they run on rails; immediately after sunrise these carriages, with their pans covered with white gauze to exclude dust and insects, are run out into the open air, but if rain be apprehended they are run back under the glazed roof. In about four days the fecula is thoroughly dry and ready to be packed."

What is done in St. Vincent may be done in Jamaica. The trash-houses of the past could easily be converted into drying-houses for the future on any old sugar estate having a supply of water. The water which might easily be made to turn a cassava mill with two pairs of rollers, the boiling, curing, and still houses might all be transformed and brought into use for cassava; and with the ingenious contrivance of our Island architect, Mr. Leahy, of having blades of glass, the drying-house might be thoroughly ventilated. I would not only have a glass roof, but for the most part glass walls, with thorough ventilation, and thus lessen the time to the minimum for thorough drying.

The cost, then, of transformation would be very moderate indeed. The metal of the sugar boilers and still would pay, or nearly pay, for the vessels required for the cassava.

There is no duty on glass manufacture, and large slabs of thick glass cost little in England. The cost of manufacture of starch and blocking the fibre, would certainly be less than the cost of sugar manufacture.

To anyone, then, who will turn his attention to cassava he would certainly have a prospect before him of making an old sugar estate a handsomely paying concern as a cassava plantation.—*Jamaica Guardian.*

On the Alloys of Copper and Zinc,

by Frank H. Storer.

(Continued from page 53.)

Pieces of the alloy, weighing from 4 to 12 grammes, accordingly as they contained a greater or less amount of copper, which had been carefully cleaned by filing and brushing, were placed in covered porcelain dishes, and dissolved in strong nitric acid on the sand-bath, the usual precautions to avoid loss from spattering being observed. To the solution thus obtained, sulphuric acid —which had been freed from its impurity of lead by dilution with water and subsequent partial concentration—was added, and the mixture evaporated to dryness over the water-bath; more sulphuric acid being added, and the solution again evaporated until all the nitric acid was expelled, or till no trace of blue crystals remained. I have rarely found it necessary to evaporate the solution more than twice in order to effect this.

It is of importance that the nitric acid be thoroughly removed, for in the presence even of a mere trace of this acid it is exceedingly difficult to precipitate, by means of iron, the last portions of copper from its solution.

After all the nitric acid had been expelled, the mixed sulphates were dissolved in hot water, and the sulphate of lead—from impurity in the zinc of the alloy—allowed to settle. This was then separated and washed with acidulated water, by decantation.

The solution, which had been collected in a capacious beaker, was now largely diluted with water and placed upon a sand-bath, the heat of which was regulated so that the solution could attain a temperature nearly equal to boiling, without, however, coming into actual ebullition. A sheet of the purest iron obtainable was next placed in the solution, leaning against the sides of the beaker, so that the largest possible amount of its surface should be exposed, in order that the iron might be attacked equally at all points by the acid, and that the copper should be precipitated in even sheets upon it.

The solution should be acid enough to ensure a slight evolution of hydrogen during the precipitation. Care should, however, be taken to guard, in the first instance, against the addition of too great an excess of acid, lest the iron be corroded to an unnecessary, or even ruinous extent. It is of course desirable to avoid dissolving any more iron than is necessary to insure regularity and completeness in the precipitation; for in this case a smaller portion of its impurities will be exposed, and its surface will remain smoother.

After the introduction of the iron, the beaker is to be covered with a glass plate, and left to itself until the copper is entirely precipitated, which occurs at the end of an hour or two at furthest. Having proved that no more copper is contained in the solution, by testing a small portion of it with sulphuretted-hydrogen water, the beaker is removed from the sand-bath, and the clear acid solution decanted off as completely as possible from the copper and iron.

If this solution be diluted with a large quantity of hot water, it immediately becomes yellow, then turbid, and in a few minutes deposits a bulky, flocculent precipitate of a basic salt of peroxide of iron, although the supernatant liquid is strongly acid. Since a quantity of this solution remains adhering to the metals, it is well, in order to avoid the precipitate which would form if they were treated at once with hot water, first to pour a small quantity of cold water upon them. The piece of iron is then to be taken from the beaker, and any particles of copper which may be loosely attached to it removed by means of the wash-bottle; the iron being carefully preserved for subsequent examination. The copper having been once more rinsed with cold water, is now to be treated with boiling water, which should be decanted off into a large, clean beaker, without waiting for all the copper to be deposited, as soon as the liquor assumes a yellow colour. After two or three additions of hot water, the subsalt of iron is no longer precipitated, and the copper may be allowed to subside at its leisure. Any particles of copper which are found adhering to the sheet of iron which was used for the precipitation must now be removed by gently rubbing them with the finger, care being taken not to disturb the black coat[1] which is found adhering to the iron any more than can be avoided; they are then to be washed into a small porcelain capsule, into which is also brought the mixed deposit of subsalt of iron and particles of copper collected during the first rapid washing of the copper. The contents of the capsule are now treated with dilute chlorhydric acid, in which the subsalt is readily soluble, washed with water, and added to the original copper, the washing of which is to be continued till the last traces of acid are removed. The copper, having been transferred to a porcelain boat, was dried at 100° C., ignited in a current of hydrogen gas, and subsequently weighed.

The iron used in these assays was that known as the "best Russia sheet." In order to obtain the best possible pieces, I have followed the custom of copper assayers in allowing a sheet-iron worker to cut up a number of pounds of Russia sheet into bits of about two and a-half by three and a-half inches, this size having been found well adapted to the work at hand, smaller pieces being unequally corroded; from this supply only those pieces which presented a perfectly smooth and even surface were selected for use. Those pieces which are at all corrugated or uneven ought to be rejected, for such are acted on unequally by the acid, and copper would be lost in the cavities thus formed.

Since the sharp angles of these rectangular bits of iron are liable to be corroded very much by the acid, in which case particles of iron might drop off and contaminate the copper, it is best to remove them by means of a rasp, and to make the corners round and smooth.

The glazed coating which occurs upon the surface of Russian sheet-iron, and which, according to Wells,[2] consists of silicate of iron, is readily removed by digestion in moderately dilute chlorhydric acid, in which it soon falls off in flakes. After washing with water, the bits of iron are dried and preserved for use.

In conducting the assay it is of importance that the solution should be dilute, since the precipitation of the copper in this case goes on more regularly, and is sooner completed; it should be warm, not only that the copper may be precipitated more rapidly, but in order to avoid the formation of a basic iron salt, which is very liable to contaminate the copper when it is precipitated in the cold. This basic salt frequently forms during the precipitation, even in warm solutions, if they are not sufficiently acidulated. If a solution be at the same time somewhat concentrated, and not sufficiently acid, it often happens that a portion of the copper becomes so firmly attached to the iron that it cannot be rubbed off. This accident will rarely occur, however, to any one accustomed to the assay.

It is remarkable that no one who has written upon this process has, so far as I can ascertain, appreciated the necessity of igniting the dry copper in a current of hydrogen before weighing it. That this operation is of the first importance is, however, well known to the analysts of this vicinity, who have long been accustomed to apply in practice.[3] It is necessitated less, as it appears to me, by the oxide of copper formed during drying,—for the bright surfaces of the copper precipitated by this method remain untarnished when rapidly dried at temperatures even as high as 110° to 115°,—than by an impurity consisting of organic matter derived from the iron; either carbon, or perhaps some one of the offensive hydro-carbons which are generated during the action of acids upon iron, which may adhere to the spongy copper. At all events, if a portion of the precipitated copper which has been dried at 100°—or at any temperature lower than that at which oxidation occurs—be placed, as was suggested to me by Mr. Dickinson in a bulb tube with narrow outlet, such as is used in testing for arsenic, and heated in the flame of a spirit-lamp, it will be found that a quantity of water collects in the narrow part of the tube, while the copper becomes lustrous and assumes the bright red colour proper to it. At first sight it would appear as if this behaviour was produced by the reaction of a portion of the spongy copper upon the oxide of copper with which it might be soiled, in which event a small quantity of suboxide of copper would be formed, the colour of which could hardly mask, to any extent, that of the pure copper. But on examining the reaction more attentively I have found that carbonic acid as well as water can readily be detected in the atmosphere expelled from the tube in which the copper is heated; a distinct empyreumatic odour is at the same time perceptible. I have also several times observed traces of ammonia. This gas is always evolved in considerable quantity when the dirty copper is heated in an atmosphere of hydrogen; the aqueous vapour driven off in this case being strongly alkaline from its presence. Organic matter must therefore have been attached to the spongy copper, and the lustre which the latter acquired when heated may have been occasioned by the simple expulsion of the impurity, or in part by the reduction of oxide of copper by it.

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[1] The black crust which forms on the iron beneath the layer of copper contains a small amount of carbon and some silica, but consists for the most part of what appears to be a compound of silicic acid and oxide of iron. This substance, called "slag" by Morfit and Booth in their very able "Report upon the Chemical Analysis of Cast-iron Gun-metal" (*vide* "Report of Experiments, on Strength, &c., of Metals for Cannon, by Officers of the Ordnance Department, U.S Army" [Philadelphia, Baird, 1856, 4to], p. 408), is but sparingly soluble in dilute chlorhydric acid; slowly soluble in cold, more rapidly in hot concentrated chlorhydric acid; easily soluble in concentrated nitric acid, especially when hot; with separation of silica in both cases. It is slowly decomposed by strong soda lye. This "slag" is formed in considerably larger quantity when sulphuric acid is used than when chlorhydric acid is substituted for it in the assay. As it usually adheres, however, to the iron with considerable firmness, the copper can generally be rubbed off without disturbing it materially.

[2] "Proceedings of Boston Society of Natural History," iv. p. 296.

[3] In lieu of this, some assayers convert the precipitated copper into oxide, and weigh the latter.

That the phenomenon does not depend upon the presence of free carbon seems to be proved by the fact that portions from the exterior of a mass of spongy copper, which had been protected from contact with the iron by a layer of copper nearly an inch in thickness, were found to exhibit reactions similar to those just described, water and empyreuma being evolved when the dry copper was heated in a closed tube. Indeed, the formation of water in any case in Mr. Dickinson's experiment with the arsenic-tube is an insuperable objection to this explanation, as well as to the idea that mixed copper and oxide of copper have reacted upon each other.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. Faraday, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

Lecture V. (Jan. 5, 1861.)

Oxygen present in the Air—Nature of the Atmosphere—Its Properties—Other Products from the Candle—Carbonic Acid—Its Properties.

We have now seen that we can produce hydrogen and oxygen from the water that we obtained from the candle. Hydrogen, you know, comes from the candle, and oxygen, you believe, comes from the air. But then you have a right to ask me, "How is it that the air and the oxygen do not equally well burn the candle?" If you remember what happened when I put a jar of oxygen over a piece of candle, you recollect there was a very different kind of combustion to that which took place in the air. Now, why is this?—it is a very important question, and one I shall endeavour to make you understand; it relates most intimately to the nature of the atmosphere, and is most important to us.

We have several test of oxygen besides the mere burning of bodies; you have seen a candle burnt in oxygen, or in the air; you have seen phosphorous burnt in the air, or in oxygen, and you have seen iron filings burnt in oxgen. But we have other tests besides these, and I am about to refer to one or two of them for the purpose of carrying your conviction and your experience further. Here you have a vessel of oxygen. I will show its presence to you: if I take a little spark and put it into that oxygen you know by the experience you gained the last time we met, what will happen,—if I put that spark into the jar it will tell you whether we have oxygen here or not. Yes! We have proved it by combustion; and now here is another test for oxygen, which is a very curious and useful one. I have here two jars full of gas, with a plate between them to prevent their mixing; I take the plate away, and the gases are creeping one into the other. "What happens," say you, "they together produce no such combustion as was seen in the case of the candle." But see how the presence of oxygen is told by its association with this other substance. What a beautiful, curious gas I have obtained in this way, showing me the presence of the oxygen. In the same way we can try this experiment by mixing common air with this test-gas. Here is a jar containing air—such air as the candle would burn in, and here is a jar or bottle containing the test-gas. I let them come together over water, and you see the result: the contents of the test-bottle are flowing into the jar of air, and you see I obtain exactly the same kind of action as before, and that shows me that there is oxygen in the air,—the very same substance that has been already obtained by us from the water produced by the candle. But then, beyond that, how is it that air does not burn the candle as well as oxygen will? We will come to that now. I have here two jars: they are filled to the same height with gas, and the appearance to the eye is alike in both, and I really do not know at present which of these jars contains oxygen and which contains air, although I know they have previously been filled with these gases. But here is our test-gas, and I am going to work with the two jars, in order to examine whether there is any difference between them in the quality of reddening this gas. I am now going to turn this test-gas out into one of the jars, and observe what happens:—There is redenning you see; there is then oxygen present. We will now test the other jar, but you see this is not so bright, not so red, not so distinct, as the first; and, further, this curious thing happens, if I take these two gases and shake them together well with water, we shall absorb the red gas; and then if I put in more of this test-gas and shake again we shall absorb more, and I can go on as long as there be any oxygen present to produce that effect. If I let in air it will not matter, but the moment I introduce water, the red gas disappears, and I may go on in this way, putting in more and more of the test-gas, until I come to something left behind which will not redden any longer by the use of that particular body that rendered the air and the oxygen red. Why is that? You see in a moment it is because there is, besides oxygen, something else present which is left behind. I will let a little more air into the jar, and if it turns red you will know that some of that reddening gas is still present, and that, consequently, it was not for the want of this producing body that that air was left behind.

Now, you will begin to understand what I have got to say. You saw that when I burnt phosphorus in a jar, as the smoke produced by the phosphorus and the oxygen of the air condensed, it left a good deal of gas unburnt, just as this red gas left something untouched,—there was, in fact, this gas left behind which the phosphorus cannot touch, which the reddening gas cannot touch, and this is something which is not oxygen, and yet is part of the atmosphere.

So that is one way of opening out air into the two things of which it is composed,—oxygen, which burns our candles, our phosphorus, or anything else, and this other substance which will not burn them. This other part of the air is by far the largest part. Now, this substance is a very curious thing when we come to examine it; it is remarkably curious, and yet you say, perhaps, that it is very uninteresting. It is uninteresting in some respects because of this,—that it shows no bright appearance of combustion. If I test it with a taper as I do oxygen and hydrogen, it does not burn like hydrogen, nor does it make the taper burn like oxygen. Try it in any way I will, it does neither the one thing or the other; it will not take fire: it will not let the taper burn; it puts out the combustion of anything. There is nothing that will burn in it in common circumstances. It does not smell: it is not sour; it does not dissolve in water; it is neither an acid or alkali; it is as indifferent to all our organs as it is possible for a thing to be. And you might say, "It is nothing; it is not worth chemical attention; what does it do in the air?" Ah! then come our beautiful and fine results shown us by an observant philosophy. Suppose, in place of having nitrogen, or nitrogen and oxygen, we had pure oxygen as our atmosphere, What would become of us? You know very well that a piece of iron lit in a jar of oxygen goes on burning to the end. When you see a fire in an iron grate, imagine where the grate would go to if the whole of the atmosphere were oxygen. The grate would burn up more powerfully than the coals; for the iron of the grate itself is even more combustible than the coals which we burn in it. A fire put into the middle of a locomotive would be a fire in a magazine of fuel, if the atmosphere were oxygen. The nitrogen lowers it down and makes it moderate and useful for us, and then with all that it takes away with it the fumes that you have seen produced from the candle, disperses them throughout the whole of the atmosphere, and carries them away to places where they are wanted to perform a great and glorious purpose of good to man, for the sustenance of vegetation; and thus does a most wonderful work, although you say, on examining it, "why it is a perfectly indifferent thing." This nitrogen in its ordinary state, is an inactive element; no action short of the most intense electric force, and then in the most infinitely small degree, can cause the nitrogen to combine directly with the other element of the atmosphere, or with other things round about it; it is a perfectly indifferent, and therefore to say, a safe substance.

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[1] Reported *verbatim* by special permission.

But before I take you to that result, I must tell you about the atmosphere itself; I have written on this diagram, the composition of one hundred parts of atmospheric air:—

	Bulk.	Weight.
Oxygen..	20	22·3
Nitrogen	80	77·7
	100	100·0

it is a true analysis of the atmosphere, so far as regards the quantity of oxygen and the quantity of nitrogen present. By our analysis, we find that 5 pints of the atmosphere contains only 1 pint of oxygen, and 4 pints or 4 parts of nitrogen by bulk. That is our analysis of the atmosphere. It requires all that quantity of nitrogen to reduce the oxygen down, so as to be able to supply the candle properly with fuel, so as to supply us with an atmosphere which our lungs can healthily and safely breathe; for it is just as important to make the oxygen right for us to breathe, as it is to make the atmosphere right for the burning of the fire and the candle.

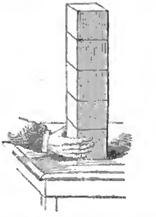
But now for this atmosphere. First of all let me tell you the weight of these gases. A pint of nitrogen weighs 10 grains and $\frac{4}{10}$ths, or a cubic foot weighs $1\frac{1}{6}$ oz. That is the weight of the nitrogen. The oxygen is heavier; a pint of it weighs $11\frac{8}{10}$ grs., and a cubic foot weighs $1\frac{1}{4}$ oz. A pint of air weighs about 10^{7} grs., and a cubic foot $1\frac{1}{5}$ oz.

You have asked me several times, and I am very glad you have, "How do you weigh gases?" I will show you; it is very simple, and easily done. Here is a balance, and here is a copper bottle made as light as we can consistent with due strength, turned very nicely in the lathe, and made perfectly air-tight, with a stop-cock, which we can open and shut, which at present is open, and, therefore, allows the bottle to be full of air. I have here a nicely-adjusted balance in which I think the bottle, in its present condition, will be balanced by the weight on the other side. And here is a

pump by which we can force the air into this bottle, and with it we will force in a certain number of volumes of air as measured by the pump [Twenty measures were pumped in]. We will shut that in and put it in the balance. See how it sinks; it is much heavier than it was. By what? By the air that we have forced into it by the pump. There is not a greater *bulk* of air, but there is the same bulk of *heavier* air, because we have forced in air upon it. And that you may have a fair notion in your mind as to how much this air is, here is a jar full of water. We will open that copper vessel into this jar, and let the air return to its former state. All I have to do now is to screw them tightly together, and to turn the taps, when there, you see, is the bulk of the twenty pumps of air which I forced into the bottle; and to make sure that we have been quite correct in what we have been doing, we will take the bottle again to the balance, and if it is now counterposed by the original weight, we shall be quite sure we have made our experiment correctly. It is balanced; so, you see, we can find out the weight of the extra volumes of air forced in in that way, and by that means we are able to ascertain that a cubic foot of air weighs $1\frac{1}{5}$ oz. But that small experiment will by no means convey to your mind the whole literal truth of this matter. It is wonderful how it accumulates when you come to larger volumes. This bulk of air [a cubic foot] weighs $1\frac{1}{5}$ oz. What do you think of the contents of that box above there which I have had made for the purpose? The air which is within that box weighs one pound—a full pound, and I have

calculated the weight of the air in this room,—you would hardly imagine it, but it is above a ton. So rapidly do the weights rise up, and so important is the presence of the atmosphere, and of the oxygen, and the nitrogen in it, and the use it performs in conveying things to and fro from place to place, and carrying bad vapours to places where they will do good instead of harm.

Having given you that little illustration with respect to the weight of the air, let me show you certain consequences of it. You have a right to them because you would not understand so much without it. Do you remember this kind of experiment: have you ever seen it? Suppose I take a pump somewhat similar to the one I had a little while ago to force air into the bottle, and suppose I place it in such a manner that by certain arrangements I can apply my hand to it. My hand moves about in the air so easily that it seems to feel nothing, and I can hardly get velocity enough by any motion of my own in the atmosphere to make sure that there is much resistance to it. But, when I put my hand here [on the air-pump receiver, which was afterwards exhausted] you see what happens. Why is my hand fastened to this place, and why am I able to pull this pump about? And see! how is it that I can hardly get my hand away? Why is this? It is the weight of the air—the weight of the air that is above. I have another experiment here which I think will explain to you more



about it. When the air is pumped from underneath the bladder which is stretched over this glass, you will see the effect in another shape; the top is quite flat at present, but I will make a very little motion with the pump, and now look at it,—see how it has gone down, see how it is bent in; you will see the bladder go in more and more, until, at last, I expect it will be driven in and broken by the force of the atmosphere pressing upon it [the bladder, at last, broke with a loud report]. Now, that was done entirely by the weight of the air pressing on it, and you can easily understand how that is. The particles that are piled up in the atmosphere stand upon each other, as these five cubes do; you can easily conceive that these five cubes are resting upon the bottom one, and if I take that away the others will all sink down. So it is with the atmosphere; the air that is above is sustained by the air that is beneath, and when the air is pumped away from beneath them, the change occurs which you saw when I placed my hand on the air-pump, and which you saw in the case of the bladder, and which you shall see better here. I have tied over this jar a piece of sheet india-rubber, and I am now about to take away the air from the inside of the jar, and if you will watch the india-rubber—which acts as a partition between the air below and the air above, you will see when I pump how the pressure shows itself. See where it is going to, I can actually put my hand into the jar; and yet this result is only caused by the great and powerful action of the air above. How beautifully it shows this curious circumstance.

Here is something that you can have a pull at when I have finished to-day. It is a little apparatus of two hollow brass hemispheres, closely fitted together, and having connected with it a pipe and a cock, through which we can exhaust the air from the inside; and although the two halves are so easily taken apart while the air is left within, yet, you will see when we exhaust it by-and-by, no power of any two of you wil be able to pull them apart. Every square inch of surface that is contained in the area of that vessel sustains fifteen pounds by weight, or nearly so, when the air is taken out; and you may try your strength presently in seeing whether you can overcome that pressure of the atmosphere.

Here is another very pretty thing,—the boys' sucker, only refined by the philosopher. We young ones have a perfect right to take toys, and make them into philosophy, inasmuch as now-a-days we are turning philosophy into toys. Here is a sucker, only it is made of india-rubber; if I clap it upon the table, you see at once it holds. Why does it hold? I can slip it about, and yet if I try to pull it up, it seems as if it would pull the table with it. I can easily make it slip about from place to place; but only when I bring it to the edge of the table can I get it off. It is only kept down by the pressure of the atmosphere above. Here is a couple of them; if you take these two and press them together, you will see how strong they stick. And, indeed, we may use them as they are proposed to be used, to stick against windows or against walls, where they will adhere for an evening, and serve to hang anything on that you want. I think, however, that you boys ought to have experiments that you can make at home; and so here is a very pretty experiment in illustration of the pressure of the atmosphere. Here is a tumbler of water; suppose I were to propose to you to turn that tumbler upside down so that the water should not fall out, and yet not keep it in by my hand, but merely by using the pressure of the atmosphere; could you do that? Take a wine-glass, either quite full or half full of water, and put a flat card on the top; turn it upside-down, and then see what becomes of the card and of the water. The air cannot get in because the water by its capillary attraction round the edge keeps it out.

I think this will give you a strong notion of what you may call the materiality of the air, when I tell you that that box holds a pound of it, and this room more than a ton, and you will begin to think that air is something very serious. I will make another experiment to convince you of this positive resistance. There is that beautiful experiment of the pop-gun, made so well and so easily you know out of a quill, or a tube, or anything of that kind; where we take a slice of potato for instance, or an apple, and take the tube and cut out a pellet, as I have now done, and push it to one end. I have made that end tight; and now I take another piece and put it in: it will confine the air that is within the tube perfectly and completely for our purpose; and now I shall find it absolutely impossible by any force of mine to drive that little pellet close up to the other. It cannot be done; I may press the air to a certain amount, but if I go on pressing, long before it comes to the second the confined air will drive the front one out with a force something like that of gunpowder; for gunpowder is in part dependent upon the same action that you see in this case.

Here is an experiment which I saw the other day and was very pleased with, as I thought it would serve our purpose here. (I ought to have held my tongue for four or five minutes before I began this experiment, because I depend upon my lungs for the success of it.) By the proper application of air I expect I can drive this egg out of one cup into the other by the force of my breath, but if I fail it is in a good cause, and I do not promise success, because I have been talking more than I ought to do to make the experiment succeed.

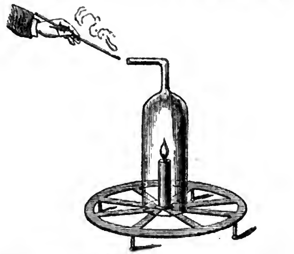
[The Lecturer here tried the experiment, and succeeded in blowing the egg from one egg-cup to the other.]

You see that the air which I blow goes downwards between the egg and the cup, and makes a blast under the egg, and is thus able to lift a heavy thing—for a full egg is a very heavy thing for air to lift. If you want to make the experiment you had better boil the egg quite hard first, and then you may very safely try to blow it from one cup to the others with a little care.

I think I have now kept you long enough upon this property of the weight of the air, but there is another thing I should like to mention. You saw the way in which, in this pop-gun, I was able to drive the second piece of potato half or two-thirds of an inch before the first piece started, by virtue of the elasticity of the air; just as I pressed into the copper bottle the particles of air by means of the pump. Now this depends upon a wonderful property in the air, namely, its elasticity, and I should like to give you a good illustration of this. It is this: if I take anything that confines the air properly, as this membrane, it is able to contract and expand so as to give us a measure of the elasticity of the air, and to confine in it a certain given portion of air: and then, if we take the atmosphere off from the outside of it, just as in these cases we put the pressure on—if we take the pressure off you will see how it will then go on expanding and expanding, larger and larger, until it will fill the whole of this bell-jar, showing you that wonderful property of the air, its elasticity, its compressibility, and expansibility, to an exceedingly large extent; and this is very essential for the purposes and services it performs in the economy of creation.

We will now turn to another most important part of our subject, remembering that we have examined the candle in its burning, and have found that it gives rise to various products. We have the products, you know, of soot, of water, and of something else, which you have not yet examined. We have collected the water, but have allowed the other things to go into the air. Let us now examine some of these other products.

Here is an experiment which I think will help you in part in this way. We will put our candle there, and place over it a chimney, thus. I think my candle will go on burning, because the air-passage is open at the bottom and the top. In the first place, you see the moisture coming—that you know about. It is water produced from the candle by the action of the air upon its hydrogen. But besides that, something is going out at the top: it is not moisture,—it is not water,—it is not condensible; and yet, after all, it has very singular properties. You will find that the air coming out of the top of our chimney is nearly sufficient to blow the



light out I am holding to it, and if I put the light fairly opposed to the current, it will blow it quite out. You will say, that is as it should be, and I am supposing that you think it ought to do so, because the nitrogen does not support combustion, and ought to put the candle out, since the candle will not burn in nitrogen. But is there nothing else there than nitrogen? I must now anticipate,—that is to say, I must use my own knowledge, to supply you with the means that we adopt for the purpose of ascertaining these things, and examining such gases as these. I will take an empty bottle,—here is one,—and if I hold it over this chimney, I shall get the combustion of the candle below sending its results into the bottle above; and we shall soon find that this bottle contains, not merely an air that is bad as regards the combustion of a taper put into it, but having other properties.

Let me take a little quick lime and pour some common water on to it,—the commonest water will do. I will stir it a moment, then pour it upon a piece of filtering paper in a funnel, and we shall very quickly have a clear water proceeding to the bottle below, as I have here. I have plenty of this water in another bottle, but nevertheless I should like to use the lime water that was prepared before you so that you may see what its uses are. If I take some of this beautiful clear lime-water and put that into this jar which has collected the air from the candle, you will see a change coming about. Do you see that that water has got quite milky? Observe, that will not happen with air merely. Here is a bottle filled with air, and if I put a little lime-water into it neither the oxygen nor the nitrogen, nor anything else that is in that quantity of air will make any change in the lime-water, it remains clear and perfect, and no shaking of that quantity of lime-water with that quantity of air in its common state will cause any change; but if I take this bottle with the lime-water and hold it so as to get the general products of the candle in contact with it, in a very short time you see we shall have it milky—there is the chalk, consisting of the lime which we used in making the lime-water, combined with something that came up from the candle,—that other product which we are in search of and which I want to tell you about to-day. This is a substance made visible to us by its action, which is not the action of the lime-water *either* upon the oxygen or upon the nitrogen, nor upon the water *itself*, but it is something new to us from the candle. And then we find this white powder produced by the lime-water and the vapour from the candle appears to us very much like whitening or chalk, and when examined it does prove to be exactly the same substance as whitening or chalk. So we are led, or have been led, to observe upon the various circumstances of this experiment, and to trace this production of chalk to its various causes to give us the true knowledge of the nature of this combustion of the candle,—to find that this substance issuing from the candle is exactly the same as that substance which would issue from a retort if I were to put some chalk into it and make it red hot with a little moisture: you would then find that exactly the same substance would issue from it as from the candle.

But we have a better means of getting this substance, and in greater quantity, so as to ascertain what its general characters are. We find this substance in very great abundance in a multitude of cases where you would least expect it. All lime-stones contain a great deal of this gas which issues from the candle, and which we call *carbonic acid*. All chalks, all shells, all corals, contain a great quantity of this curious air. We find it fixed in these stones, for which reason Dr. Black called it "fixed air,"—finding in these fixed things like marble and chalk,—he called it fixed air because it lost its quality of air, and assumed the condition of a solid body. We can easily get this air from marble. Here is a jar containing a little muriatic acid, and here is a taper which, if I put it to that jar, will show only the presence of common air. There is, you see, pure air down to the bottom; the jar is full of it. Here is a substance—marble, a very beautiful and superior marble, and if I put these pieces of marble into the jar, a great boiling apparently goes on. That, however, is not steam; it is a gas that is rising up, and if I now search the jar by a candle I shall have exactly the same effect produced upon the taper as I had from the air which issued from the end of the chimney over the burning candle. It is exactly the same action, and caused by the very same substance that issued from the candle; and in this way we can get carbonic acid in great abundance,—we have already nearly filled the jar. We also find that this gas is not merely contained in marble. Here is a vessel in which I have put some common whitening-chalk which has been washed in water and deprived of its coarser particles, and so supplied to the plasterer as whitening. Here is a large jar containing this whitening and water, and I have here some strong sulphuric acid, which is the acid you might have to use if you were to make these experiments (only in using this acid with limestone, the body that is produced is an insoluble substance, whereas, the muriatic acid produces a soluble substance that does not so much thicken the water). And you will seek out a reason why I take this kind of apparatus for the purpose of showing this experiment. I do it because you may repeat in a small way what I am about to do in a large one. You will have here just the same kind of action, and I am evolving in this large jar carbonic acid exactly the same in its nature and properties as the gas which we obtained from the combustion of the candle in the atmosphere. And no matter how different the two methods by which we prepare this carbonic acid, you will see, when we get to the end of our subject, that it is all exactly the same, whether prepared in the one way or in the other.

We will now proceed to the next of our experiments with respect to this gas. What is its nature? Here is one of the vessels full, and we will try it as we have done so many other gases—by combustion. You see it is not combustible, nor does it support combustion. Neither, as we know, does it dissolve much in water, because we collect it over water very easily. Then you know that it has an effect and becomes white in contact with lime-water, and when it does become white in that way, it becomes one of the constituents to make carbonate of lime or lime-stone.

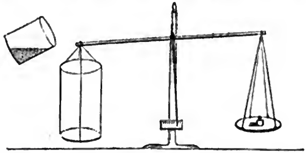
Now, the next thing is to show you that it does dissolve a little in water, and therefore that it is unlike oxygen and hydrogen in that respect. I have here an apparatus by which we can produce this solution. In the lower part of this apparatus is marble and acid, and in the upper part cold water. The valves are so arranged that the gas can get from one to the other; I will set it in action now, and you can see the gas bubbling up through the water, as it has been doing all night long, and by this time we shall find that we have this substance dissolved in the water. If I take a glass and draw off some of the water, I find that it tastes a little acid to the mouth; it is impregnated with carbonic acid; and if I now apply a little lime-water to it, that will give us a test of its presence. This water will make the lime-water turbid and white, which is the carbonic acid test.

Then it is a very weighty gas; it is heavier than the atmosphere. I have put their respective weights at the lower part of this table, along with, for comparison, the weights of the other gases we have been examining:—

	Pint.	Cubic Foot.
Hydrogen	$\frac{3}{4}$ grs.	$\frac{1}{12}$ oz.
Oxygen	$11\frac{9}{10}$	$1\frac{1}{3}$
Nitrogen	$10\frac{4}{10}$	$1\frac{1}{6}$
Air	$10\frac{7}{10}$	$1\frac{1}{5}$
Carbonic acid	$16\frac{1}{3}$	$1\frac{9}{10}$

A pint of it weighs $16\frac{1}{3}$ grs., and a cubic foot weighs $1\frac{9}{10}$ oz., almost two ounces. You can see by many experiments that this is a heavy gas. Suppose I take a glass containing nothing else but air, and this vessel containing the carbonic acid; and suppose I pour a little of this gas into that glass, I wonder whether any has gone in or not; I cannot tell by the appearance, but I can in this was [introduces the taper]. Yes, there it is, you see; and if I were to examine it by lime-water, I should find it in the same way. I will take this little bucket, and put it down into the well of carbonic acid,—indeed we too often have real wells of carbonic acid,—and now, if there is any carbonic acid, I must have got to it by this time, and it will be in this bucket, which we will examine with a taper. There it is, you see; it is full of carbonic acid.

I have another experiment here by which I will show you its weight. I have here a jar suspended at one end of a balance—it is now equipoised, but when I pour this carbonic acid into the jar on the one side which now contains air, you will see it sink down at once because of the carbonic acid that I pour into it. And now I examine this jar with the lighted taper. I shall find that the carbonic acid has fallen into it, and it no longer has any power of supporting the combustion. If I blow a soap bubble, which of course will be filled with air, and let it fall into this jar of carbonic acid, it will float. But I shall first of all take one of these little balloons filled with air. I am not exactly sure where the carbonic acid is, we will just try the depth, and see where abouts is its level. There you see we have this bladder floating on the carbonic acid, and if I evolve some more of the carbonic acid, you will see the bladder lifted up higher. There it goes, the jar is nearly full, and now I will see whether I can blow a soap bubble on that and float it in the same way. [The Lecturer here blew a soap bubble and allowed it to fall into the jar of carbonic acid, when it floated in it midway.] It is floating as the balloon floated by virtue of the greater weight of the carbonic acid than of the air. And now, having so far given you the history of the carbonic acid, as to its sources in the candle, as to its physical properties and weight, when we next meet I shall show you of what it is composed, and where it gets its elements from.



[*The Sixth and Last Lecture will appear in our next Number.*]

THE ROYAL INSTITUTE OF BRITISH ARCHITECTS.

A General Meeting of this Society was held at the rooms of the Institute, on Monday evening last, Jan. 28, 1861.

Mr. Digby Wyatt occupied the chair.

Some preliminary business having been disposed of,

Mr. Tite, M.P., opened a discussion on the various methods which had been proposed for the preservation of the stone used in the construction of the New Houses of Parliament. The notice which had been put upon the paper was, that he would give some account of the processes used by the Ancients for protecting stone or other materials. If he had been asked this time last year whether the Ancients possessed any such processes, or adopted any such practice, he should have contradicted it in the strongest possible terms; and it was only recently that a single incident induced him to modify that opinion. All his experience of their buildings, and certainly all his reading on the subject, would induce him to believe that they made no such attempt to preserve their building-stones, because in their climate no such attempt was necessary. They painted their buildings and statues with colour, it was true; but he believed that no colouring which they used was in the slightest degree intended for the purpose of preventing decay. Recently, however, notice had been given of two processes: one of which had been called *Zopissa*, and the other had a distinct reference to the ancient encaustic. He had found two references to the ancient zopissa; the one in Dioscorides, and the other in Pliny. They both gave a similar description of it as being composed of pitch scraped off old vessels, and which had been exposed to the action of water, united with wax. He had failed to discover any other reference in any of the ancient authors. He considered there was no analogy between it and the material for which the name was now claimed, and the present use of that word was only the employment of a very hard term with a very unintelligible meaning. With reference to encaustic, Vitruvius, a great authority in the matter, gave a very detailed account of its use. From that author it appeared, that encaustic was made from marble burnt into quick-lime, and applied very much the same as plaster in the present day. At the time it was proposed to build the Houses of Parliament, the commissioners wished to obtain a stone which would not be subject to the decay to which Portland stone was liable; they at last found between Derbyshire and Yorkshire, a stone called Dolomite, which was, in point of fact, magnesian limestone, or a compound of carbonic acid, lime, and magnesia. The name Dolomite, was given by the French, in consequence of Dolomer, an eminent mineralogist, having distinguished it from ordinary limestone. The magnesia of commerce was mainly manufactured from this Dolomite. He believed that the main cause of the failure of this stone was, that the specimens employed were not crystalline. When crystalline, the stone was almost imperishable, but when crystallisation had not taken place, decay was very rapid. He then decomposed a portion of the stone by means of sulphuric acid, thereby evolving carbonic acid. What was there done rapidly the sulphuric and sulphurous acids in the air were doing slowly. It was that action which destroyed the stone in London. As an example of the absence of these acids in the air of some countries, he referred to the glittering appearance of the roofs and spires of Swiss edifices. The angles of the spires and the guttering of the roofs of the houses were made of tin; but in London it was known that tin would not preserve its brightness for more than three or four months. It was this effect of the London atmosphere with which they had to deal, and, in point of fact, to guard against. With regard to the remedy, the most obvious means for the preservation of a surface, whether of cement or stone, was to paint it. Another remedy which had been proposed was, to bitumenize it. Specimens of bitumenized stone were to be seen at Tunbridge Wells. But the black appearance of stones so treated rendered the process unfit for the present case. The process was exceedingly valuable for some purposes. The remedies proposed might be classified into bituminous, oleaginous, and silicious, the first of which he had already shown to be inapplicable. With regard to the oleaginous, he cited an instance from his own experience. About 25 years ago, when the great streets of the city of London were being constructed under Sir Robert Smirk, a preparation was patented under the name of Howlet's Cement. The result was beautiful in colour, and the surface which it made admirably compact. This substance was ordered for use, and was composed of sand or ground stone, grey oxide of lead, and red oxide of lead; the red lead being used merely to give the composition a warm colour. These were united with linseed oil, and laid in the form of paste on the front of the building. He submitted a portion to Mr. Faraday for examination, who showed that it was nothing more than a coarse paint, and that, like any other paint, it would decompose; and demonstrated his opinion by the simple experiment of burning a piece of the cement in a candle. "That which the candle will do readily," said Mr. Faraday, "the air will do sooner or later." Another of the oleaginous class was the patent of Mr. Szerelmey. It was dated in 1857. He describes his invention as a preparation for rendering walls and other structures waterproof. There was no direction as to how the composition was to be used, but all the patentees claimed the application of their patents to stone. With regard to silicious methods, the first process was patented by Mr. W. Edward Newton; but it appeared that that specification was merely the patenting of all Kohlmann's processes. The only other process to which he would refer was Mr. Ransome's process. It referred mainly to the preparation of artificial stone, and he (Mr. Tite) believed that in that particular it had succeeded; he believed the artificial stone was a very useful and economical material. There was also a claim for applying the solution to stone. It involved the question of silication. The meeting would see that his leaning for the cure of the evil they so much deplored, lay in that direction. If they used anything that decayed rapidly, they would only be palliating and concealing the evil. He could not understand why, in the present state of science, a thorough remedy could not be devised. The obvious means was to communicate to the face of the stone a surface of glass—a material almost indestructible. We had now found a means of liquefying glass, and all that was wanted was to find a means of applying it. The whole of the question was carefully stated in certain papers which had been printed at the instance of H.R.H. the Prince Consort. Kohlmann's essay on this subject had also been translated from the German at the request of the Prince Consort. The method recommended in that essay had been adopted very extensively in France, at Lisle, and many other places, and was deemed a perfect success. It was also used by Kaulbach for the coating of fresco pictures. With regard to its application, the inventor had stated that the concentrated solution was not perceptibly affected by the atmosphere, although it was decomposed and converted into a stiff jelly on passing carbonic acid through it. He (Mr. Tite) begged to correct that statement. When concentrated water-glass was brushed upon marble or thick paper, although at first forming a shining surface, it afterwards cracked. Water-glass, when applied to stone in England, must be especially liable to this accident, as the stone was very rarely dry. Our moist climate and constant rain saturated the stone, and left little room for the absorption of the silica. The silica must enter into combination with the lime. The great durability of the ancient mortars was owing, not to the silica, but to the silicate of lime which was formed. He had been asked whether he could not coat the silica of soda with bitumen so as to give time for the silicate of lime to unite with the lime underneath it. He believed that could not be done, because the silica of lime threw out the soda, which, by its efflorescence, would cause the surface to fall away; or, if that were not the case, the carbonic acid would not unite with the silicate of soda and the lime, and silication would not take place. He wished to show what he apprehended was the difficulty in Mr. Ransome's patent. The theory was that it was possible by introducing over the surface of the silicate a composition to produce an artificial stone. If that could be effected, if the carbonate of lime and silica could be made to unite on the surface of the stone, the secret would be solved, but that could not be because the lime was not soluble. Mr. Ransome therefore used the muriate of lime, or chloride of calcium, as it was called in the softer language of the present day. The substance was hard, and could certainly be applied to the outside of the Palace at Westminster; but the difficulty which occurred was, that when it got thoroughly dry it became powdery and liable to be washed away.

Mr. **Burnell** said the stone which had been used was an amorphous Dolomite, and that was the great error that had been made. It should have been a crystalline Dolomite, and would not then have been liable to the disintegration of the surface. But the real question which concerned them, as practical and professional men, was, having a material which had already absorbed causes of decay, how were they to be counteracted? Bituminous matter and paints were liable to rapid decomposition; and he believed the only material applicable was some precipitate of the silicate of lime; but if it were attempted to apply that upon a stone saturated with moisture, it would not penetrate, and must be thrown off by the first frost.

Mr. **Cowper**, M.P., Chief Commissioner of Works, said Szerelmey's process had been applied to a portion of the building, at the instance of the late Sir Charles Barry, but the result had not been satisfactory. Her Majesty's Government were naturally looking to scientific men for guidance, and would be willing to adopt the best means that could be suggested.

The discussion was adjourned till Monday next, February 4th.

CHEMICAL SOCIETY.

Thursday, January 17.

Alfred Smee, *Esq., F.R.S., V.P., in the Chair.*

Dr. **Apjohn** was elected a Fellow, and Mr. A. **Reynolds** an Associate, of the Society.

The following papers were read:—

"*Analysis of the Saline Water of Purton, near Swindon, North Wilts,*" by Dr. **Noad**, F.R.S. The spring, which has a great medicinal reputation, yields about 120 gallons of water daily. It is situated at the distance of about a mile and a-half from the Thames at Crickdale. The soil of the neighbourhood rests upon limestone. The fixed contents in an imperial gallon of the water amounted to 409·6 grains. These consisted principally of the carbonates, sulphates, and chlorides of sodium, calcium, and magnesium, with a smaller quantity of some potassium, salt, and traces of iodine, bromine, and phosphoric acid.

Dr. **Voelcker** also read a paper "*On the Composition of Purton Water.*" His specimen was obtained in October, 1859. He observed that the water varied considerably in the total and relative amounts of its constituents. His arrangement of results was different from Dr. Noad's. On account of the strongly-marked alkalinity of the water, he had expressed the fixed carbonic acid in the form, not of carbonate of lime, but of carbonate of potash.

The direct results obtained by the two analysts are as follow:—

	Grains per Imperial Gallon.		
	Noad.	Voelcker.	
Carbonate of lime ..	40·608	———	
Carbonate of magnesia ..	2·104	———	
Lime	25·760	34·536	
Magnesia	38·060	25·736	
Potash	5·544	20·707	
Soda	76·360	49·006	
Chloride of sodium ..	———	34·297	
Chlorine	22·480	———	
Sulphuric acid	191·200	165·074	
Carbonic acid	———	33·090	
Silica	2·240	1·280	
Phosphoric acid.. ..	0·248	0·280	} phosp. of iron and alumina.
Iodine	0·075	0·056	
Bromine	traces	0·080	
Apocrenic acid	0·896	———	
Crenic acid	traces	———	
Organic matter, &c. ..	———	8·750	
	405·575	372·892	

Professor **Bloxam** read a paper "*On the Electrolytic Test for Arsenic.*" In a former communication on this subject he had shown that when arsenical compounds mixed with diluted sulphuric acid were acted upon by the current produced from four or five cells, arseniuretted hydrogen gas was liberated from the negative pole; but that arsenic acid did not respond to this test, and that the presence of mercury interfered with it. The author now obviated these difficulties by adding a small quantity of sulphuretted hydrogen water to the liquid during its electrolysis. By this means a ring of tersulphide of arsenic, followed by one of metallic arsenic, is readily obtained, both from arsenic acid and from organic substances containing arsenic, which have been destroyed by chlorate of potash and hydrochloric acid. The sulphuretted hydrogen, moreover, precipitates the mercury, and thereby prevents its interference. It also precipitates any antimony that may be present, whereby the contamination of the arseniuretted hydrogen with antimoniuretted hydrogen is avoided. The author found that, when antimony and arsenic were present in an organic substance, clear evidence of both metals could be successively obtained. The product of the action of hydrochloric acid and chlorate of potash upon the substance was introduced into the decomposing cell, whereby antimoniuretted hydrogen was evolved free from arseniuretted hydrogen. Afterwards, on the addition of sulphuretted hydrogen, arseniuretted hydrogen free from antimoniuretted hydrogen was evolved. The author observed that, according to his experience, sulphuric acid, as a rule, could be obtained more pure than hydrochloric acid. Moreover, he had found various samples of caustic potash to be contaminated with arsenic. Sulphuretted hydrogen prepared from diluted pure sulphuric acid and sulphide of iron was found to contain traces of arseniuretted hydrogen.

Dr. Hofmann made a short verbal communication on "*The Silver Bases of the Phosphorus, Arsenic, and Antimony Series.*" Triethylphosphine, triethylarsine, and triethylstibine, as is well known, have no action on test-papers. Treated with freshly precipitated oxide of silver, these substances are converted into powerfully alkaline liquids, containing respectively,—

Hydrate of argento-triethylphosphonium,

$$\left.\begin{matrix}[Ag(C_2H_5)_3P]\\ H\end{matrix}\right\}O,$$

Hydrate of argento-triethylarsonium,

$$\left.\begin{matrix}[Ag(C_2H_5)_3As]\\ H\end{matrix}\right\}O, \text{ and}$$

Hydrate of argento-triethylstibonium,

$$\left.\begin{matrix}[Ag(C_2H_5)_3Sb]\\ H\end{matrix}\right\}O.$$

The solutions of these three bases exhibit with metallic salts the deportment of potassa. Treated with alkaline chlorides these bases yield white precipitates of difficultly soluble double compounds, which, by ebullition with water, are gradually converted into the chlorides of the three compound metals. The salt of the phosphorus series is crystalline, and contains

$$[A_9(C_2H_5)_3P]Cl.$$

The hydrate of argento-triethylphosphonium is decomposed by hydrochloric acid, with formation of chloride of silver and chloride of triethylphosphonium. The existence of the above compounds appears to throw some light upon the composition of the explosive substances formed by the action of ammonia on oxide of silver, the nature of which is still doubtful.

Dr. Odling suggested that the formation of compounds of this description might possibly serve to settle the question of the bi-equivalency or bi-atomicity of the metals—lead, mercury, zinc, &c. If lead, for instance, were diatomic, it should form a diamine base. How could Dr. Hofmann distinguish by its chemical properties the chloride or oxide of a diamine base from the chloride or oxide of a monamine base?

Dr. Hofmann replied, that the platino-chlorides of the diamine bases crystallised in needles, and those of the monamine bases in octahedra. If the double chloride of platinum and triethyl-zinc-ammonium crystallised in octahedra, he should consider the compound to be a monammonium, and to contain monatomic zinc $= 31$, thus $Zn\,Et_3N\,Cl$; but, if it crystallised in needles, he should be inclined to regard it as a diammonium, containing diatomic zinc $= 63$, thus $Zn''\,Et_6N_2Cl_2$.

NOTICES OF BOOKS.

The Successful Application of Charcoal Air-Filters to the Ventilation and Disinfection of Sewers. A Letter to the Right Honourable the Lord Mayor, William Cubitt, M.P. By John Stenhouse, LL.D., F.R.S. Churchill, London.

In this pamphlet Dr. Stenhouse once more draws attention to the extraordinary power of charcoal in disinfecting and deodorising the gaseous products of putrefactive fermentation. The first practical application of this important property was made in 1854, when, at the author's suggestion, a charcoal air-filter was fitted up in the Justice-room of the Mansion House. The window of this room opens just above a large urinal, the smell from which was the subject of constant complaint in the room. The filter at once destroyed the nuisance, and "although six years have elapsed, the charcoal has never required to be renewed."

The deleterious nature of the gases evolved from decomposing sewage are too well proved by the numerous outbreaks of disease which have been distinctly traced to their influence. Yet, strange to say, the simple remedy, or rather preventive measure, which Dr. Stenhouse suggested six years ago, has yet received but a very limited application. It seems that 103 filters have been put down in one district in the City, and that so far the results are perfectly satisfactory, no bad smell being observed where they are placed, and no obstruction being caused to the ventilation of the sewers. Mr. Rawlinson has also applied them with a like success in two or three country towns, and expresses an opinion that the entire metropolis may be freed from sewer-gas smell by a very simple plan and at very small cost. Under these circumstances, we must thank Dr. Stenhouse for again thrusting the subject on public attention; and it will be for the Board of Works, which so readily spends hundreds of thousands of pounds on new streets and doubtful improvements, to consider whether a few thousands might not be very profitably invested in the abatement of a dangerous nuisance, and the probable salvation of many valuable lives.

NOTICES OF PATENTS.

Improvements in the Manufacture of Certain Colouring Matters by the Oxidation of Salts of Aniline and other Salts of a Similar Nature. By E. J. Hughes, of Manchester. Communication from MM. Depoully and Lauth, of Clichy, France.

In the Provisional Specification of this patent, hypochlorous acid and the hypo-chlorites are claimed for producing violet colours by acting with them upon salts of aniline, and nitric acid is claimed for producing red. The final specification, however, is confined to the use of nitric acid, the Patentee having, probably, become aware of the previous patents alluded to in our seventh number.

To produce a colour from aniline by the agency of nitric acid, the patentee mixes in a still two hundred parts of aniline with sixty-five parts of nitric acid specific gravity, 1·360. The mixture is heated by means of a sand-bath, or some convenient substitute, to 300° or 400° F. The distillate consists partly of aniline and partly of water, previously contained in the nitric acid. When no more water comes over, the residue is boiled for another hour; this converts the mixture into a dark red, syrupy fluid. This latter is then mixed with a little water, neutralised by carbonate of soda, and washed with water until the latter comes away colourless or slightly reddened. The product when dried consists of the red colouring matter and a brownish impurity. The two are to be separated by taking advantage of the comparative insolubility of the impurity. On boiling the impure colour, therefore, with water, the resinous matters float and may be removed by skimming and subsequent filtration through sand. The colour precipitates on cooling, and may be dissolved in alcohol or acetic acid for use.

Improvements in the Production of Colours for Dyeing or Printing. By E. Chambers Nicholson. Provisional Protection only.

Mr. Nicholson takes nitric acid and adds to it aniline or its homologues, and heats the mixture in a still until the colour is sufficiently developed. The coloured product is to be extracted by boiling the residue in the still with water, or extracting it with alcohol. "From these solutions the colouring matter may be prepared for use by any convenient and well-known method." In the same manner that Mr. Nicholson's provisional specification, dated January 25, 1860, for the use of arsenic acid, followed Mr. Medlock's of the week previous; so his provisional for nitric acid follows Mr. Hughes's, dated four days previously. Probably neither process will be very largely employed. One of our correspondents, who carefully tried the action of nitric acid upon aniline, with a view of obtaining colouring matter, reports very unfavourably, both as to the colour produced, and the economy of working. This remark only applies to the acid used without the intervention of an oxide.

A New Process for Manufacturing Red and Violet Colouring Matter. By Messrs. Georges de Laire and Charles Girard, of the Imperial Mint, Paris.

As the patentees merely employ arsenic acid which was patented in this country by Mr. H. Medlock, in January 18, 1860, and as their patent is dated May 26, 1860, we do not think it necessary to describe it any further.

Improvements in Obtaining Colouring Matters for Dyeing and Printing. By John Dale and Heinrich Caro.

First Process.—Purples.—An aqueous solution of a salt of aniline is mixed with a solution of chloride of copper, or mixtures of soluble salts of copper, with alkaline chlorides. On boiling these solutions, a black or dark purple precipitate is formed, containing the colouring matters. The following is a more precise direction:—One equivalent of a natural [doubtless a printer's error for neutral] salt of aniline is mixed with six equivalents of perchloride of copper, dissolved in as much water as is equal to thirty times the weight of the aniline used. The mixture having been boiled to determine the formation of the dark precipitate previously alluded to, is to be filtered, and washed with a weak solution of an alkali or alkaline carbonate until all soluble chlorides are removed. The purple colour may be extracted by means of boiling water, and from the aqueous solution the colouring matter may be precipitated by an alkali or alkaline carbonate, the solution of such precipitate in methylated spirit may be used for dyeing or printing.

A portion of the dark precipitate refuses to dissolve in boiling water; it is, therefore, to be dried at 212° F., and exhausted with methylated spirit, specific gravity 0·95. The spirit having been removed by distillation, the colouring matter is to be thrown down in the same manner as before. The precipitate is to be dried and extracted as previously described; the solution in methylated spirit may be used for dyeing or printing in the same manner as the other aniline purples.

Second Process.—Reds.—The patentees employ mixtures containing aniline, an anhydrous nitrate, and an anhydrous acid. The following is a condensed account of their method of working:—Two parts of aniline and two parts of nitrate of lead, dry and in fine powder, are to be raised to the boiling point of aniline [360° F.]. One part of anhydrous phosphoric acid is then added in small portions, with constant agitation, the temperature not being allowed to rise above 380° F. In an hour and a-half or two hours the operation will be complete. The colour is to be extracted from the mass by hot water, and may be precipitated by common salt. The solution in methylated spirit is used for dyeing and printing.

The patentees also give the following process which, if equally productive, and if the colour be equally good, is preferable on account of its dispensing with anhydrous phosphoric acid, a re-agent which is excessively unmanageable on a large scale. One part of aniline is saturated with dry, muriatic acid gas. The resulting compound is to be heated with constant agitation to 380° F. for the same time as before, two parts of dry and powdered nitrate of lead being added in small portions. The extraction, &c. of the colour from the mass is to be conducted as before.

This patent appears to us to be exceedingly interesting and instructive. It opens up a new class of aniline reactions. It is true that the final result is probably the same with all processes for violets; and also that the reds producible by such a vast number of decompositions are all essentially the same; nevertheless, some of the modes of operating which have been patented yield such inferior products that it is evident they were never intended to be followed. We do not think this reproach applies to he patent last described.

CORRESPONDENCE.

An Apparatus for Finding the Density of Vapours and Gases.

To the Editor of the Chemical News.

Sir,—The density of steam and other vapours and gases could be found by constructing a framework of sails, which could be worked in any convenient manner, and placing it, after being set in motion, into the chamber or vessel containing the gas or vapour, when the rapidity of its rotation would indicate the comparative density of the resisting medium; inasmuch as, whatever the density of this, the pressure would be exerted upon the contrivance equally from every direction, the results with gases and vapours of various densities would be proportionable, and therefore correct. It is of course supposed that the number of the rotations would be registered, and from this their velocity could clearly be found.

Mr. Babbage has ("Economy of Machinery and Manufactures") proposed a framework of sails for measuring the altitudes of mountains in this manner.—I am, &c.

J. Alexander Davies.

January 14, 1861.

Chemical Notices from Foreign Sources.

II. ORGANIC CHEMISTRY.

Constitution of Lactic Acid.—Kolbe and Wurtz discuss the constitution and basicity of lactic acid,—the former in the *Annalen der Chemie und Pharmacie*, bd. cxiii. s. 223; the latter in the *Annales de Chimie et de Physique*, t. lix. p. 161. Kolbe believes that lactic acid is monobasic. His reasons are the following:—1. Propionic, chloropropionic, and lactic are derived from one another by substitution. The two former are monobasic; therefore, lactic acid must be so. 2. No neutral lactate is known containing two equivalents of oxide. 3. We know no neutral lactic ether containing two atoms of basic ethyl. 4. No diamide of lactic acid is known. Wurtz admits that in a majority of cases lactic acid is only monobasic, but, at the same time, contends that it must be diatomic. He distinguishes between the word *diatomic*, which is applied to the molecular constitution of a body, and *bibasic*, which expresses the capacity of saturation. In answer to Kolbe, Wurtz shows that lactic acid is also derived from propyl-glycol, a diatomic compound, by simple substitution. Lactic acid is also allied to the bibasic acids by the circumstance that it may be changed into the anhydrous acid by the simple elimination of one molecule of water, without doubling its own molecule.

$$\underbrace{\text{Ȼ}_3\text{H}_6\text{Ɵ}_3}_{\text{Lactic acid.}} = \underbrace{\text{Ȼ}_3\text{H}_4\text{Ɵ}_2}_{\text{Lactide.}} + \text{H}_2\text{Ɵ}$$

It is impossible, says Wurtz, to quote a single monatomic acid which possesses this character.

Solubility of Glucose in Alcohol.—Anthon has determined (*Chemisches Centralblatt*, bd. v. s. 292) that glucose is soluble in alcohols of different strengths, at 64° F., to the following extent:—

	Specific gravity.			
100 parts of alcohol	0·837	will dissolve	1·95	parts of glucose
„ „	0·880	„	9·30	„ „
„ „	0.910	„	17·74	„ „
„ „	0·950	„	36·45	„ „

After some time, however, a little glucose will crystallise from these solutions, and the following numbers, the author says, will show the real solubility:—

	Specific gravity.			Per cent.	
Alcohol	0·837	will hold	in solution	1·94	of glucose
„	0·880	„	„	8·10	„
„	0·910	„	„	16·00	„
„	0·950	„	„	32·50	„

LABORATORY MEMORANDA.

Preparation of Phosphuretted Hydrogen Gas.—This body may be obtained with as much facility as hydrogen itself, by the following modification of an old process:—Fit a tube-funnel and a delivery-tube to a Florence, or other flask; introduce some pieces of phosphorus and about a couple of ounces of water; boil briskly until a spontaneously inflammable jet of phosphorised steam issues from the delivery-tube; lastly, gradually add strong solution of caustic soda, or potash, by the tube-funnel. By this method, the gas is given off very regularly, and the upper part of the flask being full of steam, no unpleasant explosions occur there.—*J. Attfield.*

MISCELLANEOUS.

Chemical Society.—At the next Meeting, on February 7, a paper will be read by Professor Field, "On the Carbonates of Copper, Nickel, and Cobalt."

Royal Institution.—The following Lectures will be delivered at this Institution during the ensuing week:—Monday, February 4, at 2 o'clock, General Monthly Meeting. Tuesday, February 5, at 3 o'clock, Professor Owen "On Fishes." Thursday, February 7, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 8, at 8 o'clock, Professor T. H. Huxley "On the Nature of the Earliest Stages of the Development of Animals." Saturday, February 9, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

Sugar. Wine. Tanning.—We extract two or three statements from the *Moniteur Universel* which our readers may take for what they are worth. The first is, that a sugar-maker in Louisiana has discovered that the cane-juice when exposed to a hot sun will granulate or crystallise without giving any molasses. Another is, that wine and brandy acquire the properties of old age after being submitted for a time to diminished atmospheric pressure. The third is, that if Divi-divi be used in place of bark in tanning a skin, it is as effectually tanned in fifteen days as in several months by the old process.

ANSWERS TO CORRESPONDENTS.

THE CHEMICAL SOCIETY.

The Council of the above Society having expressed to us their objection to seeing papers which have been read at the Meetings of the Society published in our columns, we beg to inform gentlemen who desire to have their discoveries published in the Chemical News, that their papers must in future be forwarded to us before they are sent to the Chemical Society.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

⁂ All *Editorial Communications* are to be addressed to Mr. Crookes and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the Chemical News, containing a copious Index, is now ready, price 12*s.*, by post, 12*s.* 8*d.*, handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1*s.* 6*d.* Subscribers may have their copies bound for 2*s.* if sent to our Office, or, if accompanied by a cloth case, for 6*d.* A few copies of Vol. I. can still be had, price 10*s.* 6*d.*, by post 11*s.* 2*d.* Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Collodion Balloons may be made by pouring some collodion of the proper consistency into a perfectly dry glass globe or flask of the desired shape. When the ether has evaporated the film will separate from the glass. Surgical, not photographic, collodion should be used.

Cracking of Kitchen Boilers.—*W.* asks if there is any other cause for a kitchen boiler cracking besides want of water, or sudden influx of cold water into the hot boiler. When the inside of the boiler gets thickly coated with fur it is very liable to crack. We have known several boilers cracked by the servant sending the point of the kitchen poker through them.

W. H.—No particular time was mentioned, twenty minutes would be about right. The bottom layer should be the mixture of cryolite, &c. Any large Operative Chemist should now supply you with sodium at a cheap rate. We do not know the present price; but it is only a shilling or two per ounce.

C. E.—Cyanide of calcium is not at present an article of commerce: It would have to be made to order by a Manufacturing Chemist, and of course a considerable price would have to be paid. We are much obliged for the information.

J. P. P.—The quantity of iron in 1 grain of the compound $Fe_2O_3PO_5$ may be calculated by the following formula:

as 151 (eq. of $Fe_2O_3PO_5$) : 1 grain :: 56 (2 eqs. of Fe) : the amount required.

Paste or Liquid Blacking.—A correspondent (C. Eades) has favoured us with the following receipt in answer to an inquiry a few weeks ago:—Fuller's earth, 8oz.; treacle, 3lbs.; ivory black, 2lbs.; butter scrapings, 4oz.; rape oil, 4oz.; strong gum-water, half-a-pint; powdered Prussian blue, ½oz.; oil of vitriol, 8oz.; if wanted liquid, use half-a-gallon of vinegar.

J. H. M., M.A.—Apply to the Secretary at Burlington House, who will forward every particular: you would be quite eligible, in our opinion.

A Subscriber, F.C.S., F.G.S.—We do not think such an advertisement ever appeared in our columns. Chlorate of potash can be obtained at any Chemist's.

A Subscriber wishes to know the most recent work or publication giving information on the subject of distilling wood, making naphtha, acetate of lime.

R. C. C. writes as follows:—"I have lately heard there is a plant growing in the Caucasus, which yields nitrate of potash in sufficient quantity to make gunpowder. Can you tell me anything about it? Can you also let me know the best method of preparing arsenic acid in quantities?"

THE ORIGINAL "DR. STEERS' OPODELDOC."
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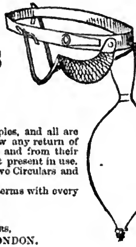

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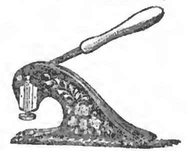

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THE CHEMICAL NEWS

Vol. III. No. 62.—February 9, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Molecular Constitution of Glycollic and Lactic Acids, by W. H. Perkin, *F.C.S.*

The artificial formation of glycollic acid from the chlorine and bromine derivatives of acetic acid, and also the transformation of lactic acid into chloropropionic acid, have of late brought these interesting substances under the consideration of chemists, and many are the views regarding their constitution which have been brought forward during the last two or three years. It is, therefore, with some diffidence that I venture toadd anything to what has been already written on this subject; but as what I have subjoined is supported by experimental evidence, I have had the boldness to produce it.

Before entering into any considerations regarding either glycollic or lactic acids, I wish to call attention to those substances from which they are derived,—namely, acetic and propionic acids.

Gerhardt considers acetic acid, propionic acid, and all other acids of the formula $C_{2n}H_{2n}O_4$, to consist of formic acid, in which one equivalent of hydrogen is replaced by an alcohol radical, such as methyle or ethyle, thus:—

Formic acid may be written . .	$C_2\ H\ HO_4$
Acetic acid (or, methyle formic) .	$C_2(C_2H_3)HO_4$
Propionic acid (or, ethyle formic) .	$C_2(C_4H_5)HO_4$

Which similitude of constitution is justified, as he remarks, by the decomposition of these acids when subjected to the influence of the electric battery, yielding, as is well known, carbonic acid and anhydro-carbons which are the alcohol radical of which they are in part built up, thus:—

Formic acid . .	gives Hydrogen
Acetic acid . .	,, Methyle
Propionic acid .	,, Ethyle.

Also, the decomposition of the cyanides of alcohol radicals under the influence of the hydrate of potassium give additional confirmation to this view.

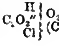
If, then, we view acetic and propionic acids as methyle and ethyle formic acids, what is the constitution of formic acid?

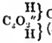
I think the following table throws much light upon this subject:—

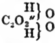
C_2O_2'' Carbonyle.

$\left.\begin{array}{r}Cl\\C_2O_2''\\Cl\end{array}\right\}$ Chloride of Carbonyle.

$\left.\begin{array}{r}H\\C_2O_2''\\H\end{array}\right\}$ Formic aldehyde.
(Hydride of Carbonyle.)

Chlorocarbonic acid

$\left.\begin{array}{r}H\\C_2O_2''\\Cl\end{array}\right\}$ O_2 Chloroformic acid.
(Chloride and Hydrate of Carbonyle.)


$\left.\begin{array}{r}H\\C_2O_2''\\H\end{array}\right\}$ O_2 Formic acid.
(Hydride and Hydrate of Carbonyle.)

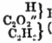

$\left.\begin{array}{r}H\\C_2O_2''\\H\end{array}\right\}$ O_2 Carbonic acid.
O_2 (Hydrate of Carbonyle.)

Here formic acid is represented as a compound of carbonyle. If we replace part or all of the hydrogen in these compounds by methyle and ethyle, we obtain acetic acid, propionic acid, and their derivatives, thus:—

$\left.\begin{array}{r}Cl\\C_2O_2''\\Cl\end{array}\right\}$ Chloride of Carbonyle.

$\left.\begin{array}{r}Cl\\C_2O_2''\\C_2H_3\end{array}\right\}$ Chloride of Acetyle.
(Chloride and Methylide of Carbonyle.)

$\left.\begin{array}{r}Cl\\C_2O_2''\\C_4H_5\end{array}\right\}$ Chloride of Propionyle.
(Chloride and Ethylide of Carbonyle.)

Aldehyde

$\left.\begin{array}{r}H\\C_2O_2''\\C_2H_3\end{array}\right\}$ Hydride of Acetyle.
(Hydride and Methylide of Carbonyle.)

Propionic aldehyde

$\left.\begin{array}{r}H\\C_2O_2''\\C_4H_5\end{array}\right\}$ Hydride of Propionyle.
(Hydride and Ethylide of Carbonyle.)

$\left.\begin{array}{r}C_2H_3\\C_2O_2''\\C_2H_3\end{array}\right\}$ Acetone.
(Methylide of Carbonyle)

$\left.\begin{array}{r}C_4H_5\\C_2O_2''\\C_4H_5\end{array}\right\}$ Propione
(Ethylide of Carbonyle.)

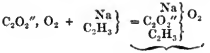
$\left.\begin{array}{r}H\\C_2O_2''\\C_2H_3\end{array}\right\}$ O_2 Acetic acid.
(Hydrate and Methylide of Carbonyle.)

$\left.\begin{array}{r}H\\C_2O_2''\\C_4H_5\end{array}\right\}$ O_2 Propionic acid.
(Hydrate and Ethylide of Carbonyle.)

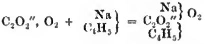
$\left.\begin{array}{r}H\\C_2O_2''\\C_2H_3\end{array}\right\}$ O_2 Methyle Carbonic acid.
O_2 (Hydrate of Oxide of Methyle and Carbonyle.)

$\left.\begin{array}{r}H\\C_2O_2''\\C_4H_5\end{array}\right\}$ O_2 Ethyle Carbonic acid.
O_2 (Hydrate of the Oxide of Methyle and Carbonyle.)

That the formulæ for acetic and propionic acids given in the above table are correct, I think the experiments of Wanklyn prove. As all are aware, he found that the remarkable substances sodium-methyle and likewise sodium ethyle, when brought in contact with carbonic anhydride (oxide of carbonyle), combined, yielding acetate of sodium in the one case and propionate of sodium in the other, thus:—



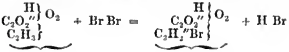
$$C_2O_2'',\ O_2 + \left.\begin{matrix}Na\\C_2H_3\end{matrix}\right\} = \underbrace{\begin{matrix}\left.\begin{matrix}Na\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_2H_3\end{matrix}\right\}\end{matrix}}_{\text{Acetate of Sodium.}}$$



$$C_2O_2'',\ O_2 + \left.\begin{matrix}Na\\C_4H_5\end{matrix}\right\} = \underbrace{\begin{matrix}\left.\begin{matrix}Na\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_4H_5\end{matrix}\right\}\end{matrix}}_{\text{Propionate of Sodium.}}$$

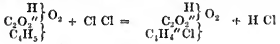
Acetic acid thus written may be viewed as the hydrate of the methylide of carbonyle, or, perhaps more clearly, as a compound of monohydrate of carbonyle $(\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2)'$ with methyle, and in the case of propionic acid, with ethyle.

When bromine acts upon acetic acid it is evident that it must attack the hydrogen in the methyle thus:—



$$\underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_2H_3\end{matrix}\right\}\end{matrix}}_{\text{Acetic acid.}} + Br\,Br = \underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_2H_2''Br\end{matrix}\right\}\end{matrix}}_{\text{Bromacetic acid.}} + H\,Br$$

Or if chlorine is made to act upon propionic acid, it must replace the hydrogen in the ethyle,—



$$\underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_4H_5\end{matrix}\right\}\end{matrix}}_{\text{Propionic acid.}} + Cl\,Cl = \begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}\\C_4H_4''Cl\end{matrix}\right\}\end{matrix} + H\,Cl$$

Bromacetic acid represents a compound of monohydrate of carbonyle $(\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2)'$ with monobromide of methylene $(C_2H_2''Br)'$, while chloropropionic acid represents a similar compound, containing the monochloride of ethylene $(C_4H_4''Cl)'$ in place of the monobromide of methylene.

The manner in which the bromine, in bromacetic acid, or the chlorine, in chloropropionic acid behaves towards re-agents is remarkable. Bromacetic acid, for example, when treated with ammonia, yields glycolle, just in the same manner as bromide of methyle and ammonia yields methylamine.

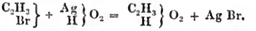
Chloropropionate of ethyle and butyrate of sodium re-act, forming an ethereal body, containing butyric acid, just as chloride of ethyle would act upon the same salt and produce butyric ether.

Bromacetic ether likewise reacts on acetate of potassium, benzoate of potassium, and succinnate of potassium, producing bodies having properties somewhat similar to acetate, benzoate, or succinnate of methyle.

Bromacetate of ethyle also acts upon picoline and other nitryle bases producing the bromides of ammoniums exactly as bromide of ethyle does.

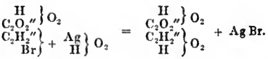
From these facts, it is evident that chloropropionic acid has the double property of an acid and of a chloride of an electro-negative radical, and that bromacetic acid has the twofold property of an acid and of a bromide of an electro-negative radical, as the formula I have proposed for them shows.

When bromide of methyle is decomposed by hydrate of silver, hydrate of methyle (methylic alcohol) and bromide of silver are formed,—



$$\left.\begin{matrix}C_2H_3\\Br\end{matrix}\right\} + \left.\begin{matrix}Ag\\H\end{matrix}\right\}O_2 = \left.\begin{matrix}C_2H_3\\H\end{matrix}\right\}O_2 + Ag\,Br.$$

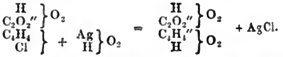
A similar reaction takes place when bromacetic acid is treated in the same manner, the monobromide of methylene of the acid being converted into a hydrate of methylene, and the hydrate of silver into the bromide of silver, thus:—



$$\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_2H_2''\\Br\end{matrix}\right\}\end{matrix} + \left.\begin{matrix}Ag\\H\end{matrix}\right\}O_2 = \begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_2H_2''\\H\end{matrix}\right\}O_2\end{matrix} + Ag\,Br.$$

Consequently, glycollic acid may be said to consist of the monohydrate of carbonyle and monohydrate of methylene, the former acting as an acid, and the latter as an alcohol.

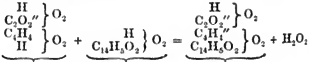
The formation of lactic acid from chloropropionic acid, under the influence of hydrate of silver, is precisely similar to the formation of glycollic acid, and may be represented thus:—



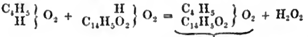
$$\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_4H_4\\Cl\end{matrix}\right\}\end{matrix} + \left.\begin{matrix}Ag\\H\end{matrix}\right\}O_2 = \begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_4H_4''\\H\end{matrix}\right\}O_2\end{matrix} + Ag\,Cl.$$

Therefore, lactic acid may be represented as consisting of the monohydrate of carbonyle and monohydrate of ethylene.

The formation of benzolactic acid, by the action of heat on a mixture of lactic and benzoic acids, goes to show the existence of an alcohol in lactic acid, and is also analogous to the formation of benzoic ether from alcohol and benzoic acid:—

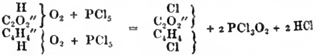


$$\underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_4H_4\\H\end{matrix}\right\}O_2\end{matrix}}_{\text{Lactic acid.}} + \underbrace{\left.\begin{matrix}H\\C_{14}H_5O_2\end{matrix}\right\}O_2}_{\text{Benzoic acid.}} = \underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_4H_4''\\C_{14}H_5O_2\end{matrix}\right\}O_2\end{matrix}}_{\text{Benzolactic acid.}} + H_2O_2$$

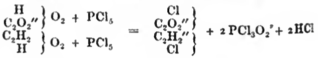


$$\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}O_2 + \left.\begin{matrix}H\\C_{14}H_5O_2\end{matrix}\right\}O_2 = \underbrace{\left.\begin{matrix}C_4H_5\\C_{14}H_5O_2\end{matrix}\right\}O_2}_{\text{Benzoate of Ethyle.}} + H_2O_2$$

The fact of these substances having the twofold character of an acid and alcohol shows why they are monobasic, although representing two molecules of water. It also explains the reason why lactic acid when distilled with pentachloride of phosphorus yields chloride of chloropropionyle, and also why glycollic acid, under similar circumstances, yields the chloride of chloracetyle, thus:—

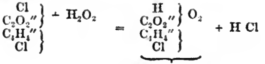


$$\underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2 + PCl_5\\ \left.\begin{matrix}C_4H_4''\\H\end{matrix}\right\}O_2 + PCl_5\end{matrix}}_{\text{Lactic acid.}} = \underbrace{\left.\begin{matrix}Cl\\C_2O_2''\\C_4H_4\\Cl\end{matrix}\right\}}_{\text{Chloride of Chloropropionyle.}} + 2\,PCl_3O_2 + 2\,HCl$$

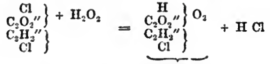


$$\underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2 + PCl_5\\ \left.\begin{matrix}C_2H_2\\H\end{matrix}\right\}O_2 + PCl_5\end{matrix}}_{\text{Glycollic acid.}} = \underbrace{\left.\begin{matrix}Cl\\C_2O_2''\\C_2H_2''\\Cl\end{matrix}\right\}}_{\text{Chloride of Chloracetyle.}} + 2\,PCl_3O_2 + 2\,HCl$$

As chloride of chloropropionyle and chloride of chloracetyle consist of the chloride of an electro-positive radical, and the chloride of an electro-negative radical, they are only half decomposed by water, as the chlorides of the electro-negative radicals are not easily decomposed by that agent, thus:—

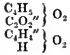


$$\left.\begin{matrix}Cl\\C_2O_2''\\C_4H_4''\\Cl\end{matrix}\right\} + H_2O_2 = \underbrace{\begin{matrix}\left.\begin{matrix}H\\C_2O_2''\end{matrix}\right\}O_2\\ \left.\begin{matrix}C_4H_4''\\Cl\end{matrix}\right\}\end{matrix}}_{\text{Chloropropionic acid.}} + H\,Cl$$



$$\left.\begin{matrix}\mathrm{Cl}\\ C_2O_2''\end{matrix}\right\}\atop\left.\begin{matrix}C_2H_2''\\ \mathrm{Cl}\end{matrix}\right\} + H_2O_2 = \underbrace{\left.\begin{matrix}H\\ C_2O_2''\end{matrix}\right\}O_2\atop\left.\begin{matrix}C_2H_2''\\ \mathrm{Cl}\end{matrix}\right\}}_{\text{Chloracetic acid.}} + H\,\mathrm{Cl}$$

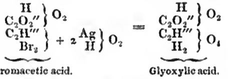
Being still desirous of further verifying the idea of glycollic and lactic acids being both acids and alcohols, I made further experiments. I wished first to get rid of the acid properties of one of these acids, so that I might study its alcoholic nature alone. For this purpose I selected lactic ether, which may be represented thus:—



$$\left.\begin{matrix}C_4H_5\\ C_2O_2''\end{matrix}\right\}O_2\atop\left.\begin{matrix}C_4H_4''\\ H\end{matrix}\right\}O_2$$

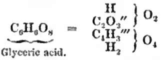
This ether dissolves sodium with evolution of hydrogen just as alcohol does. If brought in contact with the chloride of acetyle or chloride of succinyle it evolves large quantities of hydrochloric acid, with formation of oily etherial bodies, just as ordinary alcohol does. Therefore, I think there can be no doubt that they possess the properties of alcohols.

The decomposition of acids containing a still greater amount of bromine or chlorine than bromacetic or chloropionic acids yield also analogous results,—thus, bibromacetic acid and hydrate of silver yields an acid which represents a diatomic alcohol and a monobasic acid:—



$$\underbrace{\left.\begin{matrix}H\\ C_2O_2''\end{matrix}\right\}O_2\atop\left.\begin{matrix}C_2H'''\\ Br_2\end{matrix}\right\}}_{\text{Bibromacetic acid.}} + 2\left.\begin{matrix}Ag\\ H\end{matrix}\right\}O_2 = \underbrace{\left.\begin{matrix}H\\ C_2O_2''\end{matrix}\right\}O_2\atop\left.\begin{matrix}C_2H'''\\ H_2\end{matrix}\right\}O_4}_{\text{Glyoxylic acid.}}$$

This acid is analogous to glyceric acid, which may be expressed in a similar manner:—



$$\underbrace{C_6H_6O_8}_{\text{Glyceric acid.}} = \left.\begin{matrix}H\\ C_2O_2''\end{matrix}\right\}O_2\atop\left.\begin{matrix}C_4H_3'''\\ H_2\end{matrix}\right\}O_4$$

On the Analysis of Milk, by JOHN HORSLEY.

1000 grains, specific gravity 10·30, on evaporation to dryness yielded 100 grains, or $\frac{1}{10}$th, of solid matter, which, by a set of careful experiments with further quantities of the same milk, was found to consist of

Butter	20 parts
Casein	35 "
Lactin and salts . .	45 "
	100 "

M. Poggiale's experiments with milk make its composition as follows, but unfortunately the time of year is not stated, which is important, since milk is richer in the summer than winter; and from the very great difference in the amount of solids, yield of butter, &c., it is most probable his experiments were conducted in the summer time:—

Butter	43·8 parts
Casein	38 "
Lactin and salts . .	55·4 "
	137·2 "

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. BOWDITCH.

(*Continued from page 69.*)

Although collateral matter has been carefully excluded from this paper, I cannot refrain from remarking that the property of clay here mentioned is in fact the announcement of a new property of soils, and one which will help to account for the formation of many natural metallic sulphides. I hope soon to have some investigations on this subject ready for publication.

To ascertain whether this property of breaking up a sulphurised compound in purified gas and removing tar was possessed by clay alone, or shared by other substances used in purification, some purified gas was passed through a considerable chemical excess of all the substances employed in purification, viz. lime, precipitated peroxide of iron, sulphate of iron, chloride of calcium, and dilute sulphuric acid, all but the lime being mixed with moist sawdust. Upon passing the gas next through a purifier filled with clay, it darkened lead-paper, and affected turmeric when it had passed a subsequent purifier filled with lime. This proves the power of clay to break up one or more sulphurised compounds which no other substance used in purification effects; and if this sulphur were not liberated from the impure naphthalin compound already mentioned, it seemed certain that gas which had been previously purified by clay might be much improved, if not rendered pure, by a removal of the sulphur of the naphthalin. There is strong experimental evidence that the compound from which clay liberates sulphide of hydrogen is not the sulphurised naphthalin one; for if hydrogen be passed through a vessel containing this substance, then through clay, and subsequently over lead-paper, no trace of sulphide of hydrogen is found, though the gas passing smells strongly of impure naphthalin. This gas and vapour burn with a lightless flame. Subsequently some naphthalin was heated to ebullition, and a current of hydrogen sent through it and then burnt. The flame was lightless as before. I mention this fact to remove the popular error that naphthalin, as it exists in coal-gas, is a good illuminant. Even Dr. Frankland thus regards it; and both in Clegg's book on coal-gas, and in the new edition of Ure's Dictionary, states that the hydrocarbons in gas are valuable in proportion to the carbon they contain, and that naphthalin is the most valuable as containing the largest proportion of carbon. The above-mentioned experiments evince the contrary. On another occasion I determined the illuminating power of some gas, and then, without alteration of the quantity passing, sent the gas through a U-tube containing naphthalin from the London gas-mains. The character of the flame was changed from white to red, but the photometer indicated no difference in the light given. Two other persons conversant with photometry were present at this experiment and agreed in the result, though up to that time they had held the prevailing opinion as to the value of naphthalin in gas without testing the statements made upon the subject.

Another sulphur compound is said always to be present in coal-gas and to be irremoveable, and which, like those I have hitherto spoken of, does not affect lead-paper, viz. bisulphide of carbon. To ascertain the presence of bisulphide of carbon, I pass gas through strong spirit of wine (methylated spirit answers perfectly) kept at about 160° F. The gas and vapours pass out of the flask which contains the spirit up a long tube into an inverted flask, so that all which is condensed may run back into the spirit. It then passes into another flask for additional condensation, and thence forward to a gas-holder or burner. Bisulphide of carbon dissolved in spirit becomes precipitated as a white cloud which settles to the bottom of the vessel, when the spirit is copiously diluted with water. The white cloudy precipitate escapes slowly by single bubbles through the diluted spirit, and at length leaves a solution perfectly clear. The spirit through which gas has passed, and from which it has abstracted bisulphide of carbon, acts in precisely the same manner upon dilution, and no one who has seen the reaction once or twice can possibly mistake it. The study of other compounds led me to conclude that if this substance exist *as such*, and not merely *by its elements*, in gas, it could be removed almost as easily as the naphthalin compound could be purified, and that the same process could be made available to remove the sulphur of both. I thought that under certain conditions the affinity of hydrogen for sulphur would exceed that of carbon for sulphur, and therefore that I might obtain the sulphur of bisulphide of carbon as a sulphide of hydrogen, about the removal of which there is no difficulty. Experiment confirms the reasoning. When hydrogen mixed with vapour of bisulphide of carbon is passed through a tube filled with slaked lime or clay which has been dried at 400° or 500° F., and is kept between 400° and 600° F. during the passage of the gas and vapour, not a trace of bisulphide of carbon passes from the tube, but sulphide of hydrogen does pass. The lime is darkened by a deposit of carbon, and a sublimate of sulphur is found in the exit-tube. A considerable excess of hydrogen should be used, or else a portion of the bisulphide of carbon vapour is carried over by the current and escapes decomposition. That this reaction is not the result of heat merely, but is a truly chemical one which the base under the influence of heat effects, and the remarkable fact that slaked lime when heated forms, but does not unite with, sulphide of hydrogen, receive illustration from the following experiments.

Hydrogen and vapour of sulphide of carbon were passed through—1st, cold slaked lime; 2nd, cold clay; 3rd, hot oxide of iron used at a gas-works in purification; 4th, hot broken bricks; 5th, hot broken glass, without in any instance producing sulphide of hydrogen. On the contrary, when passed through (1) hot lime and (2) hot clay, sulphide of hydrogen was formed and passed over immediately, and continued to pass as long as the current was kept up. The lime, when cooled out of contact with the air, gave no sulphide of hydrogen upon being supersaturated with dilute sulphuric acid, but clay when thus treated gave off much.

The hydrogen used was in all cases passed through lime and over lead paper, to secure its being free from sulphide before use. On one occasion, when the clay had been imperfectly dried before heating, I observed much sulphurous acid instead of sulphide of hydrogen. I therefore passed hydrogen, bisulphide of carbon vapour, and steam over hot clay which had been properly dried. At first sulphide of hydrogen passed over alone, then mixed with sulphurous acid, which at length passed alone. Subsequently sulphide of hydrogen passed, and at length sulphurous acid ceased. As the one gas increased the other diminished, and throughout the experiment sulphide of carbon vapour passed undecomposed. It is shown by this experiment that hot clay in the presence of more water than forms a hydrate acts very differently from the same clay when dry, and the whole subject deserves a full investigation.

Action of Sulphide of Hydrogen upon Clay and Lime, Cold and Hot.—Well washed sulphide of hydrogen passed into cold slaked lime (obtained from Buxton) in a tube, colours the lime green as soon as it comes into contact with it, and the progress of the gas along the tube corresponds with the coloration. Lead-paper is not affected until the lime becomes coloured close up to the exit.

The same gas, passed into a tube containing slaked lime, kept about 600° F. at the middle, but cool at both ends, acts differently. The cool lime at the inlet end becomes coloured; the hot lime in the middle remains white, and the cool lime at the exit end becomes coloured, and lead-paper is stained as soon as these two cool portions are saturated, while the middle portion remains unchanged in colour.

The same gas, passed into a tube containing hot lime only, causes no discoloration, but instantly blackens lead-paper placed at the exit end; and upon being conducted into a tube of cold lime, colours it as if it had just passed from the vessel in which it is produced.

The same gas was passed into a tube containing lime which had been thoroughly dried at 600° F., and cooled out of contact with the air. No discoloration of the lime took place, but the gas passed unaffected by the lime, and blackened lead-paper. Water added to the lime gives it the power of decomposing the gas as if it had not been heated. The presence of more water than is necessary to form hydrate of lime ($Ca\,O, HO$) is thus shown to be required for the decomposition of sulphide of hydrogen by slaked lime.

Sulphide of hydrogen passed into a tube of cold clay is taken up in considerable quantity, and the clay becomes black from the formation of sulphide of iron. The blackening begins at the inlet end, and progresses with the passage of the gas towards the exit end of the tube.

The same gas, passed into clay, heated to 500° or 600° F., gives the same reactions; but when the clay has been heated and well-dried, and cooled in the closed tube, it takes up a very small quantity of the gas.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Six Lectures[1] *(adapted to a Juvenile Auditory), on the Chemical History of a Candle; by* M. Faraday, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

Lecture VI. (Jan. 8, 1861.)

Carbon or Charcoal—Coal Gas—Respiration and its Analogy to the Burning of a Candle—Conclusion.

A lady who honours me by her presence at these Lectures has conferred a still further obligation by sending me these two candles which are from Japan, and, I presume, are made of that substance to which I referred in a former Lecture. You see that they are even far more highly ornamented than the French candles, and, I suppose, are candles of luxury, judging from their appearance. They have a remarkable peculiarity about them, namely, a hollow wick,—that beautiful peculiarity which Argand introduced into the lamp and made so valuable. To those who receive such presents from the East, I may just say that this and such like materials, gradually assume a change which gives them on the surface a dull and dead appearance; but they may easily be restored to their original beauty if the surface is rubbed with a clean cloth or silk handkerchief, so as to polish the little rugosity or roughness; this will restore the beauty of the colours. I have so rubbed one of these candles, and you see the difference between it and the other which has not been polished, but which may be restored by the same process. Observe, also, that these moulded candles from Japan are made more conical than the moulded candles in this part of the world.

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[1] Reported *verbatim* by special permission.

I told you, when we last met, a good deal about carbonic acid. We found by the lime-water test that when the vapour from the top of the candle or lamp was received into bottles and tested by this solution of lime water (the composition of which I explained to you, and which you can make for yourselves), we had that white opacity which was in fact calcareous matter, like shells and corals, and many of the rocks and minerals in the earth. But I have not yet told you clearly and chemically the history of this substance, carbonic acid, as we have it from the candle, and I must now take you to that point. We have seen the products, and the nature of them, as they issue from the candle. We have traced the water to its elements, and now we have to see where are the elements of the carbonic acid supplied by the candle: a few experiments will show this. You remember that when a candle burns badly it burns with smoke; but if it is burning well there is no smoke. And you knew that the brightness of the candle is due to this smoke which becomes ignited. Here is an experiment to prove this: so long as the smoke remains in the flame of the candle and becomes ignited it gives a beautiful light, and never appears to us in the forms of black particles. I will light some fuel which is extravagant in its burning; this will serve our purpose—a little turpentine on a sponge. You see the smoke rising from it, and floating into the air in large quantities, and remember now, the carbonic acid that we have from the candle is from such smoke as that. To make that evident to you I will introduce this turpentine burning on the sponge into a flask where I have plenty of oxygen, the rich part of the atmosphere, and you see that the smoke is all consumed. This is the first part of our experiment, and now what follows? The carbon which you saw flying off from the turpentine flame in the air we have now entirely burned in this oxygen, and we shall find that it will by this rough and temporary experiment, give us exactly the same conclusion and result as we had from the combustion of the candle. The reason why I make the experiment in this manner is solely that I may cause the steps of our demonstration to be so simple that you can never for a moment lose the train of reasoning if you only pay attention. All the carbon which is burned in oxygen, or air, comes out as carbonic acid, whilst those particles which are not so burned show you the second substance in the carbonic acid, namely the carbon, that body which made the flame so bright whilst there was plenty of air, but which was thrown off in excess when there was not oxygen enough to burn it.

I have also to show you a little more distinctly the history of carbon and oxygen in their union to make carbonic acid. You have now a right to know this to a far greater extent than before, so I have three or four experiments for that purpose. I have here a jar filled with oxygen, and here is some carbon which has been placed in a crucible for the purpose of being made red hot. I keep my jar dry and venture to give you a result imperfect in some degree, in order that I may make the experiment brighter. I am about to put the oxygen and the carbon together. That this is carbon (common charcoal pulverised) you will see by the way in which it burns in air [letting some of the red-hot charcoal fall out of the crucible]. I am now about to burn it in oxygen gas, and look at the difference. It may appear to you at a distance as if it were burning with a flame; but it is not so. Every little piece of charcoal is burning as a spark, and whilst it so burns it is producing carbonic acid. I specially want these two or three experiments to point out what I shall dwell upon more distinctly by-and-by—that carbon burns in this way, and not as a flame.

Instead of taking many particles of carbon to burn I will take a rather large piece, which will enable you to see the form and size, and to trace the effects very decidedly. Here is the jar of oxygen, and here is the piece of charcoal, to which I have fastened a little piece of wood, which I can set fire to and so carry in the combustion, which I could not conveniently do without. You now see the charcoal burning, but not as a flame (or if there be a flame it is the smallest possible one, which I know the cause of, namely, the formation of a little carbonic oxide close upon the surface of the carbon). It goes on burning, you see, slowly producing carbonic acid by the union of this carbon or charcoal (they are equivalent terms) with the oxygen. I have here another piece of charcoal, a piece of bark, which has the quality of being blown to pieces—exploding—as it burns. By the effect of the heat we shall reduce the lump of carbon into particles that will fly off; still every particle, equally with the whole mass, burns in this peculiar way—it burns as a coal and not like a flame. You observe a multitude of little combustions going on, but no flame. I do not know a finer experiment than this to show that carbon burns with a spark.

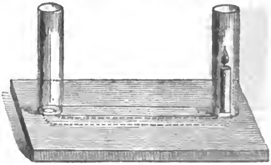
Here, then, is carbonic acid formed from its elements. It is produced at once, and if we examined it by lime-water, you will see that we have the same substance which I have previously described to you. By putting together 6 parts of carbon by weight (whether it comes from the flame of a candle or from powdered charcoal) and 16 parts of oxygen by weight, we have 22 parts of carbonic acid; and, as we saw last time, the 22 parts of carbonic acid combined with 28 parts of lime, produce common carbonate of lime. If you were to examine an oyster-shell and weigh the component parts, you would find that every 50 parts would give 6 of carbon and 16 of oxygen combined with 28 of lime. However, I do not want to trouble you with these minutiæ; it is only the general philosophy of the matter that we can now go into. See how finely the carbon is dissolving away [pointing to the lump of charcoal burning quietly in the jar of oxygen]. You may say that the charcoal is actually dissolving in the air round about, and if that were perfectly pure charcoal, which we can easily prepare, there would be no residue whatever. When we have a perfectly cleansed and purified piece of carbon there is no ash left. The carbon burns as a solid dense body, that heat alone cannot change as to its solidity, and yet it passes away into vapour that never condenses into solid or liquid under ordinary circumstances; and what is more curious still is the fact that the oxygen does not change in its bulk by the solution of the carbon in it. Just as the bulk is at first, so it is at last, only it has become carbonic acid.

There is another experiment which I must give you before you are fully acquainted with the general nature of carbonic acid. Being a compound body, consisting of carbon and oxygen, carbonic acid is a body that we ought to be able to take asunder. And so we can. As we did with water so we can with carbonic acid,—take the two parts asunder. The simplest and quickest way is to act upon the carbonic acid by a substance that can attract the oxygen from it, and leave the carbon behind. You recollect that I took potassium and put it upon water or ice, and you saw that it could take the oxygen from the hydrogen. Now, suppose we do something of the same kind here with this carbonic acid. You know carbonic acid to be a heavy gas: I will not test it with lime-water, as that will interfere with our subsequent experiments, but I think the heaviness of the gas and the power of extinguishing flame will be sufficient for our purpose. I introduce a flame into the gas, and you will see whether it will put it out. You see the light is extinguished. Indeed, the gas may, perhaps, put out phosphorus, which you know has a pretty strong combustion. Here is a piece of phosphorus heated to a high degree. I introduce it into the gas, and you observe the light is put out, but it will take fire again in the air, because there it re-enters into combustion. Now let me take a piece of potassium, a substance which even at common temperatures can act upon carbonic acid, though not sufficiently for our present purpose, because it soon gets covered with a protecting coat; but if we warm it up to the burning point in air, as we have a fair right to do, and as we have done with phosphorus, you will see that it can burn in carbonic acid; and if it burns it will burn by taking oxygen, so that you will see what is left behind. I am going, then, to burn this potassium in the carbonic acid as a proof of the existence of oxygen in the carbonic acid. [In the preliminary process of heating the potassium exploded.] Sometimes we get an awkward piece of potassium that explodes, or something like it, when it burns. I will take another piece, and now that it is heated I introduce it into the jar, and you perceive that it burns in the carbonic acid—not so well as in the air, because the carbonic acid contains the oxygen combined, but it does burn, and takes away the oxygen. If I now put this potassium into water I find that besides the potash formed (which you need not trouble about) there is a quantity of carbon produced. I have here made the experiment in a very rough way, but I assure you that if I were to make it carefully, devoting a day to it, instead of five minutes, we should get all the proper amount of charcoal left in the spoon, or in the place where the potassium was burned, so that there could be no doubt as to the result. Here, then, is the carbon obtained from the carbonic acid, as a common black substance; so that you have the entire proof of the nature of carbonic acid as consisting of carbon and oxygen. So now, I may tell you, that whenever carbon burns under common circumstances, it produces carbonic acid.

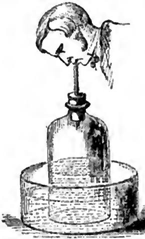
Suppose I take this piece of wood, and put it into a bottle with lime-water. I might shake that lime-water up with wood and the atmosphere as long as I pleased, it would still remain clear as you see it; but suppose I burn the piece of wood in the air of that bottle. You, of course, know I get water. Do I get carbonic acid? [The experiment was performed.] There it is you see, that is to say, the carbonate of lime, which results from carbonic acid, and that carbonic acid must be formed from the carbon which comes from the wood, from the candle, or any other thing. Indeed, you have yourselves frequently tried a very pretty experiment, by which you may see the carbon in wood. If you take a piece of wood, and partly burn it, and then blow it out, you have carbon left. There are things that do not show carbon in this way. A candle does not so show it, but it contains carbon. Here also is a jar of coal-gas, which produces carbonic acid abundantly,—you do not see the carbon, but we can soon show it to you. I will light it, and as long as there is any gas in this cylinder it will go on burning. You see no carbon, but you see a flame, and because that is bright it will lead you to guess that there is carbon in the flame. But I will show it to you by another process. I have some of the same gas in another vessel, mixed with a body that will burn the hydrogen of the gas, but will not burn the carbon. I will light them with a burning taper, and you perceive the hydrogen is consumed, but not the carbon, which is left behind as a dense black smoke. I hope that by these three or four experiments you will learn to see when carbon is present, and understand what are the products of combustion, when gas or other bodies are thoroughly burned in the air.

Before we leave the subject of carbon, let us make a few experiments and remarks upon its wonderful condition, as respects ordinary combustion. I have shown you that the carbon in burning burns only as a solid body, and yet you perceive that after it is burned, it ceases to be a solid. There are very few fuels that act like this. It is in fact only that great source of fuel, the carbonaceous series, the coals, charcoals, and woods, that can do it. I do not know that there is any other elementary substance besides carbon that burns with these conditions, and if it had not been so, what would happen to us? Suppose all fuel had been like iron which, when it burns, burns into a solid substance. We could not then have such a combustion as you have in this fire-place. Here also is another kind of fuel which burns very well—as well as, if not better, than carbon—so well, indeed, as to take fire of itself when it is in the air, as you see. [Breaking a tube full of lead pyrophorus.] This substance is lead, and you see how wonderfully combustible it is. It is very much divided, and is like a heap of coals in the fire-place: the air can get to its surface and inside, and so it burns. But why does it not burn in that way now when it is lying in a mass? [Emptying the contents of the tube in a heap on to a plate of iron.] Simply because the air cannot get to it. Though it can produce a great heat, the great heat which we want in our furnaces and under our boilers, still that which is produced cannot get away from the portion which remains unburned underneath, and that portion, therefore, is prevented from coming in contact with the atmosphere, and cannot be consumed. How different is that from carbon! Carbon burns just in the same way as this lead does, and so gives an intense fire in the furnace, or wherever you choose to burn it; but then the body produced by its combustion passes away, and the remaining carbon is left clear. I showed you how carbon went on dissolving in the oxygen, leaving no ash; whereas, here [pointing to the heap of pyrophorus] we have actually more ash than fuel, for it is heavier by the amount of the oxygen which has united with it. Thus, you see, the difference between carbon and lead or iron, if we chose iron, which gives so wonderful a result in our applications of this fuel, either as light or heat. If when the carbon burnt here the product went off as solid body you would have had the room filled with an opaque substance, as in the case of the phosphorus; but when carbon burns everything passes up into the atmosphere. It is in a fixed, almost unchangeable condition before the combustion; but afterwards it is in the form of gas, which it is very difficult (though we have succeeded) to produce in a solid or liquid state.

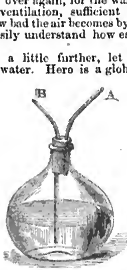
Now I must take you to a most interesting part of our subject—to the relation between the combustion of a candle and that living kind of combustion which goes on within us. In every one of us there is a living kind of combustion going on exactly like that of a candle, and I must try to make that plain to you. For that is not merely true in a poetical sense —the relation of the life of man to a taper, and if you will follow, I think I can make this clear. In order to make the relation very plain, I have devised a little apparatus which we can soon build up before you. Here is a board and a groove cut in it, and I can close the groove at the top part by a little cover; I can then continue the groove as a channel by a glass tube at each end, there being a free passage through the whole. Suppose I take a taper or candle (we can now be liberal in our use of the word "candle," since we understand what it means), and place it in one of the tubes; it will go on, you see, burning very well. You observe that the air which feeds the flame passes down the tube at one end, then goes along the horizontal tube, and ascends the tube at the other end in which the taper is placed. If I stop the aperture through which the air enters, I stop combustion, as you perceive. I stop the supply of air, and consequently the candle goes out. But now what will you think of this fact? In a former experiment I showed you the air going from one burning candle to a second candle. If I took the air proceeding from another candle, and sent it down by a complicated arrangement into this tube, I should put this burning candle out. But what will you say when I tell you that my breath will put out that candle? I do not mean by blowing at all, but simply that the nature of my breath is such that a candle cannot burn in it. I will now hold my mouth over the aperture, and without blowing the flame in any way, let no air enter the tube but what comes from my mouth. You see the result. I did not blow the candle out. I merely let the air which I expired pass into the aperture, and the result was that the light went out for want of oxygen, and for no other reason. Something or other—namely, my lungs—had taken away the oxygen from the air, and there was no more to supply the combustion of the candle. It is, I think, very pretty to see the time it takes before the bad air which I throw into this part of the apparatus has reached the candle. The candle at first goes on burning, but so soon as the air has had time to reach it it goes out. And now I will show you another experiment, because this is an important part of our philosophy. Here is a jar which contains fresh air, as you can see by the circumstance of a candle or gas-light burning in it. I make it close for a little time, and by means of a pipe I get my mouth over it so that I can inhale the air. By putting it over water, in the way that you see, I am able to draw up this air (supposing the cork to be quite tight), take it into my lungs, and throw it back into the jar: we can then examine it, and see the result. You observe, I first take up the air, and then throw it back, as is evident from the ascent and descent of the water, and now, by putting a taper into the air, you will see the state in which



it is by the light being extinguished. Even one inspiration, you see, has completely spoiled this air, so that it is no use my trying to breathe it a second time. Now you understand the ground of the impropriety of many of the arrangements among the houses of the poorer classes, by which the air is breathed over and over again, for the want of a supply, by means of proper ventilation, sufficient to produce a good result. You see how bad the air becomes by a single breathing, so that you can easily understand how essential fresh air is to us.

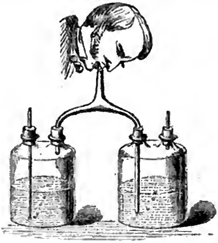


To pursue this a little further, let us see what will happen with lime-water. Here is a globe which contains a little lime-water, and it is so arranged as regards the pipes, as to give access to the air within, so that we can ascertain the effect of respired, or unrespired air upon it. Of course I can either draw in air (through A) and so make the air that feeds my lungs go through the lime-water, or I can force the air out of my lungs through the tube (B) which goes to the bottom, and so show its effect upon the lime-water. You will observe that however long I draw the external air into the lime-water, and then through it to my lungs, I shall produce no effect upon the water—it will not make the lime-water turbid; but if I throw the air *from* my lungs through the lime-water, several times in succession, you see how white and milky the water is getting, showing the effect which expired air has had upon it; and now you begin to know that the atmosphere which we have spoiled by respiration is spoiled by carbonic acid, for you see it here in contact with the lime-water.



I have here two bottles, one containing lime-water and the other common water and tubes which pass into the bottles and connect them. The apparatus is very rough, but it is useful notwithstanding. If I take these two bottles, inhaling here and exhaling there, the arrangement of the tubes will prevent the air going backwards. The air coming in, will go to my mouth and lungs, and in going out, will pass through the lime-water, so that I can go on breathing and making an experiment, very refined in its nature, and very good in its results. You will observe that the good air has done nothing to the lime-water; in the other case nothing has come to the lime-water, but my respiration, and you see the difference in the two cases.

Let us now go a little further. What is all this process going on within us which we cannot do without, either day or night, which is so provided for by the Author of all things, that He has arranged that it shall be independent of all will? If we restrain our respiration, as we can to a certain extent, we should destroy ourselves. When we are asleep, the organs of respiration and the parts that are associated with them, still go on with their action, so necessary is this process of respiration to us, this contact of the air with the lungs. I must tell you in the briefest possible manner, what this process is. We consume food: the food goes through that strange set of vessels and organs within us and is brought into various parts of the system, into the digestive parts especially; and alternately the portion which is so changed, is carried through our lungs by one set of vessels, while the air that we inhale and exhale, is drawn into and thrown out of the lungs by another set of vessels, so that the air and the food come close together, separated only by an exceedingly thin surface: the air can thus act upon the blood by this process, producing precisely the same results in kind as we have seen in the case of the candle. The candle combines with parts of the air, forming carbonic acid, and evolves heat; so in the lungs there is this curious, wonderful change taking place. The air entering, combines with the carbon (not carbon in a free state, but, as in this case, placed ready for action at the moment), and makes carbonic acid, and is so thrown out into the atmosphere, and thus this singular result takes place; we may thus look upon the food as fuel. Let me take that piece of sugar, which will serve my purpose. It is a compound of carbon, hydrogen, and oxygen, similar to a candle, as containing the same elements, though not in the same proportion; the proportions being as shown in this table:—



Sugar.

Carbon	72	
Hydrogen	11	99
Oxygen	88	

This is, indeed, a most curious thing, which you can well remember, for the oxygen and hydrogen are in exactly the proportions which form water, so that sugar is compounded of 72 parts of carbon and 99 parts of water; and it is the carbon in the sugar that combines with the oxygen carried in by the air in the process of respiration, so making us like candles; producing these actions, warmth, and far more wonderful results besides, for the sustenance of the system, by a most beautiful and simple process. To make this still more striking, I will take a little sugar, or to make the experiment shorter I will use some syrup, which contains about three-fourths of sugar and a little water. If I put a little oil of vitriol on it it takes away the water, and leaves the carbon in a black mass. [The lecturer mixed the two together.] You see how the carbon is coming out, and before long we shall have a solid mass of charcoal, all of which has come out of sugar. Sugar, as you know, is food, and here we have absolutely a solid lump of carbon where you would not have expected it. And if I make arrangements so as to oxidise the carbon of sugar, we shall have a much more striking result. Here is sugar, and I have here an oxidiser—a quicker one than the atmosphere; and so we shall oxidise this fuel by a process different from respiration in its form, though not different in its kind. It is the combustion of the carbon by the contact of oxygen which the body has supplied to it.

If I set this into action at once you will see combustion produced. Just what takes place in my lungs—taking in oxygen from another source, namely, the atmosphere, takes place here by a more rapid process.

You will be astonished when I tell you what this curious play of carbon amounts to. A candle will burn some four or five, or six, or seven hours. What then must be the daily amount of carbon going up into the air in the way of carbonic acid! What a quantity of carbon must go from each of us in respiration! What a wonderful change of carbon must take place under these circumstances of combustion or respiration! A man in twenty-four hours converts as much as seven ounces of carbon into carbonic acid; a milch cow will convert seventy ounces, and a horse seventy-nine ounces, solely by the act of respiration. That is, the horse in twenty-four hours burns seventy-nine ounces of charcoal, or carbon, in his organs of respiration to supply his natural warmth in that time. All the warm-blooded animals get their warmth in this way, by the conversion of carbon, not in a free state, but in a state of combination. And what an extraordinary notion this gives us of the alterations going on in our atmosphere. As much as 5,000,000 pounds, or 548 tons, of carbonic acid is formed by respiration in London alone in twenty-four hours. And where does all this go? Up into the air. If the carbon had been like the lead which I showed you, or the iron which, in burning, produces a solid substance, what would happen? Combustion could not go on. As charcoal burns it becomes a vapour and passes off into the atmosphere, which is the great vehicle, the great carrier for conveying it away to other places. Then what becomes of it? Wonderful is it to find that the change produced by respiration, which seems so injurious to us (for we cannot breathe air twice over) is the very life and support of plants and vegetables that grow upon the surface of the earth. It is the same also under the surface, in the great bodies of water, for fishes and other animals respire upon the same principle, though not exactly by contact with the open air. Such fish as I have here [pointing to a globe of gold-fish] respire by the oxygen in the air, which is dissolved by the water, and form carbonic acid, and they all move about to produce the one great work of making the animal and vegetable kingdoms subservient to each other. And all the plants growing upon the surface of the earth, like that which I have brought here to serve as an illustration, absorb carbon: these leaves are taking up their carbon from the atmosphere to which we have given it in the form of carbonic acid, and they are growing and prospering. Give them a pure air like ours, and they could not live in it; give them carbon with other matters, and they live and rejoice. This piece of wood gets all its carbon, as the trees and plants get theirs, from the atmosphere, which, as we have seen, carries away what is bad for us and at the same time good for them,—what is disease to the one being health to the other. So are we made dependent not merely upon our fellow-creatures but upon our fellow-existers, all Nature being tied together by the laws that make one part conduce to the good of another.

There is another little point which I must mention before we draw to a close—a point which concerns the whole of these operations, and most curious and beautiful it is to see it clustering upon and associated with the bodies that concern us—oxygen, hydrogen, and carbon, in different states of their existence. I showed you just now some powdered lead, which I set burning; and you saw that the moment the fuel was brought to the air it acted, even before it got out of the bottle,—the moment the air crept in it acted. Now, there is a case of chemical affinity by which all our operations proceed. When we breathe the same operation is going on within us. When we burn a candle the attraction of the different parts one to the other is going on. Here it is going on in this case of the lead, and it is a beautiful instance of chemical affinity. If the products of combustion rose off from the surface, the lead would take fire, and go on burning to the end; but, you remember, that we have this difference between charcoal and lead—that, while the lead can start into action at once if there be access of air to it, the carbon will remain days, weeks, months, or years. The manuscripts of Herculaneum were written with carbonaceous ink, and there they have been for 1800 years or more, not having been at all changed by the atmosphere, though coming under various circumstances in contact with it. Now, what is the circumstance which makes the lead and carbon differ in this respect? It is a striking thing to see that the matter which is appointed to serve the purpose of fuel *waits* in its action; it does not start off burning, like the lead and many other things that I could show you, but which I have not encumbered the table with; but it waits for action. This waiting is a curious and wonderful thing. Candles—those Japanese candles, for instance—do not start into action at once like the lead or iron (for iron finely divided does the same thing as lead), but there they wait for years, perhaps for ages, without undergoing any alteration. I have here a supply of coal-gas. The jet is giving forth the gas, but you see it does not take fire—it comes out into the air, but it waits till it is hot enough before it burns. If I make it hot enough it takes fire. If I blow it out the gas that is issuing forth waits till the light is applied to it again. It is curious to see how different substances wait—how some will wait till the temperature is raised a little, and others till it is raised a good deal. I have here a little gunpowder and some gun-cotton; even these things differ in the conditions under which they will burn. The gunpowder is composed of carbon and other substances, making it highly combustible; and the gun-cotton is another combustible preparation. They are both waiting, but they will start into activity at different degrees of heat, or under different conditions. By applying a heated wire to them we shall see which will start first [touching the gun-cotton with the hot iron]. You see the gun-cotton has gone off, but not even the hottest part of the wire is now hot enough to fire the gunpowder. How beautifully that shows you the difference in the degree in which bodies act in this way. In the one case the substance will wait any time until the associated bodies are made active by heat; but, in the other, as in the process of respiration, it waits no time. In the lungs, as soon as the air enters, it unites with the carbon, even in the lowest temperature which the body can bear short of being frozen, the action begins at once, producing the carbonic acid of respiration; and so all things go on fitly and properly. Thus you see the analogy between respiration and combustion is rendered still more beautiful and striking. Indeed, all I can say to you at the end of these Lectures (for we must come to an end at one time or other) is to express a wish that you may, in your generation, be fit to compare to a candle; that you may, like it, shine as lights to those about you; that, in all your actions, you may justify the beauty of the taper by making your deeds honourable and effectual in the discharge of your duty to your fellow-men.

SOCIETY OF ARTS.[1]

Wednesday, January 30, 1861.

W. H. Bodkin, *Esq., Assistant Judge for Middlesex, Vice-President, in the Chair.*

Mr. Wentworth Lascelles Scott read the following paper "*On Food; its Adulterations, and the Methods of Detecting them.*"

It is with no little diffidence that I have undertaken to address you this evening upon the subject of our food and drink—its various impurities and sophistications; a subject of such ample scope and grave importance, that I feel quite incapable of doing justice to it in the brief hour allotted for the reading of this paper. Nevertheless, important nationally and individually as the subject is, it has not hitherto received a commensurate amount of attention from scientific men, the commercial world, or the general public; indeed, at the present moment, the latter are accused of apathy and indifference in the matter—but the charge is a false one, as I shall presently endeavour to show.

The practice of adulterating articles of food and drink as well as those myriad non-alimentary substances so necessary to our comfort in various ways, is by no means of recent origin, as, according to Pliny, the wines of his period were largely adulterated; lead, either in the metallic form, or in that of acetate, being added, to work off the sourness of new or inferior varieties; while, if I am not mistaken, our Saxon Harold greatly distrusted the confectionary department (as well as the holy relics) of Duke William's palace, when on his memorable and decidedly uncomfortable visit to that crafty potentate. A history of the "rise and progress" of adulteration, however, although it might be interesting to some, is quite removed from my present purpose, which has to deal exclusively with the evil propensities of our own time and our native country.

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[1] From the *Journal of the Society of Arts.*

Adulteration, in the more comprehensive sense of the word, may be stated to signify, 1st, The dilution of any articles for sale as genuine with other substances of inferior commercial value or less marked properties, as instanced in the addition of water to wine or spirits, or of chicory, &c., to coffee. 2ndly, The heightening or simulation of their active principles, by the admixture of various matters foreign to their legitimate composition, of which examples may be found in the falsification of bread with alum, gin with capsicums, &c. 3rdly, The employment of various colours, essences, &c., for the purpose of rendering the articles more attractive to the eye, or to the senses of taste and smell. In this division may be placed bottled fruits and pickles, rendered green by cupreous salts; various syrups and light beverages, coloured and flavoured to a dangerous degree, and in fact, confectionary *in toto*. Lastly, any combination may be made of these several kinds of adulteration in the fabrication or getting up for the market, of any of those cheap food-preparations, so terribly abundant in poorer neighbourhoods, and but too frequently met with in fashionable localities.

Adulteration, however, is not quite confined to the types I have just indicated, for if a manufacturer of any one of the almost countless "foods, drinks, condiments, or narcotics," with which we are acquainted has an accident in the process of that manufacture, or if his consignment of a certain ingredient is of a very inferior quality, does he put aside for other purposes the deteriorated article, or explain its deficiencies, and sell it at a lower price? No; in the vast majority of instances he proceeds in the manufacture of that article as usual, adding thereto various colouring or flavouring adulterations (which may be poisonous, injurious, or harmless, as chance disposes), to simulate the physical qualities it ought to possess, or conceal their absence by heightening the tint, pungency, &c.

Again, I ask, in how many cases will the retail tradesman confess that an article which has been exposed in his shop window for a month or two, to the influences of sun, dust, and atmosphere, is injured in appearance and properties? I have here an illustrative specimen—some "crystallised" sugar, containing about 3 or 4 per cent. of dust, sold to me as the "same quality" as that which attracted admiring observers to the window, by its superior lustre and whiteness. Clearly this is adulteration, should be recognised as such, and should be punished as such. But perhaps the most heartless and pernicious species of adulteration, is that shown when persons make it their rule to manufacture nothing else but inferior, adulterated, and deleterious articles, buying up all sorts of injured produce, and refuse matter, for the compounding of their "pure and nutritious" or "health-giving" foods, as their advertisements would say;—relying upon an ambiguous but high-flown testimonial from "the late Dr. Blank," or some other celebrity of the same moral calibre, and also upon the tendency of a too-confiding public to purchase "cheap" things, to get rid of their vile, perhaps poisonous, mixture at an immense profit.

It must be readily seen that an intelligible and practical classification of this complex subject is somewhat difficult. I have attempted, however, to embody in the form of tables a kind of general outline of adulteration and its effects, which, if in the hands of the people at large, might possibly guard them from some few impositions.

Having now made up our minds that the cause of adulteration is simply that fraudulent tradesmen wish to acquire, in an indolent manner, more money than the honest man can obtain by years of toil and application, we have to consider, in the first place, by what methods we can discover and tangibly demonstrate the presence of adulterants of any kind in articles sold as genuine, and find out in what proportion they have been added. Secondly, what measures, social, scientific, or legislative, require to be taken for the suppression or diminution of this great and growing evil; and, lastly, what part in such measures this Society, known throughout the world by its beneficial influences upon all things tending to advance the knowledge or promote the welfare of our countrymen, seems to be called upon to perform.

The limits of my time to-night are, of course, far too narrow for me to enter upon a detailed account of the various analytical processes for the detection of food-adulterants known to modern chemists, or employed by myself, but, for the benefit of those persons (forming the greater portion of the public) who are unacquainted with any means of testing the purity of their food, and who are unprovided with the complicated and costly apparatus of the analyst's laboratory, I will briefly narrate what everyone might do, and, as I hold ought to do, with the various articles of his daily food and drink.

I will commence with what has been aptly and poetically denominated the "staff of life"—bread. Recent analytical experiences, however, show very forcibly that in most cases the "staff," as supplied in the form of "half-quarterns," is but a very frail reed indeed for a working-man to lean upon. Bread is adulterated with a variety of substances not legitimately entering into its composition—some harmless, some injurious to health, but none, to my knowledge, of an actively poisonous nature.[2] The general characters of pure bread are tolerably well-known to most people, and after Dr. Dauglish's able paper at this Society last Session, I should only be recapitulating were I to enumerate them. Of pure bread there are several specimens on the table, illustrating Dr. Dauglish's process, as carried out by Messrs. Peek and Frean, and the ordinary method, of which latter Mr. W. Salmon, of the King's Road, Chelsea, has furnished me with samples.

When bread is adulterated with other flours or starches the admixture is often somewhat difficult to detect without the aid of a good microscope, but some of the simpler and less expensive varieties of this valuable instrument will frequently answer the purpose. The presence of potatoes in bread imparts a peculiar crumbly texture, which a very casual inspection will enable us to recognise; while rice-flour causes a brittleness of structure not easy to mistake. Nitric acid affords a means of showing the presence of potato-starch when in large quantity, as it will give a coloration to wheat flour, while the other farina remains white. Maize, or Indian corn-flour, may be detected (without a microscope), if a little care is taken, by roughly estimating the amount of fatty matter, as maize contains fully four times the quantity of fat that is usually found in wheat. The bread should be carefully and completely dried, powdered, weighed, and washed with (what is sold for) pure benzole or benzine, upon a filter, then dried perfectly and weighed again. The loss upon the former weighing will give the amount of fat or oil extracted by the benzole.

Oatmeal also contains a comparatively large quantity of fat, but is seldom, if ever, used as an adulterant for bread or flour in this country. No instance has come under my own observation.

Chalk, or carbonate of lime, is shown to be present if the suspected bread effervesces strongly on the addition of an acid. If gypsum, or plaster of Paris be looked for, boil the bread in water[3] for a long time, till quite broken up and partly dissolved; then, after standing, pour off, wash the sediment (if any), and boil the latter in a solution of pure carbonate of soda; filter, or pour off the clear liquid, to which add some chloride of barium; if this gives a decided milkiness, and the washed sediment an effervescence with acids, the bread has been adulterated with plaster of Paris—a substance of great importance to fraudulent tradesmen, who use it extensively in the manufacture of confectionary (when it goes under the name of "daft"), mustard, and many other articles.

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[2] Sulphate of copper (bluestone) is used by Belgian and Austrian bakers, but not in this country.

[3] The water should be distilled.

I now come to speak of a substance concerning which there has been a great deal of discussion in this room and elsewhere, both as to the best methods of detecting it. and also on the question of its presence being hurtful or the reverse—I refer to alum. This is about the most frequent adulterant of bread, very few samples being uncontaminated by this salt. I may here mention, as an act of simple justice, that the sample of fermented bread before you on the table, is from the shop of Mr. Salmon, who is almost the only baker in the district of Chelsea, so far as my knowledge extends, who sells perfectly pure and unadulterated bread. This may be considered a bold assertion, but it is my well-founded opinion, nevertheless.

Alum, when present in pretty considerable quantity, may be detected by soaking the bread in water for some time and adding a salt of barium to the clear extract, when a cloudiness, more or less marked, will occur, an experiment I think I may be able to show you. Another way is to char the bread, and burn it nearly to an ash, and boil the latter with diluted hydrochloric acid, to which a little nitric acid has been added; the addition of ammonia to the filtered liquid will precipitate various substances, including the alumina of the alum. This precipitate must then be boiled in potassa, when, after filtration, the liquid must be neutralised with hydrochloric acid, and the alumina may then be thrown down by ammonia.

Bone-dust is now but seldom found in bread; it may be detected very readily with the microscope. The preceding observations apply of course equally well to flour which is very largely adulterated with inferior varieties of wheat andother meals in addition to alum and the adulterations before mentioned. My experiences show that on an average the bread in London is more or less adulterated to the amount of 87 per cent.—by which I mean to say that if I were to buy 100 loaves, at as many different bakers in various parts of the metropolis, I should get about 13 of good and genuine bread. At this present time I should probably not get quite so many good loaves, as the late bad harvest has thrown large quantities of damaged corn on the market, which of course is adulterated proportionately.

The adulteration of cakes, biscuits, and similar articles are chiefly those of the several ingredients composing the same with but few special additions. Saffron, gamboge, chrome yellow, &c., are, however, frequently used in the manufacture of sponge-cakes and Bath buns, for the purpose of deluding the unfortunate school-boy whose weakness lies in that direction into the belief that no expense has been spared in the matter of eggs; gastric, and probably another kind of irritation often follows the experiment. Messrs. Huntley and Palmer have favoured me with some specimens of the various biscuits made by them, as regards which they say, "it is impossible to select the articles of our manufactures of better quality or of greater purity"—an assertion my own extended observations have fully borne out. I may also direct your attention to the pure biscuits of Messrs. Peek and Frean, to those of Messrs. Hill and Jones, and to some intended for the nursery made by Mr. Salmon. Samples of all these are on the table.

Corn is subject to a variety of diseases, which often greatly impair its quality, and even render it dangerous as food; of these perhaps the *ergot fungus* and the pepper-brand (*uredo fœtida*) are the most disgusting in their effects, the former acting sometimes like an irritant poison, and the latter is said to have occasioned skin-diseases. Corn or meal affected with the smut or pepper-brand is at once distinguished by the disagreeable odour it emits and thus may be bought up cheaply, which to my personal knowlege is often done for the purpose of making cheap highly-flavoured cakes and puddings with the flour, which is also used for adulterating mustard, pepper, ginger, and other articles having a powerful odour of their own, which conceals that emanating from the uredo fungus. Another variety of uredo (the *uredo segetum*) is known to growers by the name of dust-brand, black-burn, or smut, but though injurious and destructive to growing crops, it does not appear to deteriorate the flour to any very great extent. I have, however, noticed a deficiency of nitrogen in wheat affected by smut. Various insects, too, such as the *musca pumilionis*, *cecidomyia tritici*, &c., attack and injure corn. I may not, however, dilate upon them now, but will refer those interested in the subject to the able papers of Mr. E. Quekett and Professor Henslow. An inquiry into the causes of the cereal fungi and the means of preventing their occurrence, would be one of great scientific interest and practical value, and in my opinion would be accomplished without great difficulty, if this Society were to offer a prize for the research. While on the subject of corn, I may mention a notable instance of its adulteration, which came under my notice at Wakefield some few years back. A gentleman purchased a quantity of wheat grain at the market there, at a rather lower rate than ordinary, for the apparent quality. After-examination showed that the bulk of the sacks were filled in with shrivelled or diseased wheat and barley, the uppermost twelve or fourteen inches being grain of fair quality. Certain circumstances which I need not relate rendered redress impossible.

Barley and oatmeal are frequently adulterated with inferior samples of either, and our poor horses frequently get some five-and-twenty per cent. of brewers' or distillers' "grains" served out to them in their reputed measure of oats.[4]

Peas, beans, lentils, and other *leguminosæ*, all come in for a share of adulteration; albeit, they are used largely as adulterants themselves. I may here remark that, in the examination of all cereal and pulse flours, starches, prepared farinas, &c., I have found that the previous abstraction of the oil or fat, by means of benzole, greatly assists the investigation, especially for the microscope, where very slight differences of the form and structure of starch granules have to be observed. Generally speaking, the too liberal use of alcohol or ether, as recommended in many works, should be avoided, for those liquids will occasionally produce a slight alteration in some of the granules, by cracking or wrinkling the outer membrane, and, by making them appear more like other starches, render their true origin doubtful. I am, perhaps, a little prolix on this point, as I believe that the errors now and then said to be made by microscopists may be due to this cause. The starches afforded by various plants all differ in size, form, or character of surface when viewed under a good microscope, and may be thus distinguished from one another. Into these literally microscopic details we may not enter now, but I have arranged a series of the more important starches, which Mr. Baker's very excellent microscopes will enable you to compare. Unforeseen circumstances have prevented my appending to this paper some notes on the microscopy of starches, and a table of their micrometric measurements.

Arrowroot is a farina or starch of remarkable purity—that is, the genuine "Maranta" variety; but out of 100 samples bought promiscuously in London, about 48 only would be genuine, and all those even would not prove of first-rate quality, as many of the samples imported are contaminated with albuminous and ligneous matter, from having been carelessly prepared. The adulterants are, as might naturally be supposed, various other starches and farinas, those most in vogue being potato and wheat starches, and sago meal. It has been said, and generally with truth, that the adulteration of arrowroot is of comparatively little consequence since none of the adulterants are detrimental to health, and they all answer the intended purpose nearly as well as the genuine article; but I have had a sample of "genuine Maranta arrowroot" containing a large quantity of barley starch of a bad colour, and a notable amount of flake-white—a carbonate of lead, and a substance not calculated to preserve one's health or invigorate the system.

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[4] The rape cake given to horses and cows often contains a quantity of refuse mustard-seed, which always proves dangerous, and sometimes fatal, to the former animal.

Maranta arrowroot gives an opaque paste when mixed with about twice its volume of strong hydrochloric acid, while potato-starch, under similar circumstances, yields a translucent jelly.

There are various other arrowroots known to trade, such as "East India arrowroot," derived from the tubers of *Curcuma angustifolia*, that from Tahiti (from the *Tacca oceanica*); "Brazilian arrowroot" (the farina of the *Manihot utilissima*), and many others. They are not often to be bought pure at retail shops.

I must necessarily pass over a number of mixed or "prepared" farinas with high-sounding titles, and, if we are to believe the packet-labels, superlative powers of restoring health, happiness, and peace of mind, in cases "where many eminent physicians had failed to effect a cure;" they are, without exception, frauds of the worst description, and the selling of such articles should be distinctly known to the public, as, in fact, equivalent to the grave offence of "obtaining money under false pretences." On this section of the subject, Dr. Hassall and the Editor of the *Lancet* have done good service by exposing many of these wretched impostors.

More recently, however, various manufacturers, importers, and others, have evinced a laudable desire to supply the public with pure food, and these I hold ought to receive every encouragement, as they frequently labour under very great difficulties from the opposition against which they have to contend. To cite an instance, I will point to Brown and Polson's Patent Corn-flour—an article of which I have a very high opinion, as it professes to be nothing but what it is—the fecula of maize, very carefully prepared. Specimens of this starch, or flour, are on the table, and also of the gluten and husk, which, when mixed, form an excellent occasional food for horses, cattle, &c.

Even this prepared flour, however, apparently protected by the signature and trade-mark of the manufacturers, is sometimes adulterated. I met with an instance at Islington, where, with the "trade-mark" slightly altered, some potato starch and barley flour, with but little of the maize, was sold to me for "Brown and Polson's patent farina." I am inclined to think that, in this instance, the "corn flour" was used to adulterate arrowroot, while the packets were refilled with the above mixture.

Passing from bread and farinas, we naturally come to meat, as the next staple component of our daily food. Here, again, we have ample scope for detective examination and remedial legislation, for not only is a great deal of that commonly sold not of the quality it should be, but, of the meat disposed of at the London markets, nearly ten per cent. is more or less unfit for human consumption. It is well known that butchers will adopt any means of making their meat appear better and fresher than it really is, and accordingly we not unfrequently find that washes of vinegar and water are used to heighten the colour of some meats; weak alkaline leys to improve the appearance of others; and, when decomposition or disease renders more potent agents necessary, even arsenical solutions are employed without hesitation. With game and poultry this latter practice is, indeed, quite common, and I call upon you to-night, as a small but influential portion of the British public, to express unmistakeably your opinion of such practices, and to do all in your power to suppress them.

Vegetables and fruits here follow most appropriately—their adulterations and deteriorations must be known to you all, and also the methods of detecting the same, but I may perhaps name a few points without being accused of tediousness.

The question has often been put to me—Can fruit ever be adulterated? As often, my answer has been in the affirmative. Without going into what might be called the natural and accidental adulterations—such as blight, mildew, insects, &c., I may mention that I have seen English apples, of rather inferior quality than otherwise, coloured superficially in imitation of the American Newtown Pippins, and sold as such at the rate of two and three shillings per dozen! Although we may admire the artistic genius thus displayed, we must still condemn the fraud. Old and inferior oranges, well boiled, with a little saffron added to assist Nature when her short-comings in the matter of external colouring are a little too obvious, may be purchased at many shops, as we all know. Melons and cucumbers, too, when looking pale and dejected from waiting so long to be eaten, have their rusty coats furbished up with a little acetate of copper, so that a "green old age" at least is accorded to them.

The mention of verdigris reminds us to turn to pickles, sauces, condiments, and other similar contrivances for manufacturing appetites for debilitated humanity. Here the field is so wide, as regards adulterants of all kinds and species, that a hasty glance at them is all we can afford to give.

Before considering the special adulterations of pickles, it will be well to note those practised with the several articles used in their manufacture. We will commence with vinegar, which is largely sophisticated with water and crude acetic acid,[5] sulphuric acid, or oil of vitriol, together with burnt sugar, and other materials for colouring. The strength of vinegar (*i.e.*, the per-centage of acetic acid therein) can only be accurately estimated by employing an alkalimeter, and discovering by such means how many grains of dry carbonate of soda are required to neutralise a given measure of the vinegar. A more simple, but less exact, method is to weigh carefully a small, dry, white lump of Carrara marble, and to place the same in a known weight or measure of the vinegar[6] until effervescence has entirely ceased. Then the marble may be rinsed, dried, and re-weighed, when a loss of five grains upon its original weight would show the presence of six grains of monohydrated acetic acid in the measure of vinegar taken for the experiment. The vinegar of commerce may be divided into two classes—fermented and distilled vinegar; the former derived from either sugar, malt, or wine, the latter obtained by the distillation of wood. According to the researches of Dr. Hassall, the per-centage of acetic acid in ordinary vinegar varies from 2·38 to 5·66 per cent., a disparity sufficiently large to show how greatly standard strengths are required resembling those employed in determining the value of spirits. Very weak vinegars should always be discarded, as they are unable to prevent the decomposition of meat and vegetables, while the extremely pungent varieties are immediately open to suspicions of cayenne pepper and sulphuric acid. The latter is very readily detected by the addition of a barium salt, which throws down a white, insoluble precipitate if the acid is present. Now, vinegar may often contain a small quantity of combined sulphuric acid, derived from the water with which it is made and diluted, and $\frac{1}{1000}$th part of pure acid is permitted to be added by law, although there is never any real occasion for this, as, if the vinegar itself is good, it will keep for any time. On this account vinegar will generally give a milkiness with chloride (or nitrate) of barium, but of course not to such an extent as when oil of vitriol is distinctly added as an adulteration. I have myself found as much as 7·4 grains of sulphuric acid in 1000 of vinegar, I have also once detected oxalic, and in another sample formic acid (evidently having been purposely added),—facts which I believe have not been noticed heretofore. The onions and cauliflowers in pickles are generally slightly tinted with turmeric, but as this colouring matter is itself frequently adulterated with the poisonous chromate of lead, its employment is often fraught with danger to the consumer. On the other hand, with the cucumbers, girkins, and French beans, the custom is to render them considerably greener than they ever were by nature, by the addition of salts of copper. This may be accomplished in several ways—either by directly adding verdigris, oxide of copper, or sulphate of copper, ("bluestone") to the pickle, or by leaving the vinegar in copper pans for some time so as to dissolve a portion of the metal. In relation to pickles, I must say that consumers are open to very great blame, for the too general rule with them is to buy those pickles which are of the deepest green, for no better reason, it would appear, than that they are considered prettier. It is quite time all such weaknesses were dissipated, and I hold that he who chooses to buy girkins where he can get them greenest is of the same intellectual tint personally, and, whenever he purchases a pickle, deserves also to get into one. For various specimens, and much valuable information on this subject, I am indebted to Messrs. Crosse and Blackwell, who certainly evince a great desire to deal fairly with the public —if the public will only allow them to do so, and not persist in asking for green pickles, bright yellow mustard, and intensely red cayenne pepper; I can conscientiously affirm that these dangerous articles cannot now be purchased in Soho Square.

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[5] Derived from the destructive distillation of wood.

[6] After the sulphuric acid present (if any) has been separated by the addition of a little chloride of barium.

I have here a bottle of pure pickles, and here an adulterated sample; the difference in tint, you see, is very apparent, and if I add to both a little solution of ammonia, the presence of a large quantity of copper in the one case, and its entire absence in the other, will be speedily shown. The properties of dissolved copper, as an active poison and powerful irritant, are very generally known; it is a great pity that people will not think of them a little oftener than they do. According to my calculation, about 91 per cent. of the pickles sold in London are more or less adulterated, and in Liverpool the proportion is often greater still.

I have now to exhibit what might justly be considered a very great curiosity—one, too, which it is probable that not many of those present have ever seen before—I refer to this specimen of genuine mustard, kindly procured by a friend, specially for me. Even this, however, is not absolutely pure, for the microscope detects traces of wheat-flour in the sample—looking rather as if the mill which ground it had indulged in bad habits so long that they could never be entirely eradicated. The chief adulterants of mustard are plaster of Paris, chalk, clay, quick-lime, various flours and starches, cayenne and other peppers, annatto, turmeric, gamboge, and chromate of lead, all of which come in also for making "ground ginger." I once bought a sample of mustard so largely adulterated with *fresh* plaster of Paris, that when mixed with water it "set" into a solid cake in the course of a few minutes.

I will mix a little of my genuine sample here and also some bought in a packet, which I know to be adulterated with flour and other matters. You will observe that the addition of ammonia causes no change in the good sample, while the adulterated is altered in colour to a reddish brown, thus proving the presence of turmeric. Ginger is falsified so much in the same manner that I need not again refer to it. Ginger is seldom to be met with pure, mustard *never*. The microscope furnishes the only reliable method of testing for flours and starches, in ground spices and condiments.

Turmeric is a usual and legitimate component of currie powder, which I am told cannot be made without it—a broad assertion I am greatly inclined to doubt. It is greatly falsified with various injurious and poisonous yellow colours, and as currie may contain almost every conceivable abomination, under cover of "peculiarity of the original recipe," I do not see how its adulteration can be prevented unless by the adoption of standard recipes. The specimen of currie powder on the table is quite free from all injurious admixture.

The adulterations of pepper present a few points of interest, so they must not pass quite unnoticed, for in addition to the usual flours, meals, &c., we find lamp-black, black-lead, coal ashes, and even road dust in some samples. In New Orleans, Philadelphia, Liverpool, and occasionally in this metropolis, a "pepper" is sold consisting of old ships or dogs' biscuits (such as those upon the table, but in a considerably worse condition); first soaked in an infusion of capsicums, then dried and ground finely with a little lime, and any other little additions in the way of colouring matter that might be required. This would be "nice and strong"—anyhow, "almost too genuine," as I have heard some people say in their innocence, when commenting upon a sample more than usually adulterated.

Mace and nutmegs are chiefly adulterated with damaged and inferior specimens, and occasionally a portion of the aromatic oil is extracted before they are sold, thus diminishing their intrinsic value. According to Chevallier,[7] nutmegs are frequently adulterated with worm-eaten varieties, the apertures being filled up with a paste made of nutmeg powder, flour, and oil; the same composition is used to form imitation nutmegs. In the United States wooden nutmegs are now and then sold along with the purchaser.

Somewhat similar remarks will apply to cinnamon, cloves, cassia, and pimento; the first, however, is, I believe, imitated, but inferior and spent varieties of all are used as adulterants. Microscopical examination shows when the samples have been previously boiled, as the granules are then much altered in appearance and augmented in size. With ground spices, of course, adulteration flourishes vigorously to an average of about 60 per cent. in the larger towns of Britain, and a still higher figure in country villages.

For an illustrative collection of genuine spices, I am beholden to Messrs. Travers and Sons, of St. Swithin's Lane; the specimens are now before you: I have here a pure sample of cayenne pepper, and also one adulterated with, among other things, red-lead; by sprinkling some of each upon the surface of some water they are readily distinguished, as the mineral poison will sink to the bottom whenever it is present.

The preservation of food-substances is a subject of no little importance, upon which a dozen papers might be written without fear of exhausting it; I can therefore only select a point or two here and there, for discussion to-night. Preserved fruits and vegetables are greatly sophisticated, a fact the more to be regretted, because it is evident the guilty parties must be the original manufacturers—not the retail tradesmen. The observations concerning green pickles may be wholly applied to green preserves. Various colouring matters are employed in preparing the other varieties, but these are seldom of a deleterious nature, as body colours are quite inadmissible. Gamboge, however, is a favourite addition for inferior marmalades; it requires to be carefully distinguished from turmeric, which it somewhat resembles; in testing for either colouring matter in preserves, strong alcohol should be used as the solvent.

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[7] "Dictionnaires des Altérations et Falsifications des Substances Alimentaires."

Specimens of various bottled fruits, of Messrs. Crosse and Blackwell's manufacture are on the table; they are prepared by a process as simple as it is efficient; the carefully-picked fruit, with the addition of a small quantity of water, is introduced into wide-mouthed bottles, which are placed up to their necks in large cisterns, or water-baths, heated to a temptrature of 200°, where they remain for about 24 hours; they are then taken out and stowed away in cellars until required. Some kinds of vegetables, such as green peas, are treated in a similar manner, cylindrical tin cases being substituted for bottles. I learn from an inspection of the works of the firm in question that no less than 9370 tins of green peas were sealed at their works in 1860, while of the fruits 51,000 dozen bottles, and of preserves, or, as they are more popularly called, jams, nearly 250 tons were prepared within the same year. I merely mention these statistics to show that if I describe any process or practice of Messrs. Crosse and Blackwell, I quote from no mean authority.

A point here occurs to me that is, I consider, too important to omit. There are a great number of varieties of tropical and other fruits which are seldom seen in this country, owing to the very great difficulty of transporting them safely; if, however, they were carefully suspended in metallic cases of peculiar construction, provided either with small tubes containing phosphorus or an alkaline solution of pyrogallic acid, or else with fragments of charcoal soaked in chloroform, I am fully convinced that the form, colour, and flavour of the banana, mango, mangosteen, alligator pear, plantain, and many other luscious and delicious fruits, might be known in this country, almost as perfectly as in their own.

Fruit and wine essences and syrups next claim our attention: in this department, a variety of sophistications are apparent, as the result of careless manipulation, the use of impure sugars and of a host of injurious colouring matters, or flavouring principles. Thus the liquid generally known as fusel-oil has a colour and flavour of a very disagreeable kind, and in many cases acts injuriously on the animal economy[8] but treated with various chemical re-agents, it at once becomes the source of several fruit ethers or flowering essences. Treated with bichromate of potassa and sulphuric acid, it yields by distilling the mixture, a volatile fluid, the *valerianate of amyle*, which is employed by confectioners as an essence of apples. The *acetate of amyle*, better known as essence of jargonelle pears, is the result of the distillation of fusel-oil with acetate of potassa and sulphuric acid. Melon-essence (*coccinate of ethyle*) is also artificially prepared from ordinary, or wine ethers; cocoa-nut-oil, the essence of quinces (*pelargonic ether*) by distilling together oil of rue and nitric acid, and that of pine-apples (*butyrate of ethyle*) by saponifying butter and distilling the resulting soap with sulphuric acid and alcohol. Benzole, too, under the influence of concentrated nitric acid, yields a very good imitation, in point of flavour, of essence of almonds, and, accordingly, is largely employed for the purpose. All these artificial essences, and a great many others, would probably effect their intended object much better than the natural ones, if due care were exercised in their preparation, which, unfortunately, is not very often the case; we are, as yet, greatly in the dark upon the subject, which presents a wide field for future investigation. Many of the compound ethers, I believe to be injurious to health, even in small quantities; fusel-oil, for instance, a very common impurity in essences, wines, and spirits, seems to have a powerfully narcotic effect upon some people, producing headache, nausea, and a tendency to vomit; while with others it is apparently harmless.

Sugar shall be our next article of food,—a rather prominent one, if we consider the quantity annually consumed in the United Kingdom, which, in 1859, amounted to no less than 8,641,927 cwt. of the raw variety, or about 32¼ lbs. per head of the population; and of refined sugar and sugar candy, 242,379 cwt., or, between 13 and 14 ounces per head.

Raw, moist, or brown sugar, as imported, is much contaminated with dust, fragments of cane, molasses, and some minute insects of the *acari* genus,—the *acarus sacchari* of Hassall, to whom the honour of their first discovery is due. Retail grocers, however, add to their sugars, for purposes of adulteration, inferior kinds, fine sand, sawdust, salt, water, flour, potato, and other starches. The best way of examining a sample of brown sugar is, in my opinion, to determine, in the first place, the per-centage of moisture, by carefully drying, at a temperature not exceeding 120°, a known weight of the sugar; the loss sustained will give the water contained in the same. The dried powder should then be placed upon a filter, and washed with cold distilled water until the washings are no longer perceptibly sweet, when the albuminous matters, if present, may be precipitated by boiling the solution.[9]

The insoluble matter, together with the filter, may now be dried and weighed, after which the starch granules, if any be present, can be recognised under the microscope with great facility; also *acari* and sporules of *fungi*, when they occur. Crystallised sugar is much more wholesome, and also economical variety, especially that made by the centrifugal process, as it is nearly always tolerably free from imported impurities. It is, however, constantly adulterated with the inferior kinds, so that purchasers should be particular in their selection. Loaf sugar may contain a few of the ordinary impurities, if not carefully and thoroughly refined; also traces of pipe-clay, albumen, &c. Broken lump sugar has been adulterated with fragments of white marble, but this is, of course, quite exceptional.

It is greatly to be regretted that the sale of the impure brown sugars of commerce should be permitted at all, as the amount of injury their use inflicts upon the public health can be by no means slight. I hold, therefore, that remedial measures are urgently needed here. The common practice of selecting the very coarsest and darkest coloured sugars (popularly known as *foots*) for the preparation of cakes, puddings, &c., is one which I cannot to strongly reprehend; if it be required to "make a cake look rich"—the usual pretext—why not employ a pure white sugar for the purpose of sweetening, and supply the place of the deficient dirt and colouring matter by a little wholesome burnt sugar, which will answer a great deal better. As to the finance part of the question—a delicate point with housekeepers—it will always be found cheaper to employ a fine white, or at least a light-coloured sugar, than to use a dark brown variety of a lower price.

The uses of sugar as a preservative agent are well known, and in the department of confectionary proper it holds the first place. Unluckily we insist upon having our bon-bons and conserves made, not only to please the palate, but also to attract the eye; thus has been called into existence the most universal and pernicious system of adulteration with which we are acquainted. It is easy to understand that in this department very large profits are to be made, if, as a rule, as much China clay, plaster of Paris, and flour, as possible are added to the various saccharine abominations manufactured, which are then ornamented with a variety of poisonous colours, and flavoured with some crude essence, to conceal more effectually their intrinsic inferiority. We will take the simple diluent adulterants first; the readiest methods of testing for these have already been given when speaking of the adulterations of bread, and therefore need not be repeated here. In all cases the suspected bon-bon, sugar-plum, or other article should be immersed for some time in distilled water, when the insoluble matter is then presented in a convenient form for examination. In some instances it may be found desirable to burn away all the organic matters, thus leaving the ash alone for analysis. Next, proceeding to the colouring ingredients, it should be carefully noted whether these are soluble or not in the water, alcohol, or ether; if only one tint is apparent the entire substance of the article may be taken for experiment, if several they must be carefully separated by means of a small sharp knife. I have condensed in a Table some general information relating to the chief colouring matters used for purposes of (what might be called) ornamental adulteration, amounting to upwards of sixty in number, of which twelve or thirteen are active and powerful poisons—a like number are harmless, except in special cases of disease or morbid debility—while the remainder are either more or less injurious to health, or we know too little about their effects upon the animal economy to venture a decided opinion either way. I beg here to record that my friend Dr. Benjamin W. Richardson has kindly brought his large experience to my assistance in this section of the subject, and that to him my best thanks are due, and are most gratefully proffered. Some pure confectionary of Messrs. Hill and Jones's manufacture is on the table before you.

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[8] The real properties of fusel-oil, or "*potato-spirit*" in this respect are as yet but very imperfectly known.

[9] These may, of course, be dried, and the weight ascertained if desirable.

It would absorb too much of our time to-night to narrate at length the method of testing for each colour, so — with some reluctance — I dismiss those points in a very few words. If a colour be soluble in water, it is far less likely to be an injurious mineral pigment than if it remain undissolved; it should be remembered, too, that alkalies will deepen into purples most of the animal and vegetable red colours, while acids will heighten them, and increase their brilliancy. Indigo may be recognised by mixing it with plaster of Paris, drying the cake thus formed, and cautiously heating the same in a glass tube until the indigo (if present) sublimes in minute crystals. Blue litmus is changed to red by addition of an acid, ammonia or any alkali restoring the original tint; by this latter addition the colour of Prussian blue is destroyed. The greens derived from copper or arsenic are readily detected by the various well-known tests for these metals, while the red and yellow lead colours are quickly identified by their deportment with sulphide of ammonium, which turns them black. Specimens of coloured confectionary are so familiar to the eye of almost every person, that I have considered it unnecessary to exhibit many to-night—shop-windows furnish examples without number daily.

Reverting for a time to the Animal Kingdom, we have there still four articles which claim our attention, viz., milk, butter, cheese, and lard. The two first are rarely to be met with pure in this metropolis, for the simple reason that they are very easily adulterated.

Water is the usual adulterant of milk, as the dairyman who has only 40 gallons of pure milk at his disposal, while he requires 50 or 60, finds the "cow with the iron tail" ever ready to aid him in making up the deficiency. I have myself found the quantity of extraneous water in London milk to vary from about 8 to 61 per cent., while out of 100 promiscuous samples, from all parts of this city, I believe the number, more or less adulterated, in one way or another, would be about 74. Large additions of water not only impart a bluish tint to milk, but also decrease its specific gravity; some yellow colouring matter is therefore added, generally annatto, turmeric, or gamboge; and the liquid may be thickened with various starches, mucilage, or, rarely, with the brains of some animal. This latter is best detected by the microscope. Starch is shown to be present if the milk, after separating the curd, give a blue colour with tincture of iodine. Improper feeding and housing of the cows, too, are fruitful sources of bad milk, which, when derived from diseased animals, certainly is injurious. Caution must always be exercised in determining the density of milk, as cream will diminish this very powerfully. The lactoscope of M. Donné is useful in determining the richness of milk; but a simpler, and perhaps a better instrument, was described some time since in the *Dublin Medical Press*, consisting of a thin, hollow wedge of glass, graduated on one side, into which milk can be introduced, and its opacity discovered by ascertaining at what thickness of the wedge the graduations can no longer be seen through. The instrument has also been described in the CHEMICAL NEWS.

(To be continued.)

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Quarterly Meeting, January 22, 1861.

Dr. J. P. JOULE, *President, in the Chair.*

THE following gentlemen were duly elected as honorary members:—Wilhelm Haidinger, Director-General of the I. R. Geological Institute, Vienna, and Professor James Joseph Sylvester, M.A., F.R.S.

As corresponding members:—Professor George Buckland, of University College, Toronto, and Professor Joseph Henry, Secretary of the Smithsonian Institute.

As ordinary members:—William Henry Fisher, Rev. Thomas Buckley, M.A., Simon Pincoffs, Rev. G. H. Greville Anson, M.A., Professor R. B. Clifton, B.A., John Shae Perring, William Radford, Thomas Alcock, M.D., Charles O'Neill, George Parr, jun., John Curtis, James Bottomley, and Francis Preston.

The new rules were submitted by the Council.

It was moved by Mr. BINNEY, seconded by Mr. MACLURE, and resolved:—

"That the rules proposed by the Council be adopted in place of those hitherto in force."

MICROSCOPICAL SECTION.

January 21, 1861.

Letters were read by the SECRETARY from Professor Huxley and from Mr. W. K. Parker, respecting Soundings.

Mr. HEYS, of Hazel Grove, read a paper "*On the Kaloscope*," his newly-invented instrument for the use of coloured light in the examination of objects under the microscope. This the author effects by two sets, of four discs each, of differently-coloured glass, two and a-half inches in diameter, mounted on a stand twelve inches high, one set of which is placed between the light and the bull's-eye condenser, and the other between the light and the mirror, underneath the stage, each disc having an independent motion, so that the light can be transmitted through one or more of both sets at the same time, when the object appears of the colours refracted and reflected through the discs. One of the important uses of the instrument is the protection of the eye from injury occasioned by the use of common artificial light. Many objects which do not polarise, by the kaloscope are made to disclose the beauties of polarised light; for instance, the anthers of the mallow, with their pollen, when viewed by means of red light below the stage, and, at the same time, green light (the complementary colour) through the condenser, appear of a beautiful green colour on a red or crimson ground. The author observes that some objects, viewed by means of the kaloscope, appear in such relief that they might be supposed to be seen through a stereoscope; these are anthers, jointed hairs, oil-glands, and vegetable sections in general. The calyx of the moss-rose is alluded to, under ordinary illumination, as a mere entanglement of fibres with dark beads; but by this method it is transformed into a stereoscopic branch, with glittering glands at its extremities. Sections of wood, spines of ecchini, &c., will be found as beautiful as with the polariscope; but, by another arrangement, details are brought out not observable with the latter instrument. A black surface being placed below the stage, coloured light is thrown very obliquely from the mirror, and the complementary colour through the condenser; hairs on the edges of leaves, petals, and filaments of stamens, &c., then appear, illuminated by the light of the condenser, of one colour, and fringed with the opposite colour on an intensely black ground. The author gives a list of the botanical names of objects advantageously illuminated by this method. A single coloured disc may be also used to advantage with white light from the bull's-eye lens. Details of structure are observable by means of this instrument, which, the author observed, are inconspicuous without its aid, and thinks that its efficacy in connection with such a variety of purposes cannot fail to render it of value to the scientific observer.

The reading of the paper gave much satisfaction to the members of the Section, and it was resolved to communicate the same to the Society, with a recommendation that it should be printed *in extenso* in its "Memoirs."

The Secretary read a paper "*On Preparing Objects found in Soundings*," and described Mr. Dale's process for disposing of the tallow by means of highly-rectified benzole, which is most effectual. The benzole,—called benzine by French chemists,—being recovered, to be used again as fast as required for a dozen filters, each with its specimen in process at the same time, with only trifling loss from evaporation.

This paper, and one by Mr. Dancer, on the same subject, were ordered to be printed by the Section, for circulation amongst its members.

Mr. Brothers presented to the Section a very old microscope, date unknown. He also exhibited the actinophris Eichornii, melicerta, sea-weed with lipraris, &c.

Mr. Hardman, of Davyhulme, presented three mounted specimens of the wireworm, and a number of dissecting-needles for the use of the members. He also exhibited a mounted fly, one of the *Panorpidæ*, which, he states, feeds upon leaf-rolling caterpillars. The proboscis and feet of the insect are peculiarly adapted for dragging its victims from their concealment and holding them whilst extracting their juices, the feet being provided with combs similar to those of the spider.

Mr. R. D. Darbyshire presented a quantity of mud, &c., from the washings of shells from the raised sea-bottoms at Uddevalla, in Sweden.

Mr. Dancer exhibited a new three-inch object-glass, with a large and flat field of view; also specimens of gold quartz from Wales, large curculia, and other objects.

Mr. Whalley exhibited some specimens of injections obtained from Germany, which were considered the best yet exhibited.

Mr. Latham exhibited various specimens of sand and mud from the East Indies, portions of which were distributed amongst tho members.

ROYAL DUBLIN SOCIETY.

Monday, January 21, 1861.

The Rev. Professor Galbraith, *F.S.C.D., in the Chair.*

Mr. David Moore, Curator of the Dublin Botanical Gardens, read an Account of a Botanical and Horticultural Tour made by him through portions of Germany, Holland, and Belgium, in the autumn of 1860.

The Rev. Professor Haughton brought under the notice of the meeting a Catalogue of a Geological and Geographical Collection of Minerals from Greenland, from Cape Farewell to Baffin's Bay, written by Sir Charles Giesecke, formerly Professor of Mineralogy to the Royal Dublin Society. The value of this topographical catalogue can hardly be over-estimated. It contains a detailed account of 356 minerals from 219 localities. Mr. Haughton said that he had been unable to find a map of Greenland containing the names mentioned in this valuable catalogue, which were all in the Esquimaux language, but he hoped to be able to determine the localities through the kindness of Sir Leopold McClintock, and to publish a detailed map with the catalogue of Giesecke. The minerals of Greenland, he said, were unique, and a detailed list of them, with a map, compiled by so competent an authority as Sir Charles Giesecke, would form a most valuable addition to science. He (Mr. Haughton) regretted that the Royal Dublin Society had not performed this duty during the lifetime of their distinguished Professor of Mineralogy.

The Rev. Professor Haughton also read a paper on the Composition of a Leek-gum Feldspar from Greenland in the collection of the Society. It consisted of silica, alumina, sesquioxide of iron, lime, magnesia, soda, and potash, in the following quantities:—

Silica	64·40
Alumina	18·96
Sesquioxide of iron	1·04
Lime	0·45
Magnesia	0·14
Soda	2·35
Potash	13·07
	100·41

This mineral had, therefore, the general formula,—

$$M_2O_3 . 3 SiO_3 + M 3 . SiO_3.$$

CORRESPONDENCE.

Effect of Oxalate of Ammonia upon the Precipitation of Prussian Blue.

To the Editor of the Chemical News.

Sir,—On repeating the experiment of Professor Arnandon on the influence of oxalate of ammonia in modifying the action of prussiate of potash on a salt of peroxide of iron, by which the precipitation of Prussian blue is prevented, I found that not only was this the case, but also that if the Prussian blue were first precipitated, the addition of oxalate of ammonia caused it to re-dissolve. I therefore prepared a quantity of Prussian blue, which was well washed by decantation, and afterwards on a filter. A saturated solution of oxalate of ammonia dissolved a considerable quantity of it by the aid of heat, and yielded a clear greenish-blue solution. On being evaporated and set aside, numerous crystals of oxalato of ammonia were formed, but at the same time, at the *edges* of the solution, were minute dark blue crystals, of which a larger quantity was obtained by evaporating the mother liquor. They were quite soluble in water, and the addition of a drop of acid caused a copious precipitate of Prussian blue. The quantity I obtained altogether, however, was not large enough to enable me to experiment further upon it, but I am preparing the solution on a larger scale for that purpose.—I am, &c. A. Duncan, Jun.

Dalmarnock Print Works, Bridgeton, Glasgow.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Crystalline Form of Chloride of Potassium.—Mr. J. M. Maisch (*American Journal of Pharmacy*, vol. xxxii. p. 521) evaporated a perfectly neutral solution of chloride of potassium under the boiling point, and found that it crystallised in perfect cubes. As soon as these began to grow together he decanted the mother liquor and continued the evaporation at a summer heat. He now obtained four-sided prisms. The liquid drained from these was set aside, and the salt was then found to crystallise in irregular pyramids, composed of numerous small crystals. This experiment was repeated several times with precisely the same results, and in no case did the author obtain the octohedrons said to be deposited from alkaline solutions. He thinks, therefore that chloride of potassium must be regarded as dimorphous in two different systems.

Paramorphism of the Chlorides of Potassium and Ammonium.—A solution of chloride of ammonium, says the same author (*Ibid.* p. 522), when evaporated, yields feathery crystals which on close examination prove to be composed of small cubes. When the salt is sublimed, a fibrous mass is obtained which has a crystalline structure, though perfect crystals cannot be separated. The author thinks that the fibres must be considered as four-sided prisms. The prismatic chloride of potassium behaves in a somewhat similar manner. When heated, the prisms split chiefly lengthways, just as the fibrous crystals of chloride of ammonium do under the pestle. Both chlorides may, therefore, according to Mr. Maisch, be regarded as dimorphous, each crystallising in the regular and rectangular system, and isomorphous in both forms.

Crystallised Phosphates and Arseniates.—M. Debray (*Comptes-Rendus*, t. lii. p. 44) has continued the investigations, some account of which our readers will find in the CHEMICAL NEWS, Vol. i. p. 203. In addition to the facts there mentioned, the author now mentions that water exerts a decomposing action on phosphates which tend to change them into more basic compounds. Thus the phosphates of manganese $2MnO\ PO_5\ 7HO$ obtained at 70° C. when heated in water to 100° splits up into a phosphate with 3 equivalents of manganese and into a soluble phosphate $MnO\ PO_5\ 2HO$. Sometimes water removes phosphoric acid only from the salt. This happens with the phosphate of copper $3CuO\ PO_5\ 3HO$ and the arseniate of copper $3CuO\ As\ O_5\ 3HO$ which at a sufficiently high temperature yield crystallised Libethenite $4CuO\ PO_5\ HO$ and Olivenite $3CuO\ AsO_5HO$, the liquid in which they have crystallised becoming acid. Solutions of salts of copper assist these transformations. The phosphate of lime $2CaO\ PO_5\ HO$, the corresponding arseniate of lime and phosphate of lead $2PbO\ PO_5\ HO$ are not altered by water alone at any temperature; but in the presence of chloride of the same base, and at a temperature of 250° C. they are changed into chlorophosphates. In this way the author says he has obtained apatite and a corresponding arsenic and lead compound. Chlorophosphates cannot be procured with the phosphates of manganese and magnesia. In the original memoir the author describes a large number of phosphates, a list of which will be found in the place we have indicated.

II. ORGANIC CHEMISTRY.

Saccharine Matter in Acid Fruits.—At first, says M. Buignet (*Comptes-Rendus*, t. li. p. 894), this is cane-sugar, $C_{12}H_{11}O_{11}$. As the fruit ripens it changes into the "inverted" sugar, $C_{12}H_{12}O_{12}$. When quite ripe, some fruits (grapes, currants, and figs) have nothing but the inverted sugar, while others (apricots, peaches, apples, pears, &c.) contain a mixture of the two sugars, in various proportions. It is difficult to separate the two sugars, but the process suggested by Peligot answers the best for isolating the cane-sugar. It consists in forming a saccharate of lime, which is separated by boiling, and then decomposed by carbonic acid. The author could not find starch in the green fruits, and, therefore, does not regard it as the source of the sugar. He found, however, a peculiar principle, which formed a colourless compound with iodine. It seems allied to tannin by many of its properties, and can be separated from the juice of a green fruit by adding iodine until a precipitate is formed. This precipitate is collected and carefully washed, and then it can be shown that, under the influence of dilute acids, and at a proper temperature, sugar will be produced. In green bananas both starch and tannin are found, but as the fruit ripens the quantity of each diminishes, and when perfectly ripe not a trace of either is discoverable. The sugar found in their places is cane-sugar. A curious fact pointed out by the author is, that while cane-sugar is produced in bananas which ripen on the trees, it is the inverted sugar which is formed when they ripen after having been detached from the trees.

ANSWERS TO CORRESPONDENTS.

, In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

, All *Editorial Communications* are to be addressed to Mr. CROOKES, and *Advertisements* and *Business Communications* to the PUBLISHERS, GRIFFIN, BOHN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the CHEMICAL NEWS, containing a copious Index, is now ready, price 12*s.*, by post, 12*s.* 8*d.*, handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1*s.* 6*d.* Subscribers may have their copies bound for 2*s.* if sent to our Office, or, if accompanied by a cloth case, for 6*d.* A few copies of Vol. I. can still be had, price 10*s.* 6*d.*, by post 11*s.* 2*d.* Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Volume 1.—Add pure hydrate of baryta to your mixture of alcohol and nitric acid and shake together; all the free nitric acid will unite with the baryta, forming a nitrate insoluble in alcohol. After standing, the clear liquid can be decanted from the insoluble nitrate and hydrate of baryta.

Notice was given of the scarcity of Nos. 2 and 3 as also of the increase in price of the back numbers as early as possible. The above numbers can still be obtained bound up in Vol. 1.

J. B.—We now issue Monthly Parts bound in a wrapper. We also stitch the weekly numbers before issuing them. By these means the difficulty of the two loose leaves is obviated.

R. S.—We think that Fresenius' Quantitative and Qualitative Analysis are the best works on the subject in this language. The former work was recently reviewed in our columns.

A Young Manufacturing Chemist.—Heat about 8 grains of the finely pulverised mineral with six times its weight of a mixture of pure nitre and carbonate of soda in equal proportions. Heat in a porcelain crucible till the mass is red hot and in a state of tranquil fusion. When cool dissolve and determine the sulphuric acid in the solution by the usual means.

Wm. P.—We do not know at present, but will endeavour to give you an answer in a week or two.

P. M. H.—Received.

Books Received.—"On Food," by E. Lankester, M.D., F.R.S. London: R. Hardwicke, 192, Piccadilly. "Report on the Presence of Bisulphide of Carbon in Gas," by F. Versmann, F.C.S. London. "Proceedings of the American Pharmaceutical Association, 1860." Philadelphia: Merrihew and Thompson, Lodge Street. "How to Work with the Microscope," by Lionel S. Beale, M.B., F.R.S. London: John Churchill, New Burlington Street.

Received with enclosures.—*C. Weeks—A. Gow—J. Young—W. Butler—H. M'Leod—Royal College of Chemistry—N. Butler and Co.—F. A. Abel—C. Cochrane—Lack and Co.—Professor Hofmann—H. Matthews.*

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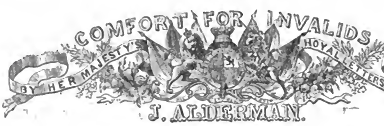
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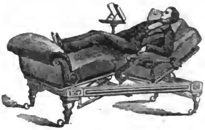
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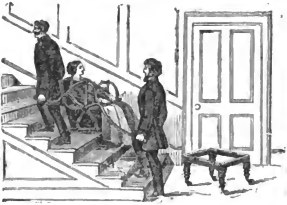
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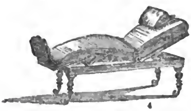
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THE CHEMICAL NEWS

VOL. III. No. 63.—February 16, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On Numerical Relations between Equivalents,[1] *by* M. CAREY LEA.

IN papers on this subject, published in the January and May Numbers of this Journal for 1860,[2] I endeavoured to show that a large number of so-called elements could be arranged in seriated groups, the members in each series differing from each other by a common quantity, in most cases the number 44, or one approximating to it. I endeavoured to show that not only were these groups natural groups, but that the chemical properties of the members of each group corresponded in many cases with their position in it. These observations seemed to favour the view at present gradually gaining ground, that those bodies which we have as yet failed to decompose, we have not proved to be elementary.

An interesting and elaborate paper, by Gustav Tschermak, published in the "Proceedings of the Academy of Science of Vienna," and extracted, in an abridged form, in Knepp's *Centralblatt*, July 4, 1860, on the subject of the law of volumes of liquid chemical compounds, affords a support to the views above expressed, from a new source. The author therein shows that many of the substances usually classed as elements comport themselves in the physical properties exhibited by their combinations as compound bodies, and that it is possible from these physical properties to determine, hypothetically, the number of "physical" or absolute atoms which he supposes to be contained in a chemical atom of such body or pseudo-element. He endeavours to show that it is possible to calculate the specific gravity of a liquid from its atomic weight and the number of simple (chemical) atoms in its compound molecule, as data, but that the results lead to the immediate inference that each chemical atom contains, with few exceptions, several physical atoms.

For particulars of his theory I must refer to the original paper, but some of his results are as follows:—

	Physical atoms to each chemical atom.[3]
O (O = 16)	2
S (S = 32)	4
F	2
Cl	4
N	2
P	4
As	5
Sb	6

If now we arrange the first six of these substances in parallel series we shall find

	Atomic weight.	Physical atoms.
Sulphur	32	4
Oxygen	16	2
Difference	16	2
Chlorine	35·5	4
Fluorine	19	2
Difference	16·5	2
Phosphorus	31	4
Nitrogen	14	2
Difference	17	2

Thus, a common difference in each case amounting to 16-17 corresponds with a difference of two of the physical atoms into which Tschermak divides the chemical atoms.

If now we put $O = 2o$, $Cl = 2cl$, $P = 2p$, the approximate difference between S and O, Cl and F, &c. $(16\text{-}17) = 2\Delta'$, the difference (48) between S and $Se = \Delta''$ and the difference (44-45) between the terms of the nitrogen series $= \Delta$, we can express the whole of three important series in terms of these six quantities, so that at one and the same time both the numerical value of the atomic weights and the number of Tschermak's physical atoms shall be correctly expressed.

	Symbols.	At. weights	Physical atoms.
A. Oxygen group:—			
Oxygen	o_2	16	2
Sulphur	$o_2\Delta'_2$	32	4
Selenium	$o_2\Delta'_2\Delta''$	80	5
Tellurium	$o_2\Delta'_2\Delta''_2$	128	6
B. Chlorine group:—			
Fluorine	fl_2	19	2
Chlorine	$fl_2\Delta'_2$	35·5	4
Bromine	$fl_2\Delta'_2\Delta$	80	5
Iodine	$fl_2\Delta'_2\Delta_2$	127	6
C. Nitrogen group:—			
Nitrogen	n_2	14	2
Phosphorus	$n_2\Delta'_2$	31	4
Arsenic	$n_2\Delta'_2\Delta$	75	5
Antimony	$n_2\Delta'_2\Delta_2$	120·3	6

In which table the number of radicals by which the chemical atom of each body is expressed corresponds with the number of Tschermak's physical atoms, while their numerical value is equal to the atomic weight of the body.

Thus, tellurium $o_2\Delta'_2\Delta''_2$ would have two each of three radicals, in all six, agreeing with the number of physical atoms assigned to it, while their value $2 \times 8 + 2 \times 8 + 2 \times 48 = 128$, at. wt. of tellurium.

These observations of Tschermak, taken in connection with the numerical relations which exist between atomic weights, give rise to very interesting results, and if the conclusions which he arrives at from his experiments should be confirmed, they cannot fail to exercise a very important influence on the progress of chemical science.

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[1] *American Journal of Science and Arts*, vol. xxx.

[2] See CHEMICAL NEWS, vol. i. p. 169.

[3] These numbers are taken from the table of mean numbers, p. 508 f *Centralblatt*, and are those subsequently used by the author for etermining the "physical atomic weights" $= \frac{m}{n}$.

On the Action of Oxygen on Aniline, by Professor C. F. Schönbein, *of Basle.*

Aniline, for so many reasons one of the most remarkable organic compounds, possesses a special interest, on account of the peculiar relation in which it stands to the three modifications of oxygen.

Action of Negative Oxygen.—A piece of white filtering-paper, moistened with colourless aniline, when introduced into strongly ozonised air, is coloured first brownish-red, then brown-red, rapidly passing into deep brown; and I may here remark that this rapid and powerful browning of the aniline is one of the most delicate reactions of ozone, and fully equals, in this respect, pyrogallic acid and hæmatoxyline. The result of this oxidising action of ⊖ on aniline is a solid, deep brown substance, which is scarcely soluble in water, but is readily taken up by aniline itself, as well as by alcohol and ether, whereby the fluids are coloured deep brown.

It is scarcely necessary to observe that aniline through which a stream of ozonised air is passed becomes rapidly coloured brown, in consequence of the production of the before-mentioned resinous body. It is, however, very probable that during the action of the ozone upon the aniline many other products of oxidation result which deserve investigation. For the information of anyone who may take this subject up, I may observe, that the brown, resinous body is destroyed by the prolonged action of ⊖, inasmuch as a piece of paper, moistened with aniline, if left long enough in an atmosphere powerfully ozonised, will be eventually bleached again, and then contains a substance which powerfully reddens litmus-paper, and which is probably oxalic acid.

Combined ⊖ acts upon aniline like free ⊖; for we find that this substance is resinified at ordinary temperatures by all the ozonides, just as it becomes brown by the action of free ⊖, the ozonides, of course, losing their ⊖ at the same time. This may be conveniently illustrated by placing in watch-glasses small quantities of the oxides of gold and silver, the binoxides of lead, manganese, &c., and pouring over them a few drops of aniline, which immediately becomes brown. If a solution of permanganic acid or permanganate of potash be shaken up with aniline, it gives immediately a deep brown precipitate, consisting of oxide of manganese and the brown resin, which latter may be extracted by alcohol.

NO_4—according to my view, $NO_2 + 2\ominus$—is remarkable among the ozonides for the energy with which it acts upon aniline, and may be compared in this particular with ozone itself. If so small a quantity of NO_4 vapour as to be scarcely perceptible either to the sight or smell is introduced into a bottle, a piece of paper moistened with aniline will be coloured precisely as if ozone itself were present: and if a quantity sufficient to slightly colour the vessel be present, the paper becomes immediately deep brown, with the production of white vapours, which surround the paper.

It is well known that ⊖ is able to combine as such with Guaiacum resin, and to form an organic ozonide, which is coloured deep blue, and is soluble in alcohol. Aniline greedily abstracts ⊖ from this as from the inorganic ozonides, so that tincture of Guaiacum, of the deepest blue, becomes instantly yellowish-brown when mixed with aniline.

Action of Positive Oxygen.—While, as I have shown, both free and combined ⊖ powerfully oxidises aniline, all the compounds containing ⊕ are quite inactive. Thus, for instance, $HO + \oplus$ may remain for days in contact with aniline without either the one becoming decomposed or the other resinified. Neither have $KO + 2\oplus$, $N_2O_2 + \oplus$, or $BaO + \oplus$ any action upon aniline, inasmuch as they leave it quite uncoloured. The so-called ozonised oil of turpentine contains its disposable oxygen in the ⊕ state, and is, therefore, an organic antozonide. As is the case with $HO + \oplus$, &c., so is this turpentine oil chemically inactive towards aniline, however much it may be laden with ⊕. The two fluids, when mixed, remain quite colourless, and no ⊕ is taken up by the aniline. These facts seem to me to prove beyond doubt, that positive oxygen behaves towards aniline precisely as it does towards pyrogallic acid,—that is, ⊕ produces no oxidising effect upon this substance, otherwise so greedy of oxygen.

Action of Neutral Oxygen.—Neutral oxygen acts upon aniline apparently as such, as it is well known that aniline in contact with pure, or atmospheric oxygen becomes coloured and resinified, which is, without doubt, an oxidising action. I find, however, that, *cæteris paribus*, this change occurs more rapidly under the influence of light than in darkness. 50 grammes of colourless aniline, shaken up in a pint bottle with atmospheric oxygen, appeared to be coloured a deep brown, after standing for four days in diffused light, while aniline, under otherwise perfectly similar circumstances, but kept in the dark, appeared to be very little changed.

According to my observations, the resinification of aniline and the charging of oil of turpentine with ⊕, takes place in the presence of light very much faster than in the dark; in fact, illuminated oxygen generally acts as ⊖, and produces oxidations which, in darkness, either do not take place at all, or, at most, very slowly. I am, therefore, inclined to assume that oxygen, as such, has no oxidising influence upon aniline.

From these investigations, it is clear that facts are rapidly accumulating which tend to show that the chemical relation of oxygen to other bodies is intimately connected with its allotropic state; and I have no doubt that the number of these facts will daily increase. We must, therefore, be prepared to modify, or entirely relinquish, our previous conceptions of "chemical affinity." I often feel that it is impossible to obtain a clear notion of, for example, the words, "The affinity of oxygen for phosphorus," &c., if we wish to express anything more than the mere fact. I must confess that the prime cause of chemical combination is still to me a deep mystery.—*Journal fur Prakt. Chemie*, bd. lxxxi. p. 5.

TECHNICAL CHEMISTRY.

On Coal Gas, by the Rev. W. R. Bowditch.

(Continued from page 84.)

Coal-gas, quite free from sulphide of hydrogen, when passed through hot lime, blackens lead-paper, showing that masked and hitherto irremovable compounds have been so altered as to be easily removeable. The lime does not take up sulphide of hydrogen, but becomes gradually, yet very slowly, darkened by the deposition of tar and carbon from vapour of bisulphide of carbon. The reaction with previously dried slaked lime commences at 108° F., and continues through the whole range of temperature up to redness. At a red heat the sulphur of the bisulphuret of carbon and other sulphur compounds unites with the lime and forms sulphide of calcium. Practically very high temperatures are useless, as the hydrocarbons of gas begin to be decomposed about the melting-point of lead, and to deposit their carbon upon the hot lime. Fortunately injurious temperatures are not required. I have frequently freed gas from every trace of sulphur, so that upon combustion no sulphurous acid was generated, by employing lime so heated as not to deposit any carbon, and removing the sulphuretted hydrogen evolved in the hot tube by ordinary hydrate of lime.

The same gas passed through hot clay gradually darkens the clay by forming sulphide of iron, and, when the blackness has reached the end of the tube containing the clay, lead-paper is blackened by the passing gas. The clay treated with an acid evolves sulphide of hydrogen. Carbonic acid is evolved in both cases. It is thus proved that bisulphide of carbon, in the presence of hydrogen passing over hot hydrate of lime, is decomposed, and that its sulphur becomes united to hydrogen. Coal-gas always contains a considerable quantity of hydrogen, so that, if it contain vapour of bisulphide of carbon, the process I have the honour to describe will effect its removal. The same process will break up the impure naphthalin compound and convert its sulphur into sulphide of hydrogen; and the employment of clay in the ordinary purifiers, before the gas passes through the hot ones, will so arrange the elements of certain other sulphur compounds as to enable the manufacturer to remove their sulphur as sulphide of hydrogen. Sulphocyanide of ammonium is decomposed by the heated lime, and its sulphur is liberated as sulphide of hydrogen. The only requisite for complete success was that no injury should be done to the light-giving materials of gas while removing the impurities. I have passed the principal illuminating constituents of coal-gas through the hot lime and clay, and find that they are not injured. The temperature which suffices for purification is not high enough for injury. The photometer shows that coal-gas is not injured.

The quantity of tar in gas as supplied to consumers, and the evil of its presence as a source of sulphur, are not considered as, I think, they deserve to be. It is exceedingly rare to find gas free from tar, and I never yet met with tar which did not contain both nitrogen and sulphur. Part of this tar is combined with ammonia in some manner, and may be obtained by passing gas through a bottle containing pebbles moistened with hydrochloric acid. Part is united to naphthalin, as I have already mentioned; part is united to benzole vapour, and part to other hydrocarbon vapours, such as paraffin, if two instances within my own knowledge be sufficient to justify a statement in reference to gas in general. In one instance I passed gas through a metal vessel filled with a number of wire-gauze diaphragms and kept below 32° F. Some cakes of solid paraffin were found floating upon the water which had been placed in the vessel before commencing the experiment, and a mixture of tarry oils which had deposited the paraffin. In another instance, an old gas-holder was about to be replaced by a new one, and on the water of the tank in which the old gas-holder had worked, there was found upwards of a thousand gallons of a dark-coloured fluid. All but two carboys were sold to a tar distiller. These two carboys were left exposed to the air without corks for some time, and when the manager of the gas-works went to get me some of the fluid for examination, he found that the whole contents had evaporated. I had previously, however, obtained about half-an-ounce of the mixture. It contained paraffin, naphthalin, and the oils which accompany paraffin. Nearly a fifth of its weight of solid pitch was obtained by distilling off the hydrocarbons. A quantity of sulphide of hydrogen and ammonia were evolved during the distillation, and some of the most stinking compounds I ever met with produced from coal. From these two instances it is clear that some, or perhaps all, of the volatile hydrocarbons in gas possess the power of upholding tar with them in their vapours; and it is proved that this tar is no inconsiderable source of the sulphurous acid produced by the combustion of gas as at present purified. I have obtained tar containing sulphur from every specimen of commercial benzole I have examined; and as this will evaporate at common temperatures without leaving a residue, we are justified in the presumption that tar thus united to benzole exists in gas.

The best method of showing the tar in gas is to pass it through or over well-purified coal-oil, and subsequently through a good condensing arrangement. I have known colourless coal-oil become of a dark mahogany colour, and have separated sulphuretted hydrogen, ammonia, and solid pitch by distillation.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

Lecture I.

Chemical Affinity—Mode of its Action—Distinction from Other Powers of Matter.

I cannot commence the present course of lectures without a word of apology to a considerable proportion of my hearers on account of the very elementary character which I shall find it necessary to give to the first lectures of the course. It seems to me that one of the objects of such a course as this is to afford to beginners in chemistry an opportunity of making themselves acquainted with the fundamental and elementary principles of that science, so that they may be able, without difficulty and with pleasure to themselves, to listen to the discourses upon the higher phenomena and recent discoveries of science which peculiarly characterise this Institution. I trust, therefore, I may claim the kind indulgence of a considerable proportion of my more instructed audience, whilst, in the first few lectures, I treat of those very elementary matters with which some of you are quite as familiar as myself.

The material objects by which we are surrounded are constantly undergoing changes of a more or less striking character. Thus, water assumes, when it is exposed to the cold of winter, the condition of ice, and, when submitted to a higher temperature, becomes converted into steam. Frequently the blue sky becomes suddenly darkened by masses of cloud, from which issue thunder and lightning. The farmer and the gardener cast into the earth seeds, which spring up and gradually develope themselves into the stems, branches, and leaves of plants. In the autumn these leaves fall to the ground, and there they undergo decay and gradually disappear altogether; whilst the iron nails by which some of those plants were trained to the wall have become covered with a red material which we all know as the rust of iron. Now, all these changes are produced upon matter by certain agencies which we term *forces*. Thus, the change in the condition of water is supposed to arise from the action of two antagonistic forces, namely, *heat* and *cohesion*. The phenomena of the thunder-cloud are due to *electricity*. The springing up of seeds into the leaves and branches of plants is due to an aggregation of forces, to which the very inappropriate term of "*the vital force*" has been applied; whilst the decay of the leaves after they have fallen to the ground by the attraction of *gravity*, and the rusting of the iron nails are due to the *chemical force*. This is the force we shall have to consider, it is closely associated with the other great powers of matter; and it is not always easy to distinguish between the operations of the chemical force and those of the other powers. Both chemistry and physics —the latter being the science which treats of the remaining forces of matter,—deal with the changes which bodies undergo, with especial reference to the *causes* of those changes, and, consequently, these two branches of science are very closely associated together.

The phenomena of the chemical force are, however, of such a character that, with a little preliminary explanation, we shall be able to distinguish them from those of the other forces.

In the first instance, it is absolutely necessary for the operation of the chemical force that we should have two different kinds of matter. No two portions of the same matter are capable of causing the manifestation of this force. Iron brought into contact with iron manifests no chemical action. Sulphur brought into contact with sulphur likewise produces no chemical effect. But if we bring together iron and sulphur, these two bodies, under favourable circumstances, are capable of exerting chemical force, or "affinity" as we term it. Now, let us examine some of the peculiarities of this force, and especially some of the marks by which it is distinguished from the other powers of matter. In the first place, with regard to the chemical force, allow me to refer you to these memoranda [pointing to a diagram], which may be regarded as the heads of my lecture. In the first place, the chemical force always changes the properties of the bodies submitted to its influence. We have always a change of a more or less striking nature,—a permanent change, so to speak, impressed upon matter upon which this force acts. For instance, we have here the bright metal, iron, and here we have enclosed in this jar the element oxygen, one of the constituents of our atmosphere. These two substances are capable of uniting chemically, but in doing so they undergo such a complete change that it would be impossible for any one, without knowing the fact, to recognise either the one or the other in the compound—rust, or oxide of iron,—which is formed of these two elements. In the same way this bright liquid metal, mercury, unites with oxygen, and produces this beautiful solid red binoxide of mercury, which is as unlike the substances of which it is composed as possible, the one being liquid, and the other an invisible gas.

This change of properties never takes place when any force other than the chemical force is exerted upon matter. If, for instance, I expose a piece of this platinum wire to the influence of heat, I get a phenomenon which is termed incandescence; the platinum wire becomes heated to redness, and may be maintained at this temperature for any length of time. At a higher temperature I could fuse it and run it into a single globule, or mass, but when cooled it possesses its original properties, quite unchanged. It possesses the same metallic properties; and, even if we were to fuse it to a globular mass, we might draw it again into wire of the same character as this. Here is another metal, not quite so brilliant on the surface, perhaps, as the platinum wire, and I will expose this to the same influence. You see this wire presents perfectly different phenomena from those we observed in the platinum wire. We have brilliant combustion, and we have the wire converted into a friable material here, which it is almost impossible to collect,—into a substance which is termed magnesia; we have converted, in fact, our bright, silver-like metal into magnesia. In the latter case we have had an exercise of the chemical force, which changes the properties; in the former, only heat was exerted, which left the matter with its original properties unimpaired. Now, no mere mechanical mixture of two portions of matter can produce a body possessing properties completely different from the two substances so mixed. I have here, for instance, a mixture of sulphur and iron in a state of fine division. The mixture is very intimate indeed, although, when examined closely, we may certainly perceive the particles of the two different substances; but unless we closely examine the mixture we do not readily detect the separate constituents of which it is composed. This mixture, however, possesses properties intermediate between these two substances; or, rather, I should say, it possesses the properties of both constituents. We have still in this mixture sulphur, as such, and iron, as such; and we could manifest their properties by a little patience, and, by a magnifying glass, we could take out the particles of iron from the particles of sulphur with which they are mixed. We can do this, however, much more effectually by means of a magnet,—for instance, by means of this powerful magnet, which will, I hope, enable us to separate these two elements from each other. [The Lecturer then spread a portion of the mixture upon paper, and placed it over the poles of a powerful electro-magnet.]

We are now about to submit the mixture to the action of magnetism; and you see how the little particles of iron rush away from the sulphur, and gather over the poles of the magnet. We can thus separate the iron better than by other means. Now, such a separation as this, either by taking up the particles under the microscope, or by submitting the mixture to the action of the magnet, cannot be effected upon a true chemical compound. Here I have a compound of these same elements, *united* in the same proportion in which they were there *mixed*. Here they are chemically combined—there mechanically mixed. Now, we will see what the difference will be in the deportment of these two substances over the poles of the magnet. You see we have not the slightest tendency to separation or accumulation over the poles of the magnet. These two substances are so completely mingled that this magnetic attraction is utterly unable to separate them from each other. Further, we can very readily ascertain the difference in properties, with regard to the magnet, between the mixture and the chemical compound. Here we have two equal weights in these two scale-pans, the one on this hand [pointing to the right], consisting of the mixture, the weight in the other pan consisting of the chemical combination of the sulphur and the iron. Let us try now whether these bodies—you will recollect that in both scale-pans we have exactly the same weight of iron, and also the same weight of sulphur in both cases—let us now try whether there is any difference with regard to the magnetic attraction which this large piece of apparatus is capable of exerting upon these two portions of matter. [The Lecturer then attached the scale-pan, containing the mechanical mixture of the sulphur and iron to the end of the lever.] You see at present, before the contact of the magnet with the battery is made, this scale pan exactly balances the long lever at the other side of the point of suspension. Now we will put upon the lever a weight, to ascertain the amount of attracting force. [The contact was then established, and the scale pan was attracted down to the magnet.] There is now an amount of attraction exerted sufficient to sustain this considerable weight upon the lever. I will now introduce our mechanical mixture of the same elements in exactly the same proportions as they exist in the chemical compound. [The pan containing the mechanical mixture was then detached, and the one containing the chemical compound was suspended from the lever.] As we have equal weights here, you see this other portion of matter is capable of exactly balancing that rod. And now when the magnetic force is applied, you see we have no attraction here; it has all disappeared, and it would be useless to suspend the weight on the other side of the point of suspension. So you see where chemical action has been exerted upon the two different bodies, the magnetic attraction has undergone a change; it is now attracted with a scarcely appreciable amount of force when the chemical union has taken place.

This change of properties manifests itself also in a variety of other ways. Sometimes it is seen in a change of state of aggregation; when for instance, one form of matter is converted into another form. Sometimes liquids are converted into solids when they are brought under the influence of this particular force. Here I have two perfectly limpid liquids, which I dare say on mixture will assume very nearly, if not quite, a solid form. [The liquids were then poured together.] You see the mass is gradually thickening. Now it has so far solidified, that I dare say we may invert the vessel without any part of the mixture falling out. [The vessel was inverted with the result anticipated.] Even under the influence of this peculiar force, gaseous matter itself, colourless, invisible gas, may be condensed into a solid form. Here are two colourless and invisible gases, which we may, in like manner, bring into contact; and we know that between these two gases, there exists a certain amount of chemical affinity, which will enable them instantly to unite, and to produce a white solid body which will gradually subside into the lower of the two vessels, constituting a layer of solid matter very much resembling snow. The two gases are, in fact, hydrochloric acid and ammonia, and produce the well-known solid chloride of ammonium.

This change of properties is also sometimes characterised by an alteration in the colour of bodies. Here, for instance, are two bodies, between which the chemical force is capable of acting. I pour them together, and you see we get, at first, a yellowish body, which afterwards changes into salmon colour, and will, I dare say, ultimately change to scarlet. [The two colourless solutions were then poured into one vessel.] These two solutions were at first colourless, and you see we have a very striking colour produced.

Change of temperature is also a very frequent—indeed, I may say, a constant—accompaniment of the action of the chemical force. There is no case of chemical action in which a change in the temperature of the particles does not appear. Generally the change is in the direction of elevation of temperature. I will endeavour to render this visible to you by a little apparatus here, which I shall, perhaps, have frequently to use in these lectures. I will, therefore, describe it now. It is the thermo-electric pile, which shows minute differences of temperature in such a manner as to make them visible to a number of persons at once. This is the instrument to which I refer—it consists of bars of antimony and bismuth; and when heat is applied to the ends of these bars it causes the needle of the galvanometer to deflect. If I breathe upon one end of this pile, or expose it to any sort of heat, I at once get a deflection of that needle; and if you notice the direction in which the deflection of that needle takes place, it will enable us to appreciate with the same instrument a difference of temperature in the opposite direction,—I mean when we have cold produced. When we have got this needle again to zero, I will place in front of this pile a mass of ice; and I think we shall find that the needle will then deviate from its original position in a direction opposite to that which it took in the first case. As the difference in the temperature, however, here is, perhaps, scarcely so great as in the former case, we shall, perhaps, not get so strong a manifestation, and it is, therefore, important that the needle should be brought as nearly as possible to zero before we expose this end of the pile to the ice. [A piece of ice was then applied to the end of the pile, but the deflection was not very marked.] I think we shall get our result better if we bring the ice nearer to the little bars of bismuth and antimony. [The ice was placed nearer.] You see, now, that is the opposite direction to which the needle moved before. If a heated substance is placed at this end of the pile the needle then moves in one direction. When a cold substance is placed there the needle then moves in the other direction.

Now, we will first try to get the effect of elevation of temperature by the action of this chemical force. Here I have two liquids which possess a very powerful affinity for each other—namely, concentrated sulphuric acid and water. I will mix them together, and I believe the vapour arising from the mixture, which is very considerably heated, is almost sufficient to show elevation of temperature. But we will not be satisfied with that. I will put the mixture in front of the pile, as I want the experiment for comparison with the subsequent one. Here, then, we have a source of heat, for we have really obtained an elevation of temperature, and you see the needle obeys the impulse and travels round in this direction, indicating that the mass placed in front of the pile here has a temperature higher than that of the surrounding air. Now, there are some circumstances under which the opposite effect is apparently produced. It is, I believe, only apparently so; but it appears to a very considerable extent in some cases. If I mix this powdered salt—the sulphate of soda—and hydrochloric acid, I have great chemical action exerted between these two substances; in fact, the sulphate of soda becomes to a great extent broken up in its chemical constitution, new compounds are formed, and a very marked depression of temperature takes place in the mixture. The mixture has now become very much colder,—much colder, in fact, than ice,—and the deflection of the needle towards the cold side will be considerably more powerful than it was in the case of the block of ice. Let me try it. We will place our mixture, which we expect to be reduced in temperature, in the same position as that previously occupied by the other vessel, and let us see if we get any result. We must remember, however, here, that our mixture is placed as the block of ice was at first,—at a considerable distance from the extremity of the pile. It is just possible we may have to place it nearer. [The galvanometer indicated a reduction of temperature.] No; I see there is no occasion for that; the effect is visible even at this distance. The red end of the needle is travelling towards you, indicating a depression of temperature in the mixture, and if we were to remove this and bring it nearer to the bars of bismuth and antimony, that effect would be greatly intensified; but it is visible enough now. In these cases, however, where a depression of temperature results, apparently, from the action of chemical affinity, I do not believe it is so in reality. I believe, in this case even, we have some elevation of temperature, and that in every case where the chemical action ensues we have heat evolved; but by the peculiar chemical action which ensued in this case a certain quantity of matter is liquefied—a certain quantity of solid water, for instance, in this salt becomes liquefied; the heat is rendered, as it is called, latent, causing a depression of temperature in the whole mass of matter; I believe in all these exceptional cases heat is evolved, and it is only because, under some circumstances, there is a greater amount of heat rendered latent than evolved that we have this apparent depression of temperature.

A change in the taste and odour of substances is another result of the action of the chemical force. This, however, we may pass over.

Change in the medicinal properties of bodies is also a very frequent occurrence when chemical action takes place. For instance, chlorine is an exceedingly poisonous and suffocating gas, so that two or three cubic inches of it taken into the lungs would produce almost instant death; it is one of the most poisonous substances there is, when taken into the lungs in a concentrated form. Sodium is also a substance which would produce the most injurious effect if taken internally. But these two energetic substances produce, by combination, common salt, one of the most innocuous bodies with which we are acquainted. Arsenic, when in chemical combination with iron, forms an innocuous compound, which may be administered to animals with impunity. Also the elements of our atmosphere form, by chemical combination, a substance possessing the most corrosive qualities.

Another peculiarity of chemical affinity is its incapability of acting at a distance. Most other forces are capable of exhibiting their effects and exerting their influence through considerable portions of space. Here I have a little collodion balloon. If I just rub it with my hand I render it electrical,—that is, I excite a certain amount of electrical force. It will follow my hand when held at a short distance from it. Thus the electricity acts through space. You are warmed by standing in front of a fire, although the intermediate air may be below the freezing point. A magnet also can exert its power through air or space filled up with glass, or through other solid substances; but the chemical force is quite incapable of acting in this way. It cannot act through an appreciable space. Let us put this to as severe a test as, perhaps, we possibly could do. I have here a material which is capable of exerting a very powerful chemical action upon this liquid with which I have moistened the glass rod. I will now endeavour to bring the two substances—the liquid upon the glass rod and the powder upon the plate—as near as possible without touching. I have a drop upon the rod which might possibly fall upon the powder. [Shakes off the superfluous drop, and replaces the rod over the powder.] Now it is so near that the space between is hardly visible. I can hardly see it myself. It might remain in this position for days or years; it is only by coming into contact that the force is capable of being exerted between them, and the energy of it is at once rendered evident. [The Lecturer then allowed the moistened rod to touch the powder, which at once ignited.]

In addition to actual contact between bodies, which we saw to be necessary even in the case of very powerful affinities, there is also required a certain amount of mobility between the particles. This is another peculiarity of the chemical force,—in addition to contact it requires mobility of particles. As a necessary consequence of this, it follows that solid bodies cannot act upon each other chemically; and although there may be a few apparent exceptions to this general law, yet they are, I believe, most of them *only apparent*, and if closely looked into, will be found not to be exceptions, but to obey this law.

I have here two solid substances which possess a very powerful affinity for each other, but which do not act by being simply brought into contact,—not even by being reduced to a very impalpable powder and then thoroughly mixed together: they may be mixed ever so intimately, but still they do not unite. They are sulphur and iron. They do not unite now, and they would never unite even if kept together for any length of time; but it is possible by causing fusion, or by liquefying one of the substances to induce chemical action. I am now going to show you an experiment which is one of the apparent exceptions to this law. I have two solid bodies here, and I shall show you that they do act chemically upon each other. [The two substances were contained in a mortar, and the Lecturer exploded a portion of them by pressure with the pestle.] By means of the pressure which I apply to them I probably liquefy minute portions of the sulphur, and the two bodies then combine with very great readiness. The two substances I have in the mortar are chlorate of potash and sulphur; and there can be no doubt that where the pressure is applied, liquefaction of the sulphur takes place, and that the sulphur then acts upon the chlorate of potash, and the heat then caused is sufficient to liquefy the particles around.

In consequence of this necessity for mobility of particles, the liquid and the gaseous conditions of matter are the most favourable for the exertion of chemical affinity; because there we have complete mobility of particles. The particles are free to move in any direction, and that intimate union which is so essential for the exertion of the chemical force can with readiness take place between them. It is not very essential that both forms of matter should be in the liquid or gaseous state; it is very frequently quite sufficient that one should be in this state. For instance, I have here dilute hydrochloric acid which can act upon marble; the one is solid, and the other liquid. You see if I drop some of this upon the marble, the marble is acted upon and carbonic acid gas is evolved. Likewise a gas may act upon a solid. You see if I introduce into this jar of chlorine some powdered antimony, it immediately takes fire when it comes in contact with the gas, thus showing that one of the bodies may be in the gaseous condition, the other remaining solid.

It is perhaps scarcely necessary for me to remark, therefore, that if a solid and a gas are capable of uniting by virtue of the mobility of the gas alone, two gaseous bodies which possess an affinity for each other can similarly be made to combine. Here we have two gases by which we may exemplify this. [The gases were contained in separate jars, one of which the Lecturer proceeded to invert over the other, bringing the mouths of the jars together.] We will bring the gases in contact. You see they immediately combine energetically, producing a dense, reddish brown vapour, which is a compound of two substances, nitrogen and oxygen. It has very generally been supposed that the gaseous condition is inimical to chemical combination; but wherever we have indubitable evidence of two substances having an affinity for each other, we find their affinities are exerted in the gaseous state. It has been said of oxygen and hydrogen, that the distance between their particles, when they are in the gaseous state, is an obstacle to the exertion of their affinities for each other; but if so, when we expand their particles still more, they ought then to exhibit a still less tendency to combine. But this is not the case. If we take, for instance, a mixture of these two gases, and plunge into it a heated substance, they will readily unite. I had intended to plunge into this mixture [exhibiting a soda-water bottle containing a mixture of hydrogen and oxygen] a heated bar of iron, but it is not ready. A flame will do, or this incandescent taper. The heat of this will expand the gases still more. [The incandescent film was introduced into the mouth of the bottle, and the gases united with a loud explosion.] Therefore it is not because the particles were in the gaseous state that they did not unite at first, before the heat was applied; because here we separate the particles still farther, and they combine more readily.

Now, whenever we have to combine two bodies only one of which possesses the necessary mobility of particles, it is very necessary that the other one should be in the most minute state of division; the finer the division, the more ready will the chemical action be. You all know carbon in its several different forms of diamond, of graphite, of coke, and of wood charcoal. Now, it is very difficult to burn a diamond in the air. If you remove the source of heat the combustion immediately ceases. In like manner the combustion of graphite is very difficult. Coke, although capable of burning, requires a powerful draught to keep up the heat. Charcoal will more readily ignite, and if we put several pieces together, it will continue to burn until the whole of the particles are consumed; but even that will not burn so readily as tinder, which is another form of the carbon. This tinder, by which we used in former days to obtain our lights, before the invention of lucifer matches, will ignite by the smallest spark, and continue to burn until all is consumed. You see in the diamond we have a very dense form of carbon; in the graphite it is in a less dense form; in coke the carbon is still less dense; in the charcoal the particles are still further apart; and here, as tinder, we have it still more minute. But here is some charcoal which has been prepared in a peculiar manner, and which is capable of exhibiting its affinity for oxygen at the ordinary temperature, without any application of fire at all. If I shake it from this tube upon the paper it ignites upon coming in contact with the air, and continues its combustion after.

Of the two conditions in which particles of matter are mobile the liquid is undoubtedly best; and as this condition is more favourable, one or both of the two substances between which we want this chemical force to act must be usually converted into this condition. This is done in two distinct ways—by what chemists call the *humid*, or wet way, that is, by solution, and by the *hot*, or dry way, that is, by application of heat. I may give you a familiar example of the wet mode in the case of the common effervescent powder, tartaric acid and carbonate of soda. They may be kept together for any length of time in a dry state without any change; but, when dissolved in water, the chemical action takes place. Here, again, we have an instance of liquefaction produced in the dry, or hot way. Here is a powder which contains constituents possessing a powerful affinity for each other, but which affinity cannot be exerted as long as the particles remain in the solid condition. We will apply heat to it, and as soon as the liquid condition has been induced upon the matter a violent deflagration—perhaps an explosion—will take place. The liquid condition is that generally employed for the production of the chemical action between those substances which do not combine in their natural form of aggregation.

The fourth, and a far more distinguishing characteristic of this force, is that it is only exerted between fixed and invariable quantities of matter. We have it here stated in our diagram. We can cause heat to act upon any weight of matter, or upon any different forms of the same matter; but in the chemical force, only certain fixed quantities take part in the reaction. In consequence of this, it follows that every chemical compound possesses a uniform and invariable composition; that the same body always possesses the same elements, and that the same substances unite in precisely the same proportions. Thus, if you examine water,—we will suppose, water that has been condensed from the form of steam, water that has been formed by the melting of ice, water which falls in the form of rain, and, finally, water produced by the combustion of hydrogen with oxygen, you will find that all these different samples possess the same composition,—1 of hydrogen, 8 of oxygen. If you take, too, common salt, taken either from the brine springs of Germany, the mines of Poland, or the salt beds of Cheshire, it always contains about 23 parts of sodium and 35½ of chlorine in one hundred parts of common salt. This fact constitutes the first great law of chemical combination which is expressed here [pointing to the diagram], that the composition and properties of bodies are fixed and invariable.

The second law is also important. The relative quantities in which bodies unite may be expressed by proportional numbers. Those numbers which have been ascertained for nearly all the elementary bodies—for all the simple forms of matter out of which material nature is constituted so far as it has been investigated,—are all recorded upon these three tables [referring to the diagrams containing a list of the chemical elements] opposite to the names of the elements themselves; and we shall probably have these tables suspended here during the whole course, so that I need only refer to the atomic weight—the combining proportion—of one or two of the elements in passing. These numbers mean that carbon, for instance, when it unites with another element, does so in the proportion of six parts by weight of carbon or some multiple of that weight. If it unites with oxygen, which is 8, we shall find that such a compound consists of 6 parts by weight of carbon and 8 parts by weight of oxygen, and so with the other elements.

Some elements are capable of combining in more than one proportion, and when this is the case the relation between the proportions is always in a very simple one; or, as it is expressed on the diagram,—"When one body, A, unites with another body, B, in two or more proportions, the quantities of the latter united with the same quantity of the former bear to each other a very simple ratio." For instance, let me represent the atoms to you with these blocks. This atom of hydrogen which has the weight 1, is capable of combining with an atom of oxygen which weighs 8, forming water. It does, however, unite with an additional atom of oxygen, which always unites in the proportion of 8. The two atoms of oxygen make 16, forming, with the atom of hydrogen, binoxide of hydrogen, or oxy-water. The atom of hydrogen can also unite with three atoms of oxygen. The weight of the three atoms of oxygen would be 24. We have no intermediate links between these compounds. It must be always 1 to 8, or 1 to 16, or 1 of hydrogen to 24 parts of oxygen. We have no compounds between those numbers. You will see, however, that it is possible that two combining proportions of one element may unite with three of another; or three of one with four of another, forming proportions which are more complex, but still simple if you remember this principle.

The remaining law is a simple deduction from the first three—namely,—that the equivalent of a compound is the sum of the equivalents of its constituents. If any of these compounds is capable of combining with any other substance, it does so in the proportion of the sum of the equivalents of the elements of which it is composed.

Finally—chemical affinity affords the most perfect means of storing up force. In no other condition can force be so conserved for any length of time. What is our present source of mechanical power? Is it not a store of force residing in our coal, which is nothing more than a mass of carbon and hydrogen, which was previously combined with oxygen, until the solar heat and light of past ages effected a divorce, and rendered them magazines of force, from which we now draw our heat? Setting out with the chemical force stored up in zinc—and in the coal, too, for without that we could not obtain the zinc from the ore,—we have Grove's beautiful arrangement of the voltaic battery. We will first of all evoke a current of electricity, which is capable of passing through these conducting wires. We will pass the current through a portion of smaller wire here, so that we shall be able to get from this electricity a certain amount of heat. We can then pass it through water, and by that means decompose the water. It may afterwards pass around this magnet, and produce there a magnetic effect of very great intensity. You see now our current is passing. The chemical force which was previously stored up in the zinc is passing as a current of electricity through these wires. Here are two gases, which have been torn asunder from water—oxygen and hydrogen—by means of this force. [After a few seconds the contact of the wires with the battery failed.] I had intended, if our contact had lasted longer, to have caused this coil of platinum wire which became red-hot, to act upon the thermo-electric pile. We should then have had a portion of heat, causing again a current of electricity upon these wires, and producing there a magnetic effect upon that thermo-multiplier,—that galvanometer—and which would have been rendered visible as a mechanical effect. You see, then, when we have this force stored up in coal, or zinc, or iron, how readily we can apply it to the production of any of these other powers which we may find it necessary to evoke.

GENERAL MONTHLY MEETING.

Monday, February 4, 1861.

W. R. Grove, *Esq., M.A., Q.C., F.R.S., Vice-President, in the Chair.*

Nicholas P. Leader, Esq., was elected a Member of the Royal Institution.

Sir Henry Holland, Bart., M.D., F.R.S., was elected a Manager of the Royal Institution, in the room of Sir Charles Fellows, deceased.

The presents received since the last meeting were laid on the table, and the thanks of the members returned for the same. Twenty-two persons were proposed for election at the next meeting.

SOCIETY OF ARTS.

Wednesday, January 30, 1861.

W. H. Bodkin, *Esq., Assistant Judge for Middlesex, Vice-President, in the Chair.*

(Continued from page 94.)

Butter, too, is one of those articles which affords a striking proof of the extent to which adulteration is carried in this country, both the "salt" and "fresh" varieties being sophisticated very largely. According to Hassall, the amount of water in the fresh butters examined by the "*Lancet* Analytical Commission," varied from 4·18 to 15·43 per cent., while the quantity of salt averaged from 0·30 to 2·91 per cent. In the salt butters, the extremes of water were 8·48 and 28·60 per cent., and those of salt, 1·53 to 8·24 per cent. My own more recent results show a wider difference, and I have found as much as 49 per cent. of water in some samples of salt butter. From a little pamphlet on adulteration, by Mr. John Postgate, of Birmingham, I learn that, in 1857, butter was sold in Liverpool to the poor, which contained only 48 per cent. of that article, 24 pounds of a mucilage (probably a kind of lichen), and 28 pounds of water making up the remainder of this delightful compound. Butter is also adulterated with flour and other substances, and one instance has come under my notice in which the silicate of soda, or soluble glass, was the adulterant employed. Butter should be examined by melting a weighed quantity, in a graduated tube or jar, when the relative proportions of fatty matter and water can be roughly estimated.

Lard is adulterated in much the same manner as butter, and is itself sometimes used as an adulterant of butter.

Cheese is not generally adulterated, although instances have come to my knowledge. Some very fine samples of this article, and also of butter, have been kindly contributed by Mr. Osborne, of Ludgate Hill. I recommend these specimens to your notice.

Infused beverages must be our next consideration, commencing with tea as the most important. Here, I feel that I am treading upon rather delicate ground; after the able paper read in this room last week by Mr. Leonard Wray, it would be presumption in me to say very much upon the subject. I have to thank Messrs. Phillips for a small but interesting collection of genuine teas,—I mean genuine as imported; for of the 21 samples here exhibited, from India, China, Java, and Japan, three have been coloured at Canton. Specimens of the Assam Tea Company's importations are also exhibited here. Some of these specimens are very fine as may be readily perceived, and I am indebted to Messrs. Phillips no less for their kindly-accorded information on many points than for the standard samples now presented to your notice. That tea is very largely adulterated in the Chinese ports before shipment, and during the first preparation of the leaves also, I think the evidence is very convincing; but as to the proportional amount of adulterated tea imported into this country, we have little or no data to go upon. Taking the average of the black teas sold retail in London, I believe that out of 100 samples, about 61 would be found more or less impure or adulterated, while of green teas the per-centage of adulterated samples would be approximately 78. Teas are adulterated with a variety of substances, but the list is too long to enumerate. A good way of examining teas is to sift them over a white, smooth sheet of paper, when the colours, or "facing," will be presented in a convenient form for analysis. When burnt, tea should leave a white ash—a coloured one indicating mineral adulteration. To detect the presence of other leaves in tea, hot water should be poured upon a small portion, and the unfolded leaves can then be inspected under a small lens, and compared with standard specimens of teas, and the leaves of other plants. Here again, however, the microscope presents the only accurate method of determining the true nature of broken leaves. Diagram 1 is a rough outline representation of the leaf of the ordinary variety of tea *(Thea viridis)*; No. 2 being the kind grown in Assam, or, perhaps, more properly Assam. Paraguay tea is shown in the next diagram; and the leaf of coffee-plant, used in Sumatra and elsewhere as a substitute for tea, and sometimes imported into this country as an adulterant, is sketched in diagram No. 4. The leaf of the *Gaultherio procumbens*[1] is employed in North America as an infused tea; it is there called Mountain Tea. In this country I have once detected it as an adulterant. For the actual leaf specimens from which these diagrams were drawn, I am indebted to Mr. J. de C. Sowerby, of the Royal Botanic Society. Some interesting specimens of "brick-teas," "tea-lozenges," and other curiosities lent by the Council of the Royal Asiatic Society, are contained in the glass-case before you.

Coffee is not often very injuriously adulterated, but it is painful to reflect how people, especially poor people, are at once defrauded of their money and robbed of their health by the vendors of ground coffee, both in London and the provinces, for, out of every 100 samples obtained from retail dealers, I do not believe that more than eight will be genuine, unsophisticated coffee of the quality inquired for.

Coffee, in all its varieties and conditions, may be distinguished from chicory, or any other roasted roots and beans, with the aid of the microscope. The pure article should leave a white ash when burnt, and should not immediately impart a coloration to cold water, as a "mixture of chicory and coffee" will do. Chicory, in its turn, is adulterated with inferior roots, brickdust, and Venetian red.

The cocoas and chocolates of commerce are adulterated to an extent which I must simply term frightful,—at this moment I hardly know a wholesale or retail establishment selling the pure ground nuts. On this account I have not accepted specimens of cocoa from anyone,—for I need hardly tell you that the samples of food-products before you have been kindly contributed by various manufacturers and importers of whose high character and general respectability I had previously assured myself.

Time will not permit me to describe at length the adulterations of wines, spirits, and beer; their detection, too, is often so laborious a task that a non-scientific man would utterly fail in attempting it; so he had much better, before "ordering in a quantity," take Dr. Lankester's advice of last week, and hand the sample over, with a fee, to a professional chemist.

On one point relating to beer I will a make a single observation, viz., that I have actually detected strychnine in two samples of bitter beer. Specimens of porter, double stout, and pale ale, from the brewery of Messrs.

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[1] The plant from which "*oil of winter-green*" is obtained.

Gasthorne and Co., of Westminster, are on the table; I can bear testimony to their remarkable purity.

I have thus briefly and imperfectly laid before you a few of the leading facts connected with the heartless system of adulteration practised with our daily food, and I beg to apologise for the incomplete nature of my illustrative specimens in some sections, arising from circumstances over which I had no control. The provisions of the recent Act for preventing adulteration are so well and so generally known that I need not enumerate them here. Two remarks, however, may not be out of place. The people of this country, I hold, are not indifferent to the subject, as is constantly asserted by interested parties, but do not know in what way to act, or how to obtain redress for their wrongs; deeply and cruelly do the people feel how hard it is that their daily bread, for which they all work, for which, too, let us hope they all pray, should be falsified and deteriorated with impunity, knowing too that as long as the monster Adulteration exist, so long will it bring death and disease to them and those dearest to them, and pour a golden stream into the purses of its worshippers.

Still, the difficulty may be mastered in one way, and to the possibility of that way being opened up, thousands of our countrymen are now looking forward in anxious expectation. Need I say that what science unaided cannot do, what legislature as it stands cannot attain, may be accomplished through the instrumentality of the "Society for the Encouragement of Arts, Manufactures, and Commerce."

I do not hesitate to assert, and I entreat the Council of this Society to give this suggestion of mine their best and most earnest attention, that if a Special Committee were appointed on the adulteration of food and drink, the evil might be greatly lessened in three years, and almost annihilated within six or seven.

Among the members of such a committee, trade and manufactures, commerce and law, chemistry and medicine should be well represented; and among the more important of their duties would be—

1st. The adoption of a series of standards of those goods whose legitimate composition is at present indefinite, such as vinegar, cocoa, currie-powder, &c.

2ndly. The consideration of what colouring-matters should be permitted to be used by confectioners and others, and of those which should be prohibited altogether.

3rdly. The extension and improvement of detective analysis in points where it is now deficient.

4thly. The consideration of the best steps to be taken to prevent the importation of adulterated articles. And,

5thly. To give all possible aid to William Scholefield, or any other man, who will endeavour to introduce a more stringent measure for the suppression of adulteration than is now in existence.

In conclusion, I have most gratefully to acknowledge the kind assistance afforded to me by Dr. Forbes Watson and Sir Emerson Tennent, in furnishing me with certain statistical information; by Mr. W. Neal, of the Royal Asiatic Society; and by my brother, Mr. C. A. Scott; to these, and several other gentlemen, I beg to tender my best thanks.

Mr. Leonard Wray said the paper they had just heard read mentioned the adulteration of tea, and thus afforded him an opportunity of referring to some assertions that he made at the last meeting on that particular point. In the paper which he had the honour of reading on that evening, he stated that seven-eighths of all the tea imported from China to the United Kingdom, of the last crop, was adulterated; and he gave, as his authority, the minutes of a meeting of tea merchants, held at Canton, on the 18th of April last. He had not, however, at that time the pamphlet containing those "minutes" but was speaking from the letter of a gentleman who had been for upwards of 20 years connected with the wholesale tea trade of London, and who, by referring to those "minutes," led him (Mr. Wray) to believe that the statement, as to the amount of adulteration, emanated from that meeting. He had since obtained the pamphlet, and although he certainly was in error in this one particular, yet it appeared from the testimony of this gentleman, that there really was no exaggeration in the statement that seven-eighths of the last crop of tea imported into the United Kingdom was more or less adulterated. In the *Morning Star and Dial* of Monday, the 28th inst., that gentleman, Mr. William Green, of Forest Hill, wrote as follows:—

"The Society of Arts, through the assistance of Mr. Wray, has done a great public service, in making known the extent of adulteration of tea in China, when intended for the English market. A correspondent of the *Star and Dial*, of Friday, doubts the correctness of the statement that seven-eighths of the tea imported here is adulterated. That, I believe is rather below the fact; and that a gentleman of practical experience for 22 years should be startled by the announcement, surprises me. The character of the last five years' importation, however, powerfully sustains the averments in Mr. Wray's paper. The initiated, as well as the general public, will permit me to say that the adulteration—for convenience—may be described as of two classes, the positively spurious and the sophisticated. The former is known in the trade as Tayshan Congou; it also represents most of the pekoes, capers, and Canton greens. In their manufacture is employed exhausted leaf; also leaves from three plants, *Gynura auriculata*, *Ardisia crispa*, and a common species of mint. These teas are mostly from Canton and its locality, and their quality for the most part is execrable. Those which I have denominated as 'sophisticated' are from Foo-choo-foo; and for purity they rank higher than the former. The adulteration has, however, gradually extended and increased for the last five years. The new crop of this class has just arrived. It is nearly all refined and re-manipulated, having had large quantities of old and common leaf incorporated, though imported and sold as new and fine tea, &c., &c."

Now, if this testimony had emanated from an inexperienced man, he (Mr. Wray) should certainly not bring it before the meeting; but Mr. Green had had very great practical experience; he was in one of the largest wholesale tea warehouses in London, and, for the last twenty years of his life, had had daily experience in the article of tea. He could have no hesitation, then, in giving him as an authority. Mr. Green had promised to attend there to-night, if possible, in order to support in person the assertions which he had not shrunk from publishing to the world through the newspapers. The main point, then, in that part of his (Mr. Wray's) paper, which treated of the enormous amount of adulteration practised by the Chinese on the teas they sold to our merchants, remained as he had stated it to be. They must all, however, be glad to know that our highest merchants in Canton had pledged themselves not to purchase any teas which they knew to be adulterated; although the British Consul, Mr. Winchester, distinctly told them that success could scarcely be hoped for so long as the foreign buyers continued, in the hope of gain, to purchase these adulterated articles of the Chinese. The object of this Society must be to elicit and to disseminate truth, so that whatever subject it had in hand might be presented to the public in the clearest possible light, and free from all suspicion of incorrectness. Let this question of tea, then, be thoroughly and impartially investigated.

Mr. W. J. Bland wished sincerely that the same fairness which characterised this Society was displayed also by the members of the public press, who had chosen to insert the views of one side of the question, but utterly refused to publish the other side of the case. His letter to the *Star* newspaper, in reply to the strong assertions of

Mr. Green, had been denied publication in that journal. If an opportunity were afforded him, he was prepared entirely to disprove the statements which had gone before the public in the *Journal* of this Society. He would take upon himself to say, from the past history of the tea trade, that, so far from seven-eighths of the tea which came over to this country being adulterated, not one-eighth of our annual imports of that article was adulterated. As an old member of the Society, he had in former years brought forward facts upon this subject in the presence of Mr. Twining, Mr. Gibbs, and other leading members of the tea trade; and now, looking to the statements put forth in this room at the last meeting, he asked that the gentlemen who had made those assertions as to the alleged amount of adulteration of tea should bring forward evidence in support of them. If, as was alleged at the last meeting, seven-eighths of the tea now in our bonded warehouses was adulterated in the manner described, let them bring samples here, and he would undertake to prove that it was not adulterated or sophisticated. He claimed that this should be done, because the last speaker had urged upon the meeting to give an opportunity for candid investigation into the question, and, as he had previously stated, he was ready at any moment to come forward with proofs in support of what he had stated—and he would add that at no time within his own recollection were the public ever so safe upon the subject of tea as they were at the present time, for they now had teas of better character than was the case under the former *régime* of the East India Company. He unhesitatingly made these statements, and would be prepared to substantiate them.

Mr. Wm. Hawes confessed that he could not commence his observations upon this paper by characterising it, as it was generally in his power to do, as an able and useful one. He thought when charges of adulteration of food were dealt out in the wholesale manner in which they had been that evening, much more was required than the simple statement, before such charges would be received by an assembly such as he now saw before them. To say that 87 per cent. of the bread they ate was adulterated, was, in his opinion, a misrepresentation. To say that seven-eighths of the thousands of tons of teas in our warehouses was adulterated to the extent alleged, was in his opinion a misrepresentation. What did adulteration mean? Did it mean that seven-eights of the article so called was not tea? or did it mean that some processes were employed by which a different character was given to it (let them call it adulteration if they would), but which did not affect either the wholesomeness or the quality of the tea? If it was intended to take refuge under that statement, he would say that the assertion that this was practised to the extent of seven-eighths of the total quantity of tea imported into this country was a misrepresentation. If seven-eighths was not tea at all, then it was but fair that the promulgators of such a statement should describe what the article really was. When it was alleged that the dealers in corn were guilty of such fraud that they put 12 inches of good corn on the top of the sacks, and 20 inches of bad corn at the bottom, it ought to be proved by bringing a sack of corn bought in Mark Lane into that room. In his opinion, the great mass of traders in this country were not so thoroughly dishonest as this paper would lead them to believe. That there were fraudulent merchants and traders they all knew; but, when they went through the whole list of articles forming the chief aliments of the population, and asserted that not one-half of the things they bought were pure—not half were the articles they were represented to be, he repeated that very much more proof was required before they would consider those charges substantiated. The fashion of dealing in these assertions was too common. They had a recent Act of Parliament to protect them against adulteration of food. This was, in his opinion, a part of that paternal system of legislation which he thought had in this country passed away for ever; and it did virtually pass away when the House of Commons gave up the assize of bread, and testing for the purity of a variety of other articles of food. The public would generally take care of itself in such matters as these, and the less Parliament interfered with those petty details of life the more secure the public would be. What were the effects of the Excise laws, to preserve them from adulteration? None whatever. Let them legislate upon this subject as much as they would, all that legislation did was to show what the dishonest trader must avoid, and to drive him to some other means of arriving at the same end. Legislation on such a matter was all thrown away. The paper summed up the long list of grievances by telling them the means by which the evils complained of could be remedied. What were those means? The Society of Arts was to appoint a Committee from its own body, which in three years was to lessen adulteration very considerably, and in six years to destroy it altogether. The whole world had been trying in vain to effect this object; but the Society of Arts, by a Committee appointed by itself, with no public responsibility whatever, was to drive out this great bugbear of adulteration, and in six years! Was it possible that if seven-eighths of their tea was adulterated; that if twelve inches of good corn would conceal twenty inches of bad corn; that if pickles were adulterated with copper; if deleterious admixtures were compounded with our pepper and mustard; and if, indeed, every article sold by the butcher, baker, grocer, fruiterer, and greengrocer, were sophisticated or adulterated, that all this could be remedied in six years, by a Committee of this Society—a committee of traders, chemists, and *dilettanti* members—men appointed by this Society! He could never conceal his opinion upon such questions as those. He believed legislation upon them acted perniciously. He believed this microscopic examination of food—though very beautiful, perhaps, in itself—was of little practical service to the public, and worse than useless if it led to such statements as had been made that evening. He disagreed with the author of the paper in many of the points brought forward. He believed the results arrived at were exaggerated, and the remedies suggested were of the most futile kind.

Dr. Lankester confessed he had not expected to hear such a speech as that he had just listened to. He was surprised that a member of the Council should turn into ridicule the application of science to the practical arts of life; and he felt that, even supposing some indiscretions had been committed by Mr. Wentworth Scott, nothing could warrant the severe remarks which they had just heard. He wished this subject could be discussed without any unpleasant feeling, and he would endeavour to bring back the meeting to look at the scientific facts which the paper had brought before them. Were they to believe Mr. Scott's assertions as an honest man, or not? Were they to credit his statement that 60 per cent. of the bread he had tested was adulterated with alum or not? Mr. Scott was prepared to stake his reputation upon the matter, and on that ground he (Dr. Lankester) believed him. This was not the first time they had had this subject before them. They all recollected the analyses of food which were given in the *Lancet*, and yet he never heard it said that Dr. Hassall had given publicity to that which was untrue, or that he had unduly libelled the tradesmen of the country. He (Dr. Lankester) did not for a moment pretend to say that all persons who sold adulterated goods were aware of the extent to which the adulteration existed; but let them not encourage men to believe that they might put deleterious substances into food, and as long as this could not be detected, they were to go free. Mr. Scott, by the aid of chemistry, had shown the public what they were taking, and had warned them what they ought not to take. He had shown that the public required more scientific knowledge upon these subjects. He had brought before them a series of facts which showed the necessity, not only of the community being instructed in the elementary details of science, but also that tradesmen who wished to deal in an honest article should know something of the nature and scientific character of the products which they sold, and something of the laws by which human life was regulated. If a tradesman received a chemical education, he would be able to detect many of these adulterations. There, for instance, was a bottle of pickles which was manifestly adulterated, and which stood side by side with an unadulterated article. The vendor of that article knew it was not genuine—he knew the West Indian pickle was adulterated, and anyone who ate freely of it at supper would find this out in the course of the night too. Such a thing should be put down, and if they were unable to effect this through the medium of the ordinary influences which operated upon society, they might fairly ask for the help of the Legislature in the matter. He was surprised therefore to hear a gentleman whom he knew advocated the amendment of the laws of this country, condemn legislation on this subject. With equal reason they might condemn legislation for the criminal who robbed his employer, as for those who systematically sold bad articles of food over the couter. Surely the Legislature, which was effective in the one case, would be effective also in the other case; and he contended that the Hon. Member for Birmingham, who carried that Bill through the House was right in endeavouring to set legislation in motion on such a subject. What he complained of was that, like too many Acts of Parliament, it was insufficient, and could not be practically worked. The fee that was fixed for the analysis was too small to insure a proper investigation by the chemist; it would not pay him for the time that was occupied by it. In the next place, the person who sold the goods must be proved to have a knowledge that he was selling an adulterated article, and unless they brought that guilty knowledge home to him, he could not be convicted. He hoped that further legislation would be really effective. The existing Act had, however, been the means of calling attention to the fact that there was something like legislation going on, and that there were means by which these practices could be prevented. With regard to the question of tea, it was one which could be answered by the analytical chemist alone. Let them not permit any gentleman who was only a dealer in tea, and knew nothing of chemistry, to say that tea was not adulterated; but it should be put into the hands of the botanist to say whether there were other leaves in it than those of the tea plant; and into the hands of the chemist to say whether there was anything besides the natural product. He would not take the opinion of a man who had dealt in tea for 25 years, if he knew not how to distinguish between the leaf of tea and the leaf of any other plant. Let them not, upon such evidence as that, decide that tea dealers were too honest to sell bad tea. Passing over the subjects of the paper, he would say that he thought Mr. Wentworth Scott had brought before them a number of practical hints by which persons with a limited knowlege of chemistry might assist themselves in judging whether an article was adulterated. With regard to bread Mr. Scott's process for detecting alum was, perhaps, too complicated for general application, for alum was not easily detected, and he warned persons against concluding from such evidence as Mr. Scott had brought forward as to the presence of alum in bread. He (Dr. Lankester) thought the test of the blow-pipe was more satisfactory in that case. Mr. Scott had spoken of the adulteration of meat. There was an astounding thing! In the newspapers of that day there was an account of hundreds of pounds of meat having been seized a few days ago by the sanitary authorities of the City. There was the fact of the unwholesome meat being exposed for sale in the market. He knew how common it was to bring into the markets of London measled pork, which, once seen and known, would always be recognised as diseased; which, when eaten by human beings, resulted in the production in the stomach or bowels of a worm, which was the pest of life ever after. Ought the public to be protected from such things as these? That pork, if not sold in joints to the public, found its way to the sausage-makers; and were they not to interfere between her Majesty's subjects and the horrible tape-worm? Mr. Scott had also referred to the adulteration of pepper and other spices. There was always more or less difficulty in detecting adulteration in powders. These ginger, pepper, and mustard powders might contain almost anything, for what the public knew, and it was not easy for them to ascertain what they really were. Upon this part of the subject he need only refer them to the great Liverpool pepper case, which occurred within the last twelve months. They saw from that how very difficult it was to detect the adulteration of those spices. It was frequently to the interest of tradesmen to adulterate even only to the extent of 5 or 10 per cent., to enable them to undersell their neighbours to that extent; so that there seemed to be good reason why the Government should be alive even on this matter. Even in cases where there was no Excise duty to be protected, there was an equivalent of value which no Government ought to overlook. Human life was money value. A slave in America would be worth from £100 to £200, and surely the life and labour of a human being in this country was worth that amount. Therefore, as a mere money question, it behoved the Government to see that the most stringent measures were carried out to detect the adulteration of food.

Dr. Kinns wished to mention the fact, that whilst staying with a friend in Derby, last summer, he had the most convincing proofs afforded him that plaster of Paris was used by tons for the adulteration of mustard. It was sent in very large quantities to some of the largest mustard manufacturers in the kingdom.

Mr. Wm. Green had attended solely for the purpose of confirming the statements he had made in print, as alluded to by Mr. Leonard Wray, upon the subject of the adulteration of tea, which had occupied a large share of his attention during the last five years. His experience in one of the largest houses in the kingdom for twenty years had been, that teas were very largely adulterated; and he was prepared to stake his reputation upon the statement he made, that of the tea which had been imported into this country, during the last three years, seven-eighths was adulterated. Startling as that statement was, he might, perhaps, add an explanation, which would, in some measure, qualify it. Mr. Wray had correctly stated, he believed, the extent to which the tea was adulterated in putting it at seven-eighths, and that only one-eighth was absolutely pure. Out of the whole quantity one-eighth might be regarded as a spurious material altogether; that left three-fourths of the stock to be more or less adulterated or sophisticated. That three-fourths might be taken to be adulterated to an extent varying from 1 to 50 per cent.; and if Mr. Bland (who might be looked upon as one of the fathers of the tea trade) could convince him that no adulteration took place, he should be happy to acknowledge himself mistaken. He felt it was impossible, whatever a man's knowledge of tea might be, to detect adulteration in it when carried to a comparatively small extent. He would say that of the entire crop of Foo-choo-foo tea which had just arrived, he believed there were but few lots that were absolutely pure. He had obtained some lots of that tea, and he found that, although it was only three months old, it had already deteriorated in commercial value to the amount of 6d. per lb. Then, with regard to the black tea of Congou, the orange pekoe, and other varieties, he believed the assertion that seven-eighths was adulterated was borne out by the facts. He believed the quantity of tea exported from this country amounted to about 10,000,000 lbs. annually, shipped principally to northern ports; and it was some consolation to know that the tea so exported consisted chiefly of the most inferior qualities. He had waited anxiously to hear some suggestion by which this confessedly great evil might be remedied or overcome; and it appeared to him that, as long as they carried on a war with China, as long as they exacted compensation for such a war, and as long as they taxed the produce of that country at home, it was in vain to expect that they could get a true and genuine article in tea. He would urge upon those who were anxious to be delivered from this evil, that they should direct their attention to the obtaining of teas direct from the grounds where they were grown.

(To be continued.)

PHARMACEUTICAL SOCIETY.

Wednesday, February 6, 1861.

T. N. R. Morson, *Esq., President, in the Chair.*

Mr. D. Hanbury, jun., made a communication respecting "*Balsam of Peru.*" Having read in the *American Journal of Pharmacy* that by some Bulls of the Popes Pius IV. and V. Balsam of Peru was allowed to be substituted for Mecca Balsam in the preparation of the Chrism used in several of the sacraments of the Catholic Church, Mr. Hanbury wrote to his friend Signor Vicenza Sanguinetti, a Professor of Botany at Rome, to procure for him, if possible, copies of the Bulls in question. On examination it was discovered that the Bull of Pius the Fourth did not exist in the archives of the Vatican, but that of Pius the Fifth was found, a translation of which Mr. Hanbury read to the meeting. After reciting the usual formulæ, it went on to say, that, as it had been represented that in some parts of the world the true Balsam tree, or oil to be used in preparing the Chrism, was not to be found, but that another oil of a similar character existed in the country, the Pope conceded full licence and permission to the Bishops and Archbishops sojourning in those parts to freely and lawfully use this other oil in preparing the Chrism, and declared that the same efficacy should follow. The Bull is dated the 2nd of August, 1571, the sixth year of the pontificate of Pius V. Chrism, from χρισμα, oil, is defined by Dr. Hook as "oil consecrated in the Romish and Greek Churches by the Bishop, and used in baptism, confirmation, orders, and extreme unction. . . . There are two sorts. The one is a composition of oil and balsam made use of in baptism, consecration, and orders; the other is only plain oil consecrated by the Bishop, and used for catechumens and extreme unction."

The Chairman said the paper was of more historical than pharmaceutical value; but such matters as it referred to were of great interest, and the introduction of them greatly enlivened the meetings.

Mr. Hanbury said he should like to know how Mecca Balsam was obtained for use in this country. It was extremely rare. He had seen but one good sample, and that was in the Exhibition of 1851.

Mr. Allchin then read a paper on the "*Purification of Gum Resins.*" Mr. Allchin objects that the process given in the London Pharmacopœia for the purification of these bodies—viz., boiling them with water, straining, and then evaporating the water away—is in all cases prejudicial to the substance. In the case of those resins which contain a volatile oil, that portion of them is lost, while others, such as assafœtida and ammoniacum, are so much altered in their physical condition as to be rendered unfit for some medicinal purposes. Ammoniacum in tears, which consist of the pure juice of the plant, makes a good emulsion with water, so does the mass, which consists of the tears probably agglutinated together by heat. But ammoniacum purified by the Pharmacopœia process cannot be used for making the mistura ammoniaci, as it will not form an emulsion. Assafœtida, also, when purified, becomes hard and friable, and will not suspend in barley water, as ordered for the enema assafœtida. The only object of the purification is to get rid of mechanical impurities; but if this cannot be effected without causing some loss or injury to the gum resins, the author thought it would be better to use them in their natural condition.

Mr. Hanbury said he also had been struck with the objectionable nature of the Pharmacopœia process, but confessed that he did not see his way out of the difficulty. With Sagapenum he had tried aqueous and spirituous extraction, and had then re-combined the two extracts; but he had not been satisfied with the result. In Germany it was customary to expose the gum resins until they became hard and brittle, and then to powder and sift in order to separate the woody particles mixed with them. The powder was then put up in paper cornucopias, which were placed in tin cases. In this way the powder kept very well, and did not agglomerate. Ammoniacum, however, could always be bought pure.

The Chairman said the best way to purify gum resins would be to treat them alternately with ether, alcohol, and water. In this way no part of the substance would be lost, but probably the cost of the process would be objected to.

Mr. Hills and another Member urged the Council of the Society to use their influence in endeavouring to procure the collection of pure articles. Senna and scammony used always to be adulterated, but were now to be had quite pure.

Mr. Hanbury said that in the case of some of the gum resins nobody knew where or by whom they were collected.

The Chairman said pure articles could generally be had if people would only pay the price for them. It was well known that pure otto of roses and pure musk were unsaleable in this country because of the cost.

Professor Redwood then read a paper "*On the Preparation of Liquor Potassæ.*" Simple as the process for the preparation of liquor potassæ is, it admits of deviations which cause considerable variations in the product. When properly made with pure carbonate of potash and pure lime, it is a transparent, colourless solution of potash in water. If prepared with ordinary salt of tartar, however, it will contain sulphate and chlorides, which are allowed in the London Pharmacopœia, and if carelessly made it may contain undecomposed carbonate of potash, silica, alumina, oxide of iron, or lime. It was in the middle of last century that Dr. Black first discovered the true cause of the causticity of lime and alkalies. His contemporary Meyer supposed that potash deprived the lime of an acid to which he gave the name *Causticum*, or *Acidum Pingue;* but Black showed that the true reason was the loss of a gas, to which he gave the name of *fixed air*. The London Pharmacopœia of 1746, ten years before Black's discovery, gave the following directions for the preparation of what is there called *Lixivium Saponarium:*—"Take equal weights of Russia potash and quick-lime, and throw water upon them by degrees until the lime is slaked; then throw on more water, and stir all together that the salt of the ashes may be dissolved: after some time pour the liquor, filtered through paper, if needful, into another vessel. A true standard wine pint of this liquor measured with the greatest exactness ought to weigh just sixteen ounces; if it is heavier, for every drachm that it exceeds that weight, an ounce and a-half of water in measure is to be added to each pint of the liquor; but, if it is lighter, it must be boiled until the like quantity of water is carried off, or else must be thrown upon fresh lime and ashes." Now, as the wine pint of pure water weighs 7291 grains and sixteen ounces troy = 7680 grains, the specific gravity of the Lixivium Saponarium would be 1·053. In 1788 the name was changed to Aqua Kali Purum, and a new process was given. The present Pharmacopœia orders only half the weight of lime to the potash, and it is directed that the carbonate of potash shall be dissolved in one portion of the boiling water, the lime to be slaked and mixed with the other portion, and the two be shaken together until cold. The Edinburgh and Dublin Colleges direct that the carbonate of potash be dissolved, and boiled in the greater part of the water, and that the lime be slaked with the remainder, and added gradually during ebullition. The latter is the process recommended in most chemical books, and the advantage of the boiling is that the lime becomes more aggregated, precipitates denser, and absorbs less of the liquid. The Professor said the questions he proposed to discuss were:—1. Which of the three processes given above was the best? 2. Whether the lime should be weighed in the form of quick or slaked lime? 3. Whether the strength of the solution might not be altered with advantage? Taking the last question first, he said that the specific gravity of the solution ordered in the London Pharmacopœia was 1·063, and contained 6½ grains of hydrated, or 5·5 of anhydrous potash in 100. The Dublin solution (sp. gr. 1·068) and the Edinburgh (1·072) are rather stronger. Neither of these three strengths is expressed in whole numbers, nor in easily divisible decimals. The most convenient would be a 10 or 5 per cent. solution, but there was an insuperable difficulty in the way of raising the strength, although none in decreasing. It had been found that to make a ley it was necessary to have ten times the weight of water to the carbonate of potash—more would take up lime, and less would not remove the carbonic acid from the carbonate of potash. The best strength then would be a five per cent. solution, which would have a specific gravity of 1·053, exactly that of the Lixivium Saponarium of 1746. This would have 5 grains of potash in 100 grain measures of solution, half-a-grain in 10, 3 grains in a drachm, and would deviate but slightly from the present strength. The next question discussed was, which was the best form in which to weigh the lime, as quick or slaked? Various impurities are found in commercial lime. Some has not been causticised, and cinders and stones are always found after slaking. The Professor recommended, therefore, that 2 pounds of compact, massive lime should be slaked with 1 pound of water, and when the aqueous vapour had escaped and the heat had subsided, it should be sifted through a sieve with about 20 apertures to the inch. The resulting powder had a definite composition. What quantity of this should be used? It was important to have more than was necessary to take the carbonic acid from the carbonate of potash, so it would be found most convenient to take equal weights of the slaked lime and the carbonate of potash. These should be mixed with cold water as directed in the Pharmacopœia of 1746, and then pure liquor potassæ would be obtained. Alumina was always found in the liquor which had been boiled. The following was the precise form recommended:—Take pure carbonate of potash, slaked lime, of each seven ounces and a-half, water half-a-gallon, mix the lime with three pints of water in a green Winchester quart bottle; dissolve the carbonate of potash in the remainder, and add it to the lime by degrees, shaking well after each addition. Afterwards shake the whole well until no carbonic acid escapes from the clear liquor on the addition of an acid. Filter rapidly through calico.

Mr. **Whipple** said that the processes of the Pharmacopœia were intended to prepare useful medicines, and not pure chemicals. The liquor potassæ of the Pharmacopœia, was a remedy and not a test, and some of the so-called impurities, chloride of potassium for example, greatly increased its value as a medicine. [Amid some demonstrations of impatience on the part of the audience, we understood him to express his belief that *pure* medicines had caused a vast destruction of human life.]

In reply to a question from Mr. **Hanbury**, Professor **Redwood** stated that non-effervescence on the addition of an acid was a sufficient indication of the absence of the carbonate. Lime-water was a more delicate test, but when it was used, it was necessary to dilute the liquor potassæ with about as much distilled water.

THE ROYAL INSTITUTE OF BRITISH ARCHITECTS.

At the meeting of the Royal Institute of British Architects on Monday evening, the adjourned discussion on the processes adopted for preserving the Houses of Parliament from decay was resumed, Mr. **Digby Wyatt** in the chair.

The proceedings commenced by the reading of several communications which had been received since the previous meeting, among which was a letter from M. Szerelmey, in reply to some observations that had been made respecting the want of success of his "Zopissa" process in the Speaker's Court. M. Szerelmey denied that the stone had exfoliated, and he maintained that the application of his process had indurated the surface, and been quite successful. Mr. Tite also read some letters he had received, and he repeated the experiment of mixing water-glass with a solution of muriate of lime, which produced a hard insoluble substance; and he observed that if it were possible to devise the means of combining that substance intimately with the surface of the stone it would be an effectual preservation.

Mr. **Smith**, one of the Commissioners appointed for the purpose of selecting the stone for building the Houses of Parliament, explained the proceedings of the Commission, and made some revelations that excited much surprise. He stated that the Commissioners, of whom he was the only survivor, after numerous investigations and consultations with competent authorities, reported that the stone found at Bolsover Moor and the neighbourhood would be most suitable for the purpose. He said it was a mistake to suppose that the foundation of the Houses of Parliament had been built with stone from the quarry of Bolsover; for, though that quality of stone was preferred, it could not be quarried in blocks sufficiently large for the purpose; and he alone went down to examine in what part of the neighbourhood the same kind of stone could be procured. He found the same, or a similar, bed of magnesian limestone at Aston, and he brought a slab of it, six inches by nine, to London, where it was submitted to the other Commissioners, who decided that that stone should be used. He represented that it would be necessary that some one should be on the spot to see that the quarrymen got the stone from the bed selected, and it was arranged that he was to go down once or twice a-week to inspect the working, for which he was to have received a salary of £150 per annum. But when he required to know by whom that salary was to be paid, no one would take the responsibility of paying it; consequently the arrangement was not carried into effect, and the men were left to quarry the stone as they liked. To that circumstance Mr. Smith attributed the unsound quality of a great portion of the stone that was employed. In building the Museum of Practical Geology with the same kind of stone the workmen were properly inspected, for Sir Henry de la Beche said he was determined that the stone of that building should stand. The care bestowed in the quarrying had had that effect, for whilst the Houses of Parliament are rapidly crumbling, the Museum in Jermyn Street exhibits not the least appearance of decay.

Mr. **Warrington**, the Operative Chemist of Apothecaries' Hall, explained the chemical action of Mr.

Ransome's silica process. It consists in applying to the stone in dry weather several washes of a weak solution of silicate of soda, commonly called water-glass, which enters the pores of the stone. A solution of chloride of calcium (muriate of lime) is afterwards applied, and these combine together in the stone and form silicate of lime, the insoluble substance produced in Mr. Tite's experiment. Mr. Warrington said that as the interior of the stone contains a quantity of moisture, an efflorescence would appear on the surface, though the particles of the stone might be indurated; and any process that stopped up the pores of the stone must produce exfoliation of the surface.

Mr. **Gilbert Scott** observed upon the generally bad building stones employed in London, and attributed it to the want of care in selecting the stone from the proper part of the quarry. Portland stone, which has been so extensively used in London, varies very much in quality; that which is the whitest, and is generally preferred from its appearance, is a very soft stone, while the upper bed is one of the best building stones that is used. Even granite, unless properly selected, may soon decay. He remarked on the various processes that had been adopted for the preservation of stone—viz., that of Mr. Dane, which consists of sulphur and boiled oil, that of M. Szerelmey, which he endeavours to keep secret, and that of Mr. Ransome. He spoke hopefully of the two latter processes, and he trusted that a committee would be appointed to consider the whole subject.

Mr. **Godwin** mentioned the complete failure of Mr. Dane's process as applied to the statue in Caen stone of Mr. Coram, in the front of the Foundling Hospital.

Professor **Ansted** said that the characters, not only of the same stone, but of the same bed of stone, were often greatly altered by the superincumbent strata, and by other circumstances, which rendered it impossible to determine, without examination in the quarry, whether any stone is or is not fit for building. He expressed the opinion that the preserving processes, to be effective, should be applied soon after the stone is quarried; for when it has begun to crumble it was impossible for any application to be of permanent utility.

Several other gentlemen wanted to address the meeting, therefore the discussion was again adjourned.

The **Chairman** announced that instead of appointing a committee, as at first proposed, it was intended to petition for the appointment of a commission, to consist of chemists, civil engineers, and architects, to consider the subject.

The meeting then adjourned.—*Mechanics' Magazine.*

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—The last number of your journal contains a report of a paper read before the Society of Arts, by Mr. W. S. Scott, "On Food; its Adulterations, and the Methods of Detecting them," subjects, which all must concur with Mr. Scott in regarding as of "grave importance." I cannot but think, however, that in referring to matters so deeply interesting to all classes of the community, it would have been more judicious to have prefaced, by some cautionary remarks, the publication of such extraordinary statements as those advanced by Mr. Scott, since it behoves all who really desire to see the Adulteration Act efficiently carried out, to have a care that no manifestly absurd or unauthenticated assertions are put forward.

Mr. Scott informs us that adulteration consists, (3rdly,) in "The employment of various colours, essences, &c., for the purpose of rendering the articles more attractive to the eye, or to the senses of taste and smell."

Are colours and essences of every description to be henceforth decried?

Again: "The cause of adulteration is simply that fraudulent tradesmen wish to acquire in an indolent manner more money than the honest man can obtain by years of toil and application."

In reference to bread we are informed that "in most cases, as supplied in the form of half-quarterns, it is but a very frail reed indeed, for a working man to lean upon." That it is adulterated with chalk, gypsum, alum, and bone-dust. Mr. Scott expresses his "well-founded opinion" that "Mr. Salmon is almost the only baker in the district of Chelsea who sells perfectly pure and unadulterated bread!"

"That 87 per cent. of the bread in London is more or less adulterated!"

Can Mr. Scott for a moment imagine that such sweeping assertions as these, unaccompanied by a statement of the grounds upon which they have been made, will be credited? Will nearly nine-tenths of the bakers of London quietly submit to be told that they are "fraudulent tradesmen?"

Surely we ought at least to have been favoured with some particulars as to how many samples have been examined; from how many shops such samples have been procured, and what was the quantity and nature of the adulterating material employed in each case. It is also of great importance that we should be informed what was the actual process adopted by the analyst for their detection:—Was "crumbly texture" taken as evidence of the presence of potatoes, or "brittleness of structure" regarded as a proof of rice flour? and, more especially, was the presence of alum proved by the process described?

Does Mr. Scott mean to assert that water in which pure and genuine bread had been soaked would not produce a "cloudiness" on the addition of a salt of barium? or that the ash of genuine bread, after being treated as directed, would not yield a precipitate with ammonia, even if no alum had been used in its manufacture? Possibly if Mr. Scott had furnished us with the quantity of the alum found, we might be able to place more reliance on his results; but, in default of any information of the kind, the inference is unavoidable that the facts upon which he has arrived at such startling conclusions are far from trustworthy. As regards bone-dust, I am aware that it has frequently been stated to be one of the materials employed in the adulteration of bread; but I have hitherto, in spite of numerous enquiries, not succeeded in meeting with any one who had actually found it, and shall therefore be much interested in learning under what circumstances, and in how many instances, Mr. Scott succeeded in detecting it.

Passing on from bread, however, we find statements equally astonishing with those already alluded to.

Oats adulterated to the extent of "25 per cent. with brewers' or distillers' grains!" and rape cake alluded to as an article of food usually given to horses!

By what process can brewers' grains be made to bear any resemblance to oats? In what part of the country are horses fed with rape cake?

Again: "Of the meat disposed of at the London markets nearly 10 per cent. is more or less unfit for human consumption!" And "Arsenical solutions are employed without hesitation" to counteract the effects of decomposition or disease; and this practice is "quite common" with game and poultry.

Surely some proof should have been forthcoming to substantiate so serious a charge as this. How long has the discovery of such wholesale poisonous practices been made? Why were not the Inspectors of Meat immediately informed, and the offending parties brought before a magistrate? Why is the first publication of the discovery of such scandalous practices reserved for a meeting of the Society of Arts?

Mr. Scott has met with vinegar to which oxalic and formic acids have been "purposely added." Certainly a very bold assertion unless backed by the most unequivocal proofs. What possible motive could there be for the use of either of these acids?

Mr. Scott proceeds: "About 91 per cent. of the pickles sold in London are more or less adulterated," without, however, giving any statement of the figures whereby he arrives at so precise a computation.

"Mustard is never" to be met with pure, and he has purchased a sample so largely adulterated with plaster of Paris as to set when mixed with water!! Surely such a sample must have been sold to Mr. Scott as a good joke to pass upon an analytical chemist, for we can scarcely be expected to believe that any man would substitute plaster of Paris for mustard to such an extent with a view of deceiving a customer who had the use of four out of his five senses.

Mr. Scott appears to have a very different idea of the term adulteration from that generally entertained. The law already sufficiently protects us against cases of gross swindling and frauds which become manifest by mere inspection. By exercising ordinary care we can prevent ourselves from being taken in by "wooden nutmegs," by the substitution of barley for wheat, and there can be no difficulty in detecting marble in lump sugar, or brewers' grains in oats. I cannot conceive that such frauds as these fairly come within the category of adulterations in food.

We must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited; such, for example, as the use of copper-salts in pickles, chromate of lead or orpiment, in buns, arsenical colouring matter in confectionery, &c. Neither should the sale of mixed articles as genuine (*e. g.*, arrowroot, if mixed with other starches) go unpunished; but we must not forget that a good cause is sometimes most injured by its would-be friends. It seems to me that Mr. Scott's suggestions as to the course that should be adopted to protect ourselves against the ill effects of an improper dietary would do away altogether with any but the best materials for food. For example, he says "it is to be regretted that the sale of impure brown sugars of commerce should be permitted at all." On the same grounds, would Mr. Scott argue that unsound wheat should not be permitted to be sold for human food; and would he banish it from our markets? Would he support the seizure of all potatoes touched with disease? Does he mean to imply that the bread upon which our agricultural population thrive—a bread in the manufacture of which other grain than wheat is frequently employed—and the wheat often of an inferior character—is a "very frail reed for a working man to lean upon?" or does he mean to assert that the bread consumed in London by our working men is very inferior to it in nutritive properties?

I cannot conclude without urging most forcibly on all your readers that the fault of adulteration does not rest solely on the tradesman or manufacturer.

So long as the suicidal cry for cheapness is everywhere heard, so long will adulteration exist, notwithstanding the most stringent legislative enactments. Let the public recognise the fact that anything below a fair remunerating price can only co-exist with sophisticated or adulterated food, and an incalculable advance will be made towards the attainment of a better state of things, than that which we must all acknowledge now exists.—I am, &c.

VERAX.

Copper in Bread and Flour.

To the Editor of the CHEMICAL NEWS.

SIR,—Dr. Hassall in his able work on Adulteration, as well as Mr. Wentworth Scott in his Lecture before the Society of Arts (January 30, 1861), both state that copper, although said to be used in some places on the Continent, has never, to their knowledge, been found in bread or flour in this country. My experience, however, leads me to suppose its existence oftener than many persons would imagine. Having just completed a series of experiments with articles purchased in this locality, from which, in more than one instance, I extracted copper; in fact, the microscope readily revealed the presence of the sulphate in the flour.

From half-a-pound of bread I obtained a solution of copper sufficient for 12 demonstrative experiments. Iron was also present in large quantities, showing that the salt of copper used was adulterated.

Were such things practised in Turkey, the guilty individual would be summarily dealt with; in fact, nothing short of hanging, or nailing his ears to a tree, awaits him. —I am, &c. JOHN HORSLEY, F.C.S.

Cheltenham.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Preparation of Chloride of Gold.—According to M. Fordos (*Repertoire de Chimie pure et Appliquée*, liv. xii. p.373, 1860) it is impossible to obtain a neutral chloride by dissolving gold in aqua regia and evaporating the liquor to drive off the excess of acid. By this means, says the author, a hydrochlorate of hydrated chloride of gold, $AuCl_3HCl + Aq$, is produced, which is the compound ordinarily met with in commerce. When this is heated even in a water bath to drive off the last equivalent of hydrochloric acid, protochloride of gold is produced, and the salt obtained is not completely soluble in water.

II. ORGANIC CHEMISTRY.

Pyroglycerine—Explosive Glonoin.—Dr. Sobrero gives (*Ibid.* p. 400) the following directions for the preparation of this compound. He makes a mixture of two parts sulphuric acid and one of nitric acid, and when it has cooled he adds one-sixth of its volume of syrupy glycerine. It is not necessary, he has found, to operate at a lower temperature than the ordinary. The glycerine at first dissolves in the mixed acids, but the mixture soon becomes cloudy, and a quantity of yellowish transparent drops collect on the surface. The whole is now poured into a glass containing 15 or 20 times its volume of cold water. The pyroglycerine separates immediately, and falls to the bottom of the vessel; the acid water is poured off, and the product is washed until the washings are no longer acid. The glycerine may then be dried under the air pump. This compound, it seems, is one of the very limited number of explosive liquids, a fact which Mr. Ferris (*American Journal of Pharmacy*, vol. xxxii. p.524) discovered at nearly the cost of his eye-sight. It is strange that no notice of this dangerous property should be given in Gmelin's *Handbook*, vol. ix. p.501, where some account of the compound is given. The detonation is extremely violent, a large drop making a report as loud as a gun. Mr. Ferris had not perfectly recovered his hearing three weeks after exploding about half-a-drachm. An experiment may be made without danger by placing a small drop on a watch glass and touching it with a red-hot wire. When dry it will detonate on percussion. Pyroglycerine oxidises phosphorus, potassium, and copper. With hydrochloric acid it separates chlorine and produces, says Dr. Sobrero, a sort of aqua regia.

Chemical Notation.—M. F. de Lastelle has sent a memoir to the Academy of Sciences, which, however, has not been published in the *Comptes-Rendus*, in which he professes to have reduced all formulæ so as to represent only one volume of vapour. The formulæ he proposes show, he says, besides the atomic composition, the percentage composition and also the composition by volume. Further, they satisfy the law of isomorphism, and the author believes that of the capacity for heat as well. Lastly, he says, the density of gaseous bodies and their chemical equivalent may be represented by the same number. Let us hope that we shall soon hear more of M. de Lastelle's memoir.

MISCELLANEOUS.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, February 19, at 3 o'clock, Professor Owen "On Fishes." Thursday, February 21, at 3 o'clock, Professor Tyndall "On Electricity." Friday, February 22, at 8 o'clock, Professor Faraday, "On Platinum." Saturday, February 23, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

Adulteration of Food Act.—Mr. John Postgate writes:—"I think that some expression of opinion by the Society of Arts would tend materially to remove a misconception which prevails respecting the provisions of the recent Act of Parliament for the Prevention of the Adulteration of Food or Drink. The first clause of that Act makes two offences; one, that of selling knowingly articles adulterated with substances injurious to health, and would require proof of such knowledge before conviction; but the other goes to the root of the mischief, and places a remedy in the hands of the public, for it expressly states, 'every person who shall sell, as pure or unadulterated, any article of food or drink which is adulterated or not pure, shall for every such offence,' &c., forfeit and pay a penalty not exceeding £5, with costs; and for a second offence the name and residence of the offender may be advertised at his expense in the newspapers. It is, therefore, perfectly clear, under this Act, that purchasers have only to ask for the pure article and request the seller to label it as such,—and equally clear is it that impure and adulterated articles of food or drink must be sold as such; but, even these may bring the seller under the law, should they contain any material hurtful to health, as the magistrates may refuse to admit a plea of want of knowledge of his business by the seller, or that no means of knowing (an Analyst being appointed in his district) existed within his reach. Respecting the Public Analysts appointed, it is of the highest importance that they should be, not only well skilled in chemistry and the use of the microscope, but also persons of well-established reputation for integrity and honourable conduct, otherwise the power placed by this Act in their hands may be used for very bad purposes; and, I think, in all cases where analysts are appointed by local authorities, the inspectors of meat and markets should be ordered by those bodies to make purchases, submit them to the analyst, and initiate the proceedings before the magistrates,—a work quite within their duties, and one that may be carried on under this Act of Parliament. It now rests with Corporations to protect the community against the great evil of adulteration, and avert such calamities as occurred at Bradford and other places. I am glad to find, as the originator and worker of this question for several years, that the Society of Arts has taken this important matter up."—*Journal of the Society of Arts.*

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the Chemical News, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part I. of the present Volume is now ready.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*** All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the Chemical News, containing a copious Index, is now ready, price 12*s.*, by post, 12*s.* 8*d.*, handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1*s.* 6*d.* Subscribers may have their copies bound for 2*s.* if sent to our Office, or, if accompanied by a cloth case, for 6*d.* A few copies of Vol. I. can still be had, price 10*s.* 6*d.*, by post 11*s.* 2*d.* Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Per-hydrate of Alumina.—R. P. wishes to know if this substance is an article of commerce, and if so, where it can be procured.

Gas Purifiers.—H. N.—Mr. Bowditch mentions that the apparatus for his process of testing gas with hot lime, can be obtained of Mr. John Savile, jun., Wakefield, for the price of 18s.

The Regularity of Natural Forms.—An article on this subject is declined with thanks.

P. M. H.—Received.

Colouring Matter from Wood.—A correspondent wishes to know the name given to the active colouring matter obtained from wood by exhaustion in alcohol and precipitation by water. By being raised to a temperature of 250 deg. Centigrade, in a steel cylinder, when cooled, the principle deposits in the form of yellow crystals on the surface of the cylinder.

B. P.—Your suggestion of saturating the stone alternately with two salts which will precipitate an insoluble compound in its pores, is by no means new; nor are the salts proposed (chloride of calcium and sulphate of soda) the most suitable for that purpose. Mr. Ransome's process of using silicate of soda and chloride of calcium alternately, is the most perfect according to theory; but there are difficulties in the way of its successful application to stone, owing to the pores of the latter being usually filled with moisture.

Books Received.—"A Manual of Elementary Chemistry." By G. Fownes. Edited by Dr. H. Bence Jones and Professor Hofmann. Eighth Edition. London: J. Churchill, New Burlington Street. "A Manual of Photographic Chemistry." By T. F. Hardwich. Sixth Edition. London: J. Churchill, New Burlington Street. "The American Journal of Science and Art," No. 91.

THE ADULTERATION OF FOOD.

Now ready, price 6d., by post 7d.

A TABLE (reprinted from the Chemical News, and Revised by the Author,) showing the more Important Articles of FOOD or DRINK, and the Substances employed for Adulterating them. Especially suitable for Museums, Schools, Mechanics' and other Institutions, as well as for Private Reference.

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To the Medical Profeſſion of Great Britain and Ireland.

GENTLEMEN,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports,*" *vol.* 1, *p.* 317.

"Since this caſe came into my poſſeſſion, I have been led to make a good many clinical experiments, which have enabled "me to verify the reports which I had heard of the efficacy of JAMES'S POWDER, in ſometimes removing the APOPLECTIC "DIATHESIS in perſons advanced in life. It is, moreover, worthy of obſervation, that JAMES'S POWDER has been of "remarkable utility in certain inſtances of DETERMINATION OF BLOOD TO THE HEAD, which occurred at an early period of life, and "threatened to end by effuſion. Laſtly, in two caſes of GENERAL PLETHORA, in which, however, the head was more affected "than any other part, JAMES'S POWDER was exhibited by me with perfect ſucceſs."

Alſo *vol.* 1, *p.* 320.

"The following very ſimple method of exhibiting JAMES'S POWDER, in caſes of undue determination of blood to the "head, is that which I have generally purſued. The patient is made to begin with a very moderate doſe, not more than "two grains at bedtime, and to increaſe the doſe by half-a-grain every night, until ſome ſenſible effect is produced upon the "ſtomach, bowels, or ſkin. Should the ſtomach be affected with ſickneſs, the doſe muſt be leſſened by one grain on the "following night. By the addition of a little rhubarb to it, a larger quantity of JAMES'S POWDER may be adminiſtered "than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be "increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen "or twenty grains taken for a conſiderable period without any inconvenience."

The following extract (ſhowing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet,*" *June* 16, 1860, P. 590,) as follows:—

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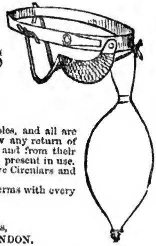

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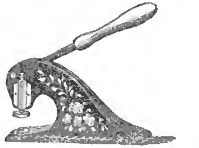

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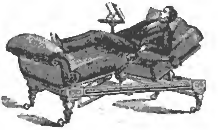
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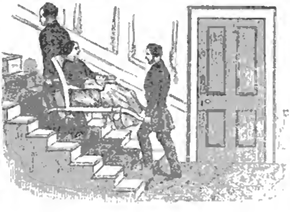

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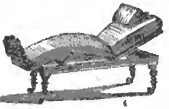
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Vol. III. No. 66.—March 9, 1861.

THE ADULTERATION OF FOOD AND DRINK.

Chemists have an interest in the Act passed for the Prevention of Adulteration of Food and Drink beyond that of any other portion of the public. They are liable to suffer by the fraudulent treatment of these substances just as much as other people, and it opens a number of appointments in the public service which are almost the only ones for which a chemist is eligible. Hitherto, neither chemists nor the public at large have benefited by the Act, a result we predicted when the Bill was first introduced into the House of Commons. As it appears to be the general impression that the able Member who introduced the Bill proposes to introduce another during the present Session of Parliament, we will take the Act as it stands, and offer a few remarks on the several clauses it comprises, and suggest some alterations which appear to us necessary to render it effective.

Clause I. gives Justices the power of inflicting a penalty not exceeding £5, and the costs attending his conviction, on any person who shall sell any article of food or drink adulterated with a substance injurious to health, or as being genuine when it contains a foreign substance not supposed to be injurious to health—provided the vendor is aware of the adulteration. For a second offence, the Justices may cause the name, address, and occupation of the offender to be published in any way they may consider desirable. This clause, which requires that the vendor shall be aware of the adulteration, appears to us to render the Act nugatory at the very outset. Who is to prove he had such knowledge if he denies that he had? The only person capable of furnishing such proof must be one of his own servants; and even supposing this evidence to be obtained, we doubt very much whether it would ensure a conviction, as Englishmen have, and rightly so, a horror of the system of delation; and this would be delation of the worst kind, short of compelling a woman to betray her husband. The sale of articles already adulterated by the wholesale dealer occurs with sufficient frequency to render it almost impossible for a Justice to convict in the face of the retailer's denial of a guilty knowledge, however strong his impression may be that the guilty party is before him. The remedy for this is obvious: let the vendor be convicted when the evidence is clear enough to establish the charge of adulteration, without entering into the question of his knowledge at all. At the same time, we would not prevent him from obtaining prompt justice whenever it happened that he was innocently condemned; and this is easily provided for by giving him the privilege of suing the wholesale dealer in the County Court for costs and damages. As to the mode of giving publicity to a second offence, we think it would be preferable to relieve the Justices of the odium they would incur by adopting what we conceive would be the only effectual one—viz., by compelling the offender to pay for an insertion of an advertisement of his offence in a newspaper, and to affix a printed copy of it on a conspicuous part of his shop window for a certain number of days,—by making it the legal punishment. Moreover, some such enactment will be found absolutely necessary, otherwise offending parties may complain that the penalties inflicted are of an arbitrary character; that whereas A was sentenced to pay a fine of £5, and to affix a printed account of his offence and conviction in his window for twenty days, and to pay for the insertion of an advertisement in a newspaper, B, for a precisely similar offence, only had to pay the fine and for the advertisement, and escaped the real penalty altogether.

Clauses. II and III. give certain bodies power to appoint Analysts possessing competent medical, chemical, and microscopical knowledge in their respective city, districts, counties, or boroughs, with such salary or allowances as they may think fit, subject to the approval of one of Her Majesty's principal Secretaries of State, whose sanction must also be obtained before an Analyst can be removed from his post. Also that the purchaser must prove to the satisfaction of the Justice who hears the case that he told the seller he meant to have the substance analysed, and gave him an opportunity of accompanying him (the purchaser) to see it placed in the hands of an Analyst, so as to secure it from being tampered with.

The grand defect here is that it simply permits vestries, district boards, &c., to appoint Analysts. The Act does not say to vestries, district boards, and so forth, "On and after a certain day you *shall* appoint an Analyst," &c., but you *may* appoint. The result of this mild species of legislation has been precisely what we anticipated when the Bill was first brought forward (see Chemical News, February 18, 1860), and we trust that Mr. Scholefield, or some other independent Member, will take an opportunity of moving the Government for a return of the number of appointments of Public Analysts that have been made. Of course we can only guess at the number of such appointments by what we have seen in different newspapers, but we have a strong impression that if the salvation of the population of the United Kingdom depended on twenty official Analysts being found within its borders, we should all be lost. The reason is clear enough. Vestrymen are, for the most part, tradesmen. Even where the Vestry contains members who are not traders, but men of independent fortune, inspired by a laudable desire to serve their fellow-parishioners in their local Parliament, by regulating the number of their gas lamps, examining the state of their roads, and watching over the salubrity of their domestic arrangements, these, being mostly dependent on the minor class of tradesmen for their re-election, are, naturally, reluctant to give offence to their constituents. If it were made compulsory on such bodies, we believe they would not be very unwilling to make the appointment; but it is, perhaps, rather too much to expect them to do so voluntarily.

As regards the salary to be paid to Analysts. This is a difficult question. If the efficient working of the Act is to be considered before all things, then the best method of accomplishing this will be to give the Analyst the services of a parish official to purchase as many specimens of food and drink as he may direct, and allow him to receive the fines. By this means the Analyst, as every chemist knows perfectly well, will be able to get through a great deal more work; as, instead of turning his attention from an analysis of coffee to one of tea, then to one of sugar, and then back again to coffee, he will deal with a good number of specimens of each of these substances in succession, and will act under the stimulus of greater pay for a greater amount of work. Such a method of remuneration would not be so agreeable to a gentleman as a fixed salary, but as the interests of the public are to be considered before the feelings of its servants, we think it would be the best to adopt, and would be by far the most effectual means of putting an end to adulteration. The municipal authorities ought, however, to guarantee him a minimum yearly salary for his services—that is to say, where the amount derived from fines is less than a sum agreed upon, the deficiency should be made up from the parish funds; because, under this method of working the Act, it is only reasonable to suppose that the practice of adulteration would be greatly checked, and, though he might perform as great a number of analyses as at the beginning, he would find fewer fraudulent specimens, so that his labour would remain the same while his reward would go on diminishing—at least, we confidently hope this would be the case.

It seems rather hard to require a tradesman to leave his shop and accompany a stranger a distance of, perhaps, two or three miles to prevent himself from being victimised; but, as the law stands at present, he must do so, no matter how convinced he may be of the genuineness of the article he has just sold, otherwise he would lay himself open to continual extortion. For example, a man comes into his shop and asks for a quarter of a pound of coffee, and when he has got it he tells him that he intends to have it analysed. The grocer is certain that the coffee does not contain any foreign substance, but the purchaser looks a rather doubtful character and may have some chicory in his pocket. The Analyst lives two or three miles away; nevertheless, he must put on his coat and go with the disreputable-looking stranger or else sends his shopman. But suppose the case of a shopkeeper in a poor neighbourhood who does not keep a shopman, what then? Must he put up his shutters and leave his business for an hour or two, or suffer the purchaser to add an ingredient to the coffee as he goes on his way to the Analyst, that he may return with the certificate which is to be the instrument of his conviction? The probable result of the announcement of the purchaser's intention would, in many instances, be the offer of a bribe to give up the specimens, which would be precisely what was wanted. We have not heard of a case of this kind, but, if none has happened, every Member of the House of Commons will admit that it is only because the idea has not yet occurred to that numerous class who live by their wits, to profit by the new opening which the Legislature has made for them.

We think it would be only just towards the shopkeeper to go a step further in legislation, and compel the purchaser, not only to give notice that he intended to have the substance analysed, but also to allow the vendor to seal it up, or secure it in any way he may think best calculated to prevent its being tampered with, before it was placed in the hands of the Analyst.

Want of space prevents us this week from passing in detail through the remaining clauses of the Act; but we intend to recur to the subject in an early number.

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On the Colours of Flames.

Merz has communicated some investigations on the colouration of the flames of Bunsen's burner and of hydrogen gas produced by the presence of various substances. These observations may be considered as supplementary to those of Bunsen, and, although less certain, are simpler, and more easily applied than the processes of the spectral analysis. The author employs a flame of pure hydrogen, as well as that of Bunsen's burner, and in addition, makes use of blue, violet, red and green glasses. The new substances, which he describes as giving characteristic colours to the flame of Bunsen's burner, are nitric, chromic, and molybdic acids, while phosphoric and sulphuric acids give a peculiar colouration to the dark core of the flame of hydrogen.

The flame of Bunsen's burner gives three sorts of colours—*a*. Border colours. These are of course peculiar only to the most volatile substances. To produce them, the loop of platinum wire is to be held outside of the flame, about one or two millimètres from the lower portion of the outer limit. *b*. Mantle colours—those namely which are seen when the substance is held in the bright blue-coloured mantle which forms the outer portion of the flame. *c*. Flame colours. To produce these, the loop is to be held horizontally, and in the hottest part of the mantle. The hydrogen flame yields another species of colour—viz., the core colours. These are produced only by sulphuric and phosphoric acids, which communicate respectively a blue and green tinge to the cold core of the hydrogen flame.

Nitric and nitrous acids give a bronze-green border colour, usually with an orange-coloured border. The test is to be previously dried in the flame, and dipped into a solution of bisulphate of potash, or into dilute muriatic acid, according as we wish to test for nitric or nitrous acid. The sensibility is about $\frac{1}{40}$ milligramme. Compounds of ammonia and cyanogen give the same bronze-green border, but more faintly.

Phosphoric acid gives a grey-yellow-green border colour, as well as a beautiful green core colour. The dry test is to be dipped into sulphuric acid and held in the flame in the manner above pointed out, in order to show the border colour. The sensitiveness is $\frac{1}{8000}$ milligramme. The green core colour is less sensitive, but indispensable in recognising phosphoric acid in the presence of large quantities of boric acid, and is produced by alternately moistening the test with a solution of fluosilicic acid, and holding it till ignition in the hydrogen flame, until the colour distinctly appears.

Sulphuric acid produces a beautiful blue core colour, being reduced to sulphurous acid. Free sulphuric acid gives the colour when the platinum loop is held in the border of the flame, but a sulphate must be held in the middle of the flame. In the latter case, it is well to dip the test into strong muriatic or fluosilicic acid. The sensibility for sulphuric acid is $\frac{1}{4000}$ milligramme, for a sulphate, $\frac{1}{1000}$ milligramme.

Boric acid gives a beautiful green mantle colour, which is so intense that the acid may be recognised in the presence of large quantities of phosphoric acid. The sensibility $\frac{1}{1400}$ milligramme. Borates are to be decomposed with sulphuric acid.

Chromic acid gives a dark brownish-red border colour, and a rose-red mantle colour. The sensibility is $\frac{1}{1000}$ milligramme. The dry test is to be moistened with concentrated sulphuric acid, and held in the border. Oxide of chromium gives no colour, and is to be first oxidised to chromic acid by moistening with a solution of hypochlorite of soda, and drying. Molybdic acid gives a yellowish-green flame colour, like that of baryta, which is however very fleeting, and only occurs on moistening the test with muriatic acid.

Muriatic acid gives a very weak greenish-blue border-colour, which lasts for a very short time, and therefore does not deserve attention. The acid is, however, decomposed into free chlorine which may easily be recognised.

Potash gives a grey-blue mantle-colour and a rose-violet flame colour. These colours appear reddish-violet through the blue glass, violet through a violet glass, and blue-green through a green glass. Potash is recognised in the presence of lithia, as well as through so thick a layer of blue glass that the lithium red is no longer visible. The test is to be moistened with sulphuric acid, and repeatedly exposed to the flame for a short time. The sensibility is $\frac{1}{31\cdot000}$ milligramme, for blue, $\frac{1}{15\cdot000}$ milligramme for green glass.

Soda gives an orange-yellow flame colour, which in very large quantities appears pure blue, but in small quantities is invisible through the blue glass; through the green glass, the soda flame appears orange yellow, even with the smallest quantities. This glass is particularly adapted to the recognition of soda in all its compounds. The sensibility is $\frac{1}{380\cdot000}$ milligramme. The test is to be moistened with sulphuric acid; dried and held in the hottest point of the flame.

Lithia gives a carmine-red flame colour, which appears violet-red through the blue, carmine-red through the violet, but is invisible through the green glass. The test is to be moistened with sulphuric acid and treated like potash. The sensibility is $\frac{1}{140\cdot000}$ milligramme. In the presence of soda, lithia is recognised through the blue glass; in the presence of potash, by the method given by Bunsen. In the case of the alkaline earths, the test is to be moistened with sulphuric acid, carefully dried, and held in the hottest point of the mantle. After all the alkalies have evaporated, the earths may be detected.

Baryta may be recognised by the yellowish-green flame colour which appears blue-green through the green glass. The sensibility for baryta alone is $\frac{1}{8500}$ milligramme. If the green disappears and a red flame colour makes its appearance, the test is to be repeatedly moistened with muriatic acid, and immediately introduced while wet into the hottest part of the flame. If the blue-green colour is no longer seen, we proceed to examine for lime.

Lime is present when the red flame colour, on evaporating the last portion of muriatic acid, appears siskin-green through the green glass. Strontia gives in this case a weak yellow. The sensibility of lime alone is $\frac{1}{1000}$ milligramme, but by employing the green glass, $\frac{1}{15\cdot000}$ milligramme.

Strontia may be recognised by the purple or rose colour which is seen through the blue glass when the test, after moistening with muriatic acid, is evaporated to dryness in the flame.

Copper as chloride gives an azure-blue zone, and as nitrate, a pure green flame colour. By the combined observation of both colours, copper may be distinguished from all other metals which give similar colours. The sensibility of copper alone is $\frac{1}{8\cdot000}$ milligramme. The other flame-colouring metals, such as arsenic, antimony, tin, lead, mercury and zinc, exhibit, especially in the form of chlorides, more or less intense bluish or greenish mantle-colours, which, however, cannot be advantageously used as reactions for the metals themselves.—*Journal fur prakt. Chemie*, No. xvi. 1860.

Researches on Arsenic, by M. LIPPERT.

THIS inquiry relates to Reinsch's process, which consists, as is well known, in extracting arsenic from its hydrochloric solution, by means of metallic copper. The black deposit which results is usually regarded as pure arsenic. Now, M. Lippert has shown, by painstaking researches, that this deposit contains only 32 per cent. of arsenic, the remainder—that is to say, 68 per cent.—being copper. This is a true alloy, of a definite and constant composition, according to the formula Cu_5As.

When heated in a hydrogen current, this black powder loses a little arsenic, and is transformed into a white, silvery alloy, oxidising slightly in the air, agreeing in its composition, Cu_6As, with *Domeykite*, a mineral found at Copiabo. The great sensitiveness of Reinsch's process arises, then, from the large proportion of copper allied with the deposited arsenic. Once aware of this fact, we can employ this process without inconvenience; for henceforth we shall know that all the arsenic will not be volatilised under the influence of the hydrogen current, and that at least half will remain in the deposit. —*Journal fur prakt. Chem.*

On the Action of Oxide of Bismuth on Solutions of Sesquioxides, by M. LEBAIGUE.

WHEN a solution of a sesquioxide, such as alum, sesquioxide of iron, or chromium, is boiled for a short time—about a quarter of an hour—with excess of oxide of bismuth, anhydrous or hydrated, the following facts are observed:—

The liquid, acid before the reaction, becomes neutral. The sesquioxide is entirely precipitated. The acid with which it was combined forms an insoluble sub-salt, with the oxide of bismuth. The liquid retains no trace of either metal. Thus, when perchloride of iron is used, sensitive tests employed to detect a persalt of iron in a liquid give evidence of no reaction.

This fact, established as respects the salts of sesquioxide of aluminium, iron, and chromium, cannot be shown in the salts of sesquioxide of manganese, on account of the difficulty in preparing them; but there is every reason to believe that the reaction would be the same.

Nothing resembling this reaction takes place when solutions of salts of zinc, protoxide of iron, nickel, cobalt, copper, and lead are operated upon. My researches only justify me in stating that, in operating on rose-coloured salts of manganese, there is sometimes precipitated a brown powder, which apparently consists of sesquioxide of manganese, but the liquid remains rose-coloured. This fact verifies the opinion that the red colour is not due to the presence of a sesquioxide. That in experimenting on a solution of commercial sulphate of copper, there is precipitated a certain quantity of sesquioxide of iron, proceeding from the sulphate of iron, always contained in this sulphate of copper. That when an imperfectly-peroxidised salt of iron is operated upon, the peroxide of iron only is precipitated, the proto-salt remaining in solution. Oxide of bismuth then allows the separation of sesquioxide and protoxide salts in metallic solutions containing a mixture of the two. On this account, oxide of bismuth is nearly allied to the oxides of copper and lead, which are employed in analyses to effect the separation of iron and manganese.

If we would deduce some considerations from the foregoing facts with reference to chemical statics, we must remember that, though in the tables of affinities of oxides and acids, as arranged by some authors, they have reserved the last place for bismuth, yet the result of the experiments just recorded show that oxide of bismuth is a more energetic base than is generally supposed, and that it should be placed in the scale of affinity of bases for acids near to copper and lead.—*Journal de Pharmacie et de Chimie.*

Researches on the Platinum Metals, by WOLCOTT GIBBS, *M.D., Professor of Chemistry and Physics in the Free Academy, New York.*

(Continued from page 131.)

Experiments which I have instituted upon the effects of fusing the ore with steel, phosphorus, arsenic, and sodium, led to no really valuable practical results. It is true that an alloy with steel can be obtained by fusion at a high temperature. Acids slowly dissolve the iron in this alloy, and leave the platinum metals in the form of a black powder which is attacked by nitro-muriatic acid, though not completely dissolved. But the process is tedious, and the results not very satisfactory.[1]

Wöhler's[2] method of resolving osmiridium, consists in passing moist chlorine over the ore mixed with common salt and heated to low redness in a glass or porcelain tube. This method is invaluable in analysis, and gives excellent results in working the ore upon a small scale. In all cases, however, several repetitions of the process are necessary for complete resolution or reduction to a soluble form. On the other hand, it can scarcely be doubted that this method could be advantageously employed upon the large scale, if vessels of porcelain of large size and of a proper shape could be obtained. Such vessels might be constructed in the form of long and flattened ellipsoids, furnished at each extremity with wide tubes several inches in length, and would be of great utility in various chemical processes. No process of fusion with oxidising agents compares with Wöhler's method in point of elegance, as no iron or other impurities afterward to be removed, are introduced by the process itself.

Fritzsche and Struve[3] treat the ore with a mixture of equal parts of hydrate and chlorate of potash, by which a more or less complete oxidation is effected, without any sensible evolution of osmic acid. The temperature required in this process is not high, but large vessels must be employed, as the mixture froths very much at first. The process in question has not appeared to me to possess any sensible advantage over that of Claus, which is moreover less expensive, and can be carried out with smaller vessels.

Claus'[4] method of resolving the ore consists in fusing for an hour, at a red heat, a mixture of one part of ore with one part of caustic potash and two of saltpetre. The fused mass is to be poured out upon a stone, allowed to cool, broken into small pieces or powdered, and then introduced into a flask, which is to be filled with cold water and allowed to stand for twenty-four hours. The clear deep orange-red solution of osmate and ruthenate of potash is then to be drawn off by means of a syphon, and the black mass remaining again washed in the same manner. The finely-divided oxidised portion of the insoluble matter may now be separated from the unattacked ore by diffusion in water and pouring off, after the subsidence of the heavier ore. The unattacked ore is then to be fused a second time with potash and saltpetre and treated as before. Claus asserts that he has been able in this manner to resolve the Siberian osmiridium completely in two operations.

I have not always been so fortunate with the raw California ore, even when in a finely-divided state. On the contrary, after three or four successive fusions, there usually remained a large quantity of black matter, insoluble in aqua regia. In one operation with 500 grammes of ore, only 200 grammes were rendered soluble by two fusions with potash and saltpetre. I employ at present the method of Claus, with several modifications which I deem essential. The ore, which is usually very impure, is in the first place to be fused with three times its weight of dry carbonate of soda. The fused mass after cooling is to be treated with hot water, to remove all the soluble portions, and then the lighter portions are to be separated by washing from the heavy unattacked ore. In this manner the greater part of the silica and other impurities present may be removed. A previous purification of this kind is not indispensable, and may be omitted altogether when the ore is in plates or large grains, but it is very desirable when the ore is in fine powder, and greatly facilitates the subsequent action of the oxidising mixture. Claus recommends the purification of the ore by boiling with a solution of caustic potash. It is certain that a much larger portion of the ore is resolved by two successive fusions with potash and nitre after previous purification by fusion with soda and washing. By cutting off the top of a mercury bottle a wrought iron crucible is obtained, in which 600 grammes of osmiridium may be fused at one operation with potash and saltpetre as above. There is usually little or no foaming, and if any occur it may easily be checked by stirring with an iron rod. No sensible quantity of osmic acid is given off during the process, which with a little care is entirely free from danger. In this manner, I have worked up 1500 grammes of ore in a few hours in three successive operations.

Claus' method of treating the fused mass to separate ruthenium and osmium is liable to two sources of inconvenience. In the first place, the quantity of water required to dissolve out the soluble portions is very large, and the subsequent treatment of such bulky solutions by distillation with acids, tedious—very large retorts being necessary. In the next place, it is impossible in this way to avoid exposure to the vapour of osmic acid, especially in transferring the solutions from one vessel to another. I therefore prefer the following process, which leaves nothing to be desired in point of safety or convenience. The fused mass is to be broken into pieces with a hammer, and brought into a clean iron pot—a common skillet, with a long handle, answers this purpose extremely well. Boiling water, containing about one-tenth of its volume of strong alcohol, is then to be added, and the whole is to be boiled over an open fire until the fused mass is completely disintegrated. The osmate of potash is, in this manner, reduced to osmite, $KOsO_4$, while the ruthenate of potash is completely decomposed, the ruthenium being precipitated as a black powder—probably a mixture of RuO_2 and Ru_2O_3, or of the hydrates of these oxides. It is advantageous, after boiling for some time, to pour off the supernatant liquid with the lighter portions of the oxides, and boil a second time with a fresh mixture of alcohol and water. In this manner we obtain a solution of osmite of potash, a large quantity of black oxides, and a heavy black and coarse powder. This last consists chiefly of undecomposed ore, mixed with a small quantity of the oxides of iridium, &c., with scales of oxide of iron from the crucible, and, if the ore has not been previously purified, with the impurities of the ore itself. The greater specific gravity of this residual mass renders it very easy to pour off from it the mixture of black oxides with the solution of osmite of potash and alkaline salts. This solution with the suspended powder is to be poured into a beaker and allowed to settle. The heavy black powder remaining in the iron pot is then to be perfectly dried over the fire, and fused a second time with potash and saltpetre as before. The fused mass is to be treated exactly as after the first fusion. The heavy portions remaining after this operation may be fused a third time with the oxidising mixture. When, however, the ore has been previously purified by fusion with carbonate of soda, or when it was originally in the form of clean scales, the heavy portion remaining after two successive oxidations will be found to consist chiefly of scales of oxide of iron.

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[1] Since the above was written the elaborate memoir of Deville and Debray on the platinum metals has appeared. (*Ann. de Chimie et de Physique*, 3d, lvi. 385.) I have not however been able to repeat any of the processes for subdividing or oxidizing osmiridium given by these chemists but consider it probable that the subdivision of the ore accomplished by fusion with zinc may be simpler than that which I have described above.

[2] *Pogg. Annalen*, xxxi. 161.

[3] *Journal für prakt. Chemie*, xxxvii. p. 483.

[4] "Beiträge zur Chemie der Platin-metalle." Dorpat, 1854.

The solutions containing osmite of potash and alkaline salts are to be carefully drawn off by a siphon from the black oxides which have settled to the bottom of the containing vessels. The oxides may then be washed with hot water containing a little alcohol, and introduced into a capacious retort. By this process, when carefully executed, no trace of osmic acid escapes—an advantage not to be despised, as the deleterious effects of this body upon the lungs have not been exaggerated, and too much care cannot be taken to avoid inhaling it.

The solution of alkaline salts contains only a portion of the osmium in the ore. The other portion exists in the mixture of oxide, and must be separated by distillation. For this purpose the retort should be provided with a safety-tube, passing through the tubulure, and with a receiver kept cold, and connected by a wide-bent tube with a series of two or three two-necked bottles containing a strong solution of caustic potash with a little alcohol, and also kept cold. All the tubulures and connections must be made perfectly tight. Strong chlorhydric acid is then to be cautiously poured into the retort, through the safety-tube, in small portions at a time. The reaction which ensues is often violent; great heat is evolved, and a portion of the osmic acid distils over immediately, and condenses in the receiver in the form of colourless needles. When a large excess of acid has been added, the action has entirely ceased, and the retort has become cold, heat may be applied by means of a sand bath. The osmic acid gradually distils over, and condenses in the receiver and in the two-necked bottles. Especial care must be taken that the beak of the retort is not too small at the extremity, as it may otherwise become completely stopped up with the condensed osmic acid. The same applies to the tubes connecting the receivers and two-necked bottles. The distillation should be continued for some time after osmic acid ceases to appear in the neck of the retort; when this has once become hot, the acid condenses, and passes into the receiver in the form of oily drops.

When the distillation is finished, the retort is to be allowed to cool, and then separated from the receiver, which is to be immediately closed with a cork. By gently heating the receiver in a water-bath, the contained osmic acid may be driven over into the two-necked bottles, where it condenses in the alkaline solution, and is reduced by the alcohol to osmite of potash. The solution thus obtained may be added to that obtained directly from the fused mass of ore, and on evaporation in a water-bath and cooling, will yield crystals of osmite of potash, the salt being but slightly soluble in strong saline solutions. The mother liquor from the crystals contains only traces of osmium, and may be thrown away as worthless.

The dissolved portions drawn off from the retort have a very dark brown-red colour. The solution is to be evaporated to dryness, re-dissolved in hot water and again evaporated, after adding a little chlorydric acid, and this process repeated till no smell of osmic acid can be perceived. A cold and saturated solution of chloride of potassium is then to be added in large excess. This dissolves the chlorides of iron and palladium which may be present, leaving platinum, iridium, rhodium, and ruthenium as double chlorides, insoluble in a strong solution of the alkaline chloride.

The undissolved mass is to be well washed with a saturated solution of chloride of potassium, which, for reasons hereafter to be mentioned, is preferable to sal ammoniac. In this manner nearly the whole of the iron and palladium may be removed, while any insoluble impurities contained in the ore remain with the mixed double chlorides.

(To be continued.)

TECHNICAL CHEMISTRY.

On the Alloys of Copper and Zinc,

by Frank H. Storer.

(*Continued from page 72.*)

I have determined the quantity of this impurity in some fifty or sixty instances: it was found to vary from 1·5 per cent. or less to 4 or even 5 per cent., in exceptional cases, of the copper dried at 100°; ordinarily, it was from 2 to 2·5 per cent. These remarks refer to copper precipitated from sulphuric acid solutions, which is usually in a porous, spongy state. When precipitated from chlorhydric acid solutions it is usually crystalline and compact, and, doubtless, contains less impurity. It is said, however, that a certain amount of loss will invariably occur, even when the best samples of copper precipitated by iron are heated in an atmosphere of hydrogen. Even in copper precipitated by pure zinc I have observed that an appreciable loss occurs,—varying in my experiments from 0·5 to 2 per cent of the dry copper,—when it is ignited in hydrogen gas. It would appear that, while copper may be determined with great accuracy when precipitated by pure zinc in a platinum vessel, as recommended by Fresenius,[1] even when the precipitate is dried only at 100°, for in this case the copper is deposited in a condition so compact that little or no impurity can adhere to, or oxide form upon it, yet it can hardly be determined with nicety when precipitated in the spongy state, unless it be ignited in a current of hydrogen, or converted into oxide before weighing; it is necessary also to exercise the greatest care that none of the smaller particles of copper are lost during the washing, for many samples of copper, when precipitated as sponge by zinc, are peculiarly liable to this accident.

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[1] "Anleitung zur, Quantitative Chemischen Analyse" (Braunschweig, 1858), s. 247.

In regard to the assay which I have attempted to describe, I would remark, that I have used iron instead of zinc,[2] simply because I was, at the time, unable to procure a sufficient quantity of the latter in a state pure enough for analytical purposes. The assay by iron, as I have used it, is clearly not an accurate analytical process. but, on the other hand, it is in several respects preferable, as an assay, to the method by zinc, and will be found, after a little practice, to yield satisfactory results. It is, doubtless, as good a method as any which we possess, wherever absolute accuracy is not required, and where a large number of determinations must be made. Admitting that the results obtained by it may vary among themselves within the limits of one per cent., or, in exceptional cases, even to the extent of one and a-half per cent., I have still deemed it sufficiently accurate for the work to which I have applied it; for it must be remembered that these differences are much smaller than the variations in composition which may occur in different parts of the same cup of alloy, from partial volatilisation of the zinc, or from incomplete mixture of the metals of which it is composed.

I may also remark that, although this assay was not adopted by myself until after thorough trial of several other processes which seemed to be applicable to the special case with which I was occupied, I now consider it a very valuable method.

It deserves mention in this connection, that the assay by iron has been entirely misunderstood by some writers. Thus, Mitchell[3] directs that the whole of the iron added shall be dissolved in chlorhydric acid, just as if it were zinc, after the copper has been completely precipitated; the absurdity of which procedure is manifest.

In concluding the subject, I may observe that American assayers are accustomed in practice to use a mixture of nitric and chlorhydric acids as a solvent in the first instance of the sample to be assayed, and to expel the nitric acid by evaporating twice to dryness with an excess of chlorhydric acid. The copper being then precipitated from the hot chlorhydric solution. Some of them collect the copper upon a filter, instead of washing by decantation, and rinse it once or twice with very dilute chlorhydric acid before washing with water.

This process has the advantage, that a larger portion of the impurity of the iron is soluble in chlorhydric than in sulphuric acid, hence there is less chance of the copper being contaminated by it; basic iron salts are also less liable to form: the evaporations, which are conducted over the sand-bath, consume somewhat less time than when sulphuric acid is used; and the copper, being precipitated in a crystalline state, is easily washed without loss, and is not liable to be attacked by the acid solution.[4]

On the other hand, in the process by sulphuric acid,—which I have used merely in order to remove the lead which is contained in the alloys,—the last traces of copper seem to be somewhat more readily precipitated from the solution, and the accident of the copper adhering too closely to the iron occurs, perhaps, more rarely.

Of the special precautions which must be taken when this assay is applied to the various impure ores of copper, I cannot here speak. Many of them have been already described by Kerl.[5] I would, however, mention the fact, that, in cases where arsenic is the only impurity, it has been found advantageous in practice to precipitate it upon iron with the copper, and to expel it subsequently when the latter is heated in the atmosphere of hydrogen.

It is a well-known fact, that the combination of copper with zinc is attended with ebullition of considerable violence, so that portions of the melted mass are often thrown to a distance of several feet from the crucible. Yet it does not appear to have been previously noticed by chemists that this action is much more energetic while the first portions of zinc are being added to the copper, and that the loss of zinc by volatilisation is far greater at this time than at any subsequent stage of the operation; indeed, when the alloy has become somewhat rich in zinc, it takes up an additional portion so quietly that scarcely any action is to be observed. The fact is, however, well known to brass-founders, who are in the habit of first melting a quantity of old brass with the copper in their crucibles before adding any zinc as such.

A very violent action will also occur if, after allowing the zinc to melt and remain floating above the copper, as it will do if the mass is not agitated after each addition of zinc, the two metals are suddenly stirred together. This action is apparently analogous to that observed by Levol,[6] which occurs when layers of melted silver and gold are mixed. Levol ascribes the violent ebullition which ensues when these metals are stirred together to the escape of oxygen which had been absorbed by the molten silver, and which would be expelled as soon as this metal unites with the gold. This hypothesis, however, does not, in my opinion, satisfactorily explain the phenomena which occur in the case of copper and zinc. Another explanation is afforded by the experiments of Person,[7] who has shown that in the formation of certain alloys a large amount of latent heat is evolved.

The apparent energy with which the combination of copper and zinc occurs is often mentioned in treatises on chemistry, as if it indicated the existence of strong chemical affinity between the two metals. Yet it is easy to separate the zinc completely from any of these alloys by heat. Indeed, Bobierre[8] has devised a method for their quantitative analysis, by heating the sample of alloy in a porcelain tube, through which a current of hydrogen is made to flow.

In preparing the alloys, it may readily happen, if the mass be not very frequently stirred, that a small portion of the copper, or of an alloy rich in copper, may become chilled and solidify at the bottom of the crucible, while a quantity of easily-fusible alloy, rich in zinc, has formed and remains liquid above it. Accidents of this nature happened to myself very frequently during the earlier part of the research. They occasioned no inconsiderable loss of time, since it was in every instance necessary to prepare a new sample of the alloy on account of the enormous waste of zinc which would occur if one attempted to re-melt the chilled *culot*; and were especially vexatious, from seeming to indicate the existence of definite alloys, having little or no affinity for each other. Similar accidents sometimes occur in brass founderies, but are evidently less liable to take place here, where the amount of melted metal is large and the heat well regulated, than in the small crucibles and furnaces of the chemical laboratory. It is worthy of remark that many brass-founders refer them to some peculiarity of the particular sample of copper used. If any trouble of this kind is experienced, a portion of common salt is usually thrown into the crucible, it being regarded as a remedy. It is very probable that the presence of some foreign metal may prevent or retard combination; on the other hand, copper which still contains a portion of sulphur is thought to combine with zinc with peculiar facility. In my own experiments, these occurrences appeared to depend entirely upon irregular heating of the furnace, or upon insufficient stirring. D. Forbes[9] has analysed such specimens which were produced accidentally in the ordinary process of brass-making. They consisted of a white alloy, containing 46·51 per cent. of copper, and of a yellow alloy, containing 56·91 per cent. of copper. From my own experience, I am satisfied that layers containing almost any proportion of the two metals may form.

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[2] Method proposed by Vauquelin, *Annales de Chimie* (1798), xxviii. 50.

[3] "Manual of Pract. Assaying" (London: Baillière, 1854), p. 257.

[4] The assay undoubtedly affords more accurate results with chlorhydric acid than when sulphuric acid is used. Mr. Dickinson assures me, that in his hands variations so great as 0·5 per cent. are entirely exceptional, the usual difference between any two assays of a homogeneous sample of ore not being larger than 0·2 to 0·3 per cent.

[5] *Op. cit.*, s. 219.

[6] *Ann. Ch. et Phys.* (3), xxxix. 168.

[7] *Ibid.*, xxiv. 146.

[8] *Thèses*, p. 57, also, *Compt.-Rend.*, xxxvi. 224.

It is, doubtless, this tendency of the metals to remain unmixed in separate layers, when not subjected to agitation, which has led several chemists to believe that the alloys of copper and zinc are apt to separate, by eliquation, into two portions, respectively rich in copper and in zinc; from which they have inferred, as I have previously remarked, the existence of definite compounds. Although in my own experiments I have been unable to detect any eliquation, I would by no means assert that a certain amount of such separation may not, under some circumstances, occur. The method of analysis which I have adopted is not delicate enough, nor will the very conditions of the case itself admit of an accurate determination of this point. My results are, however, as I think, sufficient to show that any tendency towards eliquation which may exist must be very slight, although they do not prove that a small amount of it may not be produced by gravity, or by some other force acting against the comparatively feeble affinity of the metals for each other. Experiments upon this subject, which have been recorded by previous observers, have evidently been exposed either to the error of insufficient mixing in the first instance, or to the loss of zinc by volatilisation from those portions of the alloy which were in contact with the air, or to both of these causes. Some of them appear to have also been influenced by the presence of foreign metals in the alloy.

(To be continued.)

On the Combustion of Wet Fuel in Furnaces, by B. SILLIMAN, *Jun., Prof. Gen. and App. Chem. in Yale College*.[10]

In all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air-dried wood still contains about 25 per cent. of water, according to the experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels but mineral coal and charcoal. But, technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, begasse, and dye stuffs, contain at least one-half and often more than half of their weight of water. Until a recent period, the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which, by a modification in the form of the furnace, the combustion of wet fuel is not only rendered consistent with the best economical results, but which, as it involves chemical reactions never before, it is believed, successfully applied for such purposes, is deserving of particular notice, from a scientific as well as from a practical point of view.

It is a well-established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures, is so strong, that if oxygen is not present in a free state, any compound containing oxygen which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the blast-furnace, where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke, or volatilised carbon, and oxide of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products, in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and, by the furnace now under consideration, this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion, drawn from the decomposition of the vapour of water at a high temperature by its reaction with free carbon and the oxide of carbon.

The practical solution of this problem was first successfully accomplished, as appears from a decision of Patent Commissioner Holt, by the late Moses Thompson, in 1854. The controversial questions growing out of this invention are entirely foreign to our present purpose, and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention, another instance of a truth already so often signalised in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually "consume each other." This notion was realised, and the reaction secured between the elements of water and the carbon of smoke, or the oxide of carbon, in a part of the furnace called by the inventor the "mixing-chamber."

Wherever that place may be situated, or however constructed, the one essential thing about it is, that it should be a very hot place, and one to which the atmospheric air can have no direct access until it has passed by and through the burning fuel. It is, in fact, a retort, or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement, greater or less, of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing-chamber, then, the important chemical reaction before insisted on must be set up. The vapour of water is decomposed, furnishing its oxygen to the highly-heated carbon to form carbonic acid, while the oxide of carbon is, in like manner, exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas, or light carburetted hydrogen. The vapour of water is thus made to give up, not only its constituent elements, to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. Moreover as all these products of combustion and of chemical reaction pass together over the bridge-wall of the furnace into a space from which atmospheric air is not excluded, it then and there happens that any free hydrogen, light carburetted hydrogen, or oxide of carbon, which have previously escaped combustion, take fire and burn, yielding up their quota of heat to the general aggregate.

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[9] "Report of Twenty-fourth (Liverpool) Meeting of the British Association for Advancement of Science," 1854, p. 67. See also Liebig and Kopp's *Jahresbericht*.

[10] From the *American Journal of Science*, Vol. xxx. No. 89.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire-bricks will endure it, and the colour seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated, it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion, and soon result in total suspension of the peculiar energy of this furnace. The air containing only one-fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so large a proportion of cold air,—four-fifths of which are not only indifferent, but positively prejudicial, from the quantity of heat it absorbs,—it happens that the temperature of the mixing-chamber is rapidly reduced below the point at which carbon can decompose vapour of water, and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy, and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapour. When in proper action, not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and, what is more remarkable, the reactions are so evenly balanced that no wreaths of watery vapour are observed, while in the earlier stages of combustion, before the proper temperature in the mixing-chamber is reached, both these products are seen in great abundance.

Here let the inventor speak for himself in the language of the patent:—

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such manner as to form a kind of dome with a sufficient opening at the top to feed the bagasse. The outer walls of these furnaces should be from twenty-four to thirty inches thick and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be of fire brick. The main chamber of each furnace should be divided into two parts—upper and lower—by a fire brick grate about one-fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

"In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

"Extending across the back of both furnaces, and opening into both by flues is a mixing chamber into which all the gases from both furnaces enter in a highly heated state and mix and consume each other on their way to the boiler and stack. This chamber should be about one-half the capacity of all the fire chambers and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

"The feed openings at the top of the furnaces should be closed by doors which open inwards by the weight of the feed, but are self-closing, and do not yield to pressure from within.

"The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions, the corrugations extending down to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smoothed the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat and downward passage of the vapours.

"These corrugations are unnecessary in burning tan and sawdust.

"The spaces between the grate bars for burning bagasse should be about six inches wide for the finest grinding and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to ¾ of an inch. The grate should be made of fire brick.

"The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the begasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred and will fall through the grate prepared as above unto the fire chamber and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber and the wet fuel being from time to time supplied will furnish in a highly heated state aqueous vapours which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion of all the combustible gases issuing from the fire chamber. If by accident the fire in the lower part of the furnace should predominate, the draught should be diminished and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down the draught should be let on, and any deficiency of dry fuel should be supplied to the fire chamber. Under proper management little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open grate will supply its place; and the caloric thus produced by the combustion of wet fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber and in the mixing chamber under intense heat the carbonaceous gases will decompose the steam from the wet fuel and effect complete combustion.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 1, 1861.

On Bunsen and Kirchhoff's Spectrum Observations, by Professor H. E. Roscoe.

The subject which I have selected for this evening's discourse—namely, Bunsen and Kirchhoff's Spectrum Observations—may not convey to the minds of those who are unacquainted with the facts which I shall have the honour to bring before you any idea of the vast magnitude and importance of the subject, and the interest which attaches to it. The attention becomes, however, aroused when we learn that these discoveries of Bunsen and Kirchhoff mark a new era in the science of analytical chemistry. By it we are enabled to obtain a knowledge of the composition of the earth's crust with a degree of delicacy and accuracy of which we had before no idea. With this chemical elements supposed to be most rare are found to be more widely distributed; and on the very first practical application of this method of analysis, a new and hitherto undetected alkaline metal has been discovered. The importance of these researches becomes, however, still more strikingly evident when we hear that the conclusions which are drawn therefrom outstep the bounds of our planet, enabling us to determine with all the accuracy and certainty of exact experiment the presence of certain substances well known on this earth—for instance, iron, sodium, potassium—the presence of these bodies in the sun.

These few words will give some slight idea of the magnitude of the subject, and I think I shall not be misunderstood when I say I feel some degree of diffidence in appearing here to-night in the place of those much better known to you, and who are far more qualified than I am to explain these remarkable results of experimental science.

It would be idle in me to attempt any eulogium or panegyric upon such men as Bunsen and Kirchhoff, but it falls upon me to endeavour to lay before you as clearly and precisely as I am able, the results of their discoveries, trusting to you to draw your own conclusions with regard to the value of such results and the character and ability of men able to produce such discoveries.

The colours which certain bodies impart to a flame have long been used as a test of the presence of such bodies. Thus, for instance, soda imparts a yellow colour to the flame, potash a violet colour, lithium a bright crimson, barium a green, and so on. The colours which these substances produce will be seen when I place on these colourless gas flames small quantities of the substances; I put soda on and you have the yellow flame; here is the purple flame from potash, the red or crimson from lithium, the green from barium, the crimson from strontium, and the yellowish-red flame from lime. The reason why these bodies produce these peculiar colours in flame is that they are volatile—that they volatilise at the temperature of the flame, and that at this high temperature the vapour becomes luminous and gives off this peculiar kind of light. Now, we can obtain the same effect, if we volatalise the salt in any other way. I have here some gun-paper steeped in the solution of these various salts. By lighting it we again get the yellow flame from soda, the purple from potash, and so with the others. It is only because these substances which we place in the flame are volatile at this temperature that we obtain any light at all. If we place a piece of platinum-wire in this flame, the platinum, not volatilizing, gives us no colour, it becomes white-hot itself, but it does not impart to the flame any of the peculiar colours we noticed before, for the reason that platinum is not volatile at that temperature. These facts, I say, have long been known and observed; but it was reserved for Bunsen and Kirchhoff to place these beautiful phenomena in their true light; to apply to them the modern methods of exact research, and thus to open a new and vast field for most important investigation. This they accomplished in a most simple and beautiful manner,—by examining the coloured flames by means of a prism, and thus splitting or decomposing the light produced by the incandescent vapour of the different metals. If we pass white sunlight through a prism, we see the well-known solar spectrum discovered by Newton. On this diagram we have an illustration of the solar spectrum,—a very rough one, but still showing what we want to see. It is a representation of what we see if we pass a white solar ray through a prism,—we find the spectrum changing in its gradations of colour, from the red, the least refrangible ray, noticing on our way certain dark lines, spaces, or shadows, which exist in the sun-light, and which we always find there,—lines in which we should have a considerable deal to do, until we come to the most refrangible ray, to the violet. Now, if instead of passing white sun-light through the prism, we pass the yellow light which we obtain from the soda flame, in the same manner as we passed the solar light, we get the soda spectrum. In this we find that instead of all the colours of the rainbow, as in the solar spectrum, the soda spectrum consists of only one bright yellow line. This shows us that there is only this particular kind of light present in the soda—that this yellow flame of soda only contains light of one kind. In the same way we find that when we examine the flames produced by the other metals which I have mentioned, not with the naked eye, but with the prism, we obtain spectra from all of them, each giving its peculiar system of lines. I have on a diagram the representation of the spectra produced by these various substances. The arrangement by which these are obtained is a very simple one,—it is obtained by an instrument which I have on the table before me [referring to the instrument]. It consists of a prism encased in a dark box, and on each side of the prism there is placed a telescope. The eyepiece of this first telescope is removed, and in its place is inserted a slit so arranged that it coincides with the focus of the object-glass. The coloured flame is placed in front of the slit, and by looking through the other telescope, and adjusting the prism, we can see the spectrum of the metallic salt placed in the flame. For instance, when I place this soda flame before the slit, and look through this telescope, I see the beautiful yellow line of sodium; there is nothing but a yellow line, all the rest is dark.

Now, in order to begin to illustrate the use of this mode of using flame, I have here a mixture of a salt of potassium and sodium, which contains twenty parts of potassium and one part of sodium; and yet you will see that you can scarcely recognise the presence of any potash at all in the flame. So intensely yellow is the soda that when we have only one-twentieth part of it, the potash is almost invisible. But if we look through a prism at the flame we then see the spectra of the sodium and the potassium perfectly distinct and separate. The sodium is confined to its own particular sphere,—we see only the one bright yellow line which it gives,—and we see the potassium spectrum as brightly as before. I will throw on the screen a representation of what we really see when we look through the prism. You there see that when the soda line appears the potassium lines remain as distinct as they were before, the soda being confined to its own limits. Now, this holds good with mixtures of all these metallic salts, for in no one case does the spectrum of one interfere with that of the other. These particular lines you see here, which are characteristic of the metal, are brought about by no other substances that we are acquainted with. The presence of that yellow line is an absolutely certain indication of the presence of sodium, the red line of the presence of lithium, and so on. We may, therefore, rely on these appearances as being certain indications of the presence of these substances.

Now, in order to give some opinion of the value of this kind of analysis, I would tell you that the various processes which the chemist must pursue in the ordinary way in order only to detect the presence—not estimate the quantity, but simply to detect the presence—of these five or six alkaline metals when they are mixed together, occupies from two to three hours. And, what is more important, the results by these means are never absolutely certain, especially if there is only a small quantity of a substance present. As a contrast to this I will quote the following. M. Bunsen in his most interesting paper, said:—"A mixture of the chlorides of sodium, potassium, lithium, calcium, strontium, and barium, containing at most one ten-thousandth part of a grain of each of these salts, was brought into the flame, and the spectra produced observed. At first, the bright yellow sodium line Naα appeared with a back ground formed by a nearly continuous pale spectrum. As soon as this line began to fade, the exactly defined bright red line of lithium Liα was seen; and still further removed from the sodium line, the faint red potassium line Kα was noticed, whilst the two barium lines Baα, Baβ, with their peculiar form, became visible in the proper position. As the potassium, sodium, lithium, and barium salts volatilised, their spectra became fainter and fainter, and their peculiar bands one after the other vanished, until, after the lapse of a few minutes, the lines Caα, Caβ, Srα, Srβ, Srγ, and Srδ, became gradually visible, and, like a dissolving view, at last attained their characteristic distinctness, colouring and position, and then after some time became pale and disappeared entirely."

Thus we get, with absolute certainty, in half a minute, what, according to the old method, was obtained somewhat doubtfully after three hours' hard work. [The Lecturer proceeded to illustrate the phenomena described by Bunsen, by the aid of the oxyhydrogen lime light, running through the description of the phenomena again.] But this application which I have mentioned is, however, by no means all that this method can accomplish; it cannot only do that speedily and easily, which we find a difficulty with other methods to perform, but it can do for us that which the old method never could. We can detect by this method quantities of the substances the smallness of which is perfectly astounding. Choosing from a number of beautiful examples which Bunsen gives us in his paper, I would refer you to this diagram, on which I have marked some of the most important, to show the delicacy of the spectra analytical method. "Soda: $\frac{1}{3000000}$th part of a millegramme, or $\frac{1}{180000000}$th part of a grain." These numbers seem almost absurd. I can scarcely tell you to-night how they were determined, but, in order to show you how delicate the process is, if we burn a piece of sodium weighing, say the $\frac{1}{10}$th of a grain on a piece of paper, we allow the vapour to diffuse itself throughout the room. Supposing the whole of it went up in vapour, you would have to see how much of that sodium was present in every cubic foot of the air in this room. Then you must know how much air each of these gas lamps requires to burn a certain quantity of gas in a certain given time—for instance, five minutes; then bring it down to the time required to take an observation, say two seconds, and in that two seconds you will see there must have been a minute fraction of the $\frac{1}{1000}$th part of a grain present in that volume of air. In this way, which is a sort of rough description of the method which Bunsen used, he arrived at this astounding fact, that the $\frac{1}{3000000}$th part of a millegramme, or the $\frac{1}{180000000}$th part of a grain of soda can be easily detected. Soda is always present in the air; all bodies exposed to the air show the yellow soda line. If a book be dusted near a flame, the soda re-action will be seen. If we leave a piece of platinum wire out in the air for an hour or two, it will no longer impart no colour to the flame as the platinum wire did before, but it will give us the soda flame. If I pass it through my finger you will see the same result,—the yellow flame indicative of the presence of soda, which the platinum wire has taken from my finger.

Of lithia: $\frac{1}{100000}$th part of a millegramme, or $\frac{1}{6000000}$th part of a grain, can be easily detected. Lithia was formerly only known to occur in four minerals. It is now found by spectrum analysis to be one of the most widely distributed elements. It exists in almost all rocks, in 3 cubic inches of sea and river (Thames) water; in the ashes of most plants, in milk, human blood, and muscular tissue. An experiment which Bunsen is very fond of showing is that of holding the end of a cigar, (which our German philosophers generally have at hand,) in the flame. You cannot see the lithium flame produced by it with the naked eye, but it is quite plain when we look at it through a prism.

[Some dusty books were then brought in, and the Lecturer showed very clearly the presence of soda in the dust by knocking them together near the gas-flame.]

On one occasion when Bunsen was examining the spectra of the alkalies obtained from certain of the mineral waters of Germany by means of this apparatus, he observed the occurrence of two bright blue lines, which, when he had examined similar spectra before, by these means, he had not discovered. Hence, he concluded that these bright blue lines must be produced by a new and hitherto undetected alkaline metal. Subsequent research proved the validity of the supposition. The new metal was found and separated from the accompanying alkalies. The analogy between this discovery and a celebrated one in another branch of physical science will be evident to all; as Adams and Leverrier discovered Neptune by perturbations which it produced in the orbit of another planet, so Bunsen discovered cæsium by the perturbations which it caused in the spectrum. I have through the kindness of my friend the opportunity of showing a compound of this new metal to you. This was sent to me as the bi-tartrate of cæsium by Bunsen, and was all that could be obtained from four tons of mineral water.

I will first show you the flame, which is a very bright violet, which it produces, and then the spectrum. He has also determined its various properties, which, however, I am afraid the time will not permit me properly to describe. It very closely resembles potassium; it produces an insoluble bi-tartrate, and reacts in various other ways like potassium. It is called cæsium, from the Latin word "*cæsius*," meaning "bluish-gray." I have here a representation of the spectrum of the new metal, which consists of two very bright blue lines, one of which is almost coincident with the strontium blue line. Only on Monday last I received a letter from Bunsen, which contains the following most interesting information. This is an extract from the note which he sent me, and this I believe is the first announcement which has been made of what I am going to state:—"The substance which I sent you as impure tartrate of cæsium contains a second new alkaline metal. I am now engaged in preparing its compounds, and will soon give you a more detailed information concerning it. The spectrum of the new metal consists of two violet lines, situated between Sr δ and K β." This, of course, is all I can tell you of this new metal, it being all that we know as yet; but this will serve to give you a notion of what we may expect to hear from this new branch of analysis; for if, on the examination of the alkalies of a mineral water, two new alkalies have been discovered, how many more remain to be discovered? This is a matter we must wait to learn.

That this same method of investigation can be extended to all the metallic elements is more than probable. I have before told you that when we bring platinum into flame we obtain no lines in the spectrum, because the platinum is not volatile enough. If we bring it into a flame hot enough, by which we can obtain an incandescent vapour of platinum, we then get a peculiar spectrum. Kirchhoff states that he has assured himself that the rarer metals,—for instance, erbium and terbium,—and these are substances which chemists seldom or never meet with,—can be most quickly and certainly detected by the help of the spectrum analytical method. Experiments are now being carried on, with a view of making this examination practically applicable to all metals, by Bunsen and Kirchhoff. It would therefore be premature to discuss this subject now, but we may soon expect to know more about this most important matter.

(To be concluded in our next.)

ROYAL DUBLIN SOCIETY.

Monday, February 18, 1861.

Lord Talbot de Malahide *in the Chair.*

The monthly scientific meeting of this Society was held on Monday evening, February 18, when

Professor **Davy** read a Paper "*On the Injurious Effects of Arsenical Pigments*," and said that in consequence of the difference of opinion which exists on the subject; and as it had never been touched on in Ireland, except by Dr. Apjohn, who had published a letter on it in the *Farmers' Gazette*, he had thought it would be interesting to state the results of several experiments which he had made with a view to the detection of arsenic in paper hangings, material for ladies' dress, &c. He alluded to the case of Dr. Healy, of London, who had reported the symptoms under which he had suffered himself whenever he inhabited a room covered with green paper, and which immediately disappeared on his removing his place of residence. Professor Davy also alluded to the letter published by Dr. Hinds in the *Medical Times* for May 23, 1857. In consequence of these communications Mr. Phillips had been employed by the Board of Trade to investigate the matter, and had reported to them that although the paper might contain as much as a drachm of the arsenite of copper to the square foot, still that as the arsenic was not evolved at ordinary temperatures no ill results would accrue from inhabiting the rooms. This report was still made use of by the paper makers, who circulated it among their customers. In answer to this report, Dr. Healy wrote again to the *Daily News*, and Dr. Hinds to the October number of the *Pharmaceutical Journal*, reiterating their statements, and Dr Taylor, who wrote in the *Journal of the Society of Arts*, corroborated their testimony, and said that Mr. Phillips' arguments might as well be proved by the state of the miners in Cornwall, some of whom did not suffer in the least from the practice of their trade. Dr. Taylor accounted for the immunity of some persons by the fact that the flock on some papers was very loosely applied, and became diffused through the air by simple friction. He gave also the case of a man who had got very sore eyes from having dusted some books, the deposit on which, on being analysed, gave a large per-centage of arsenic. Professor Davy also quoted the cases of two children who lately died in London from arsenical poisoning, supposed to be contracted by sucking toys which had been covered with green paint. In one case the paper on the walls was found to contain fifty-two grains of arsenite of copper to the square foot. Professor Davy remarked on the evolution of arsenic from superphosphate of lime which had been laid by, and attributed it to the existence of the metal in the sulphuric acid emplyed in its manufacture. He alluded also to cases which occurred in the Adelaide Hospital, and also to the Librarian of the Cork College, in whom inflammation of the eyes and ulceration about the nose and mouth appeared to be the most prominent symptoms. He showed that it was quite possible that the arsenic might be in a state of gaseous combination which would not be detected by the ordinary tests, but which might exert a very deleterious influence on the health, and that in this respect it was analogous to phosphorus and lead. Professor Davy had detected the poison in many forms of confectionery, in dyes for ladies' dresses, especially in green tarletan, from a piece of which, nine inches square, he had extracted twenty grains of arsenite of copper, which was so loosely attached to it that it became diffused even on shaking it. He had also analysed pieces of wax taper, in ninety grains of which he had detected half a grain of arsenic by Reinsch's method—a process which does not give more than one-half the real quantity of the poison. Professor Davy concluded by detailing the symptoms under which paper-hangers, painters, and other persons who were liable to the contact of the poison laboured, and which appeared after even one day's or a few hours' work with the deleterious material, and hoped that ladies and persons who had a *penchant* for green colours would at least consider the results which must accrue to the workmen in these materials if their manufacture be encouraged.

Dr. **Barker** mentioned that he had analysed a specimen of green confectionery for Dr. McClintock, and detected a very appreciable quantity of arsenic. He also alluded to the symptoms which attacked painters who worked exclusively with lead and turpentine, and argued that as arsenic was so much more volatile, it was the more likely to be disseminated.

Mr. **Hamilton** mentioned that he had been assured when purchasing a green paper that it contained no arsenic, but that having perceived symptoms of poisoning among his family, he had had the paper analysed and detected the poison in considerable quantity.

Professor **Davy** said that all green wall papers contained arsenite of copper, whether dear or cheap, French or English, dark or light, sized or unsized.

CHEMICAL SOCIETY.

Thursday, February 21, 1861.

Warren De la Rue, *Esq., F.R.S., Treasurer, in the Chair.*

Dr. Murray Thompson, and Messrs. W. Weston and J. Napier were elected Fellows.

The following Papers were read:—"*On the Action of Dilute Nitric Acid upon some Hydrocarbons of the Benzole Series,*" by Messrs. W. De la Rue and H. Müller. The authors endeavoured to obtain ampelic acid by acting on the benzol hydrocarbons obtained from various sources, with dilute nitric acid, but without success. But by acting upon purified coal-tar naphtha, with about fifteen times its volume of a mixture of one part of nitric acid and three parts of water, they eventually produced a yellowish-white flocculent substance, partly dissolved, partly suspended in the liquid. After partial purification from nitro-compounds, &c., this substance was treated with a large quantity of boiling water, which left some terephthalic acid undissolved. The solution on cooling yielded a deposit which after drying and fusing was distilled. It yielded first a palmitic-acid-like distillate which solidified in the neck, and then a sublimate of large acicular crystals which deposited in the body of the retort. These crystals consisted apparently of an hitherto undescribed acid, bearing some resemblance to the anhydride of nitrophthalic acid. The palmitic-acid-like distillate was resolved by treatment with a Burmese naphtha product boiling at about 90,—probably hydride of capryl—into two acids, one a homologue of benzoic acid having the formula $\text{Ꞓ}_6\text{H}_4\text{Ɵ}_2$, possibly collidinic acid and the other having the composition of benzoic acid but rather corresponding in its properties to Kolbe and Lautemann's recently-discovered isomer of benzoic acid, namely, salylic acid.

Dr. Thudichum read a Paper "*On the Putrefaction of Bile and the Formation of Gall-stones.*" He had observed casts of biliary ducts occupying the very centres of gall-stones, from which he inferred that the morbid process by which these concretions had been formed was not confined to the gall-bladder. These casts were constituted of the brown modification of cholochrome termed cholophacine. According to the author the essential constituents of gall-stones are cholochrome, cholic and choloidic acids, and earthy salts, being the products which are precipitated during the putrefaction of bile out of the body. Hence he inferred that gall-stones were formed by a process analogous to that of putrefaction, by which the bile acquired an acid reaction. Human gall-stones usually contain in addition a large quantity of cholesterine crystallised around the true biliary calculus. The author described from his own observations the process of the putrefaction of bile and agreed generally with Garup Besancy, but differed from him in considering the choloidic acid to result from the metamorphosis of the cholic, and not the cholic acid to result from the metamorphosis of the choloidic. He then described the methods of analysing ox gall-stones and human gall-stones, and of putrid ox and human bile.

Dr. Guthrie read a Paper "*On the Bisulphide of Iodine.*" No definite compound of iodine and sulphur has been hitherto described, despite the very definite character of one, at least, of the sulphides of chlorine. The author obtained bisulphide of iodine by reacting upon iodide of ethyl with bisulphide of chlorine according to the equation, $C_4 H_5 I + S_2 Cl = C_4 H_5 Cl + S_2 I$. By the spontaneous evaporation of the chloride of ethyl, the bisulphide of iodine separates in magnificent crystals. Its composition was determined by analysis.

Mr. R. Adie read a Paper "*On Ground Ice.*" He, observed that during the recent frost, despite its intensity there was very little ground ice formed, probably because the covering of snow preventing the frost penetrating to any depth in the earth. He was of opinion that the position of ground ice was one of lodgement merely. Masses of ground ice, seen in the river Eden in 1854, contained throughout their substance water-worn pebbles, and were inclined towards the current from which they were receiving continual supplies.

Dr. Frankland considered that ground ice was really formed at the spots in which it was found. He repudiated the idea of its being produced through any radiation from the bottom of the stream, because water is quite opaque to rays of heat of low intensity. He supposed that in rapid, shallow streams the temperature throughout from top to bottom was rendered uniform by intermingling and that the presence of irregularities at the bottom determined the formation of ice crystals there, in accordance with the usual tendency of crystals to become attached to rough surfaces.

NOTICES OF BOOKS.

A Manual of Elementary Chemistry, Theoretical and Practical. By George Fownes, F.R.S. Eighth Edition, Revised and Corrected. London: John Churchill. 1861.

Fownes' excellent Manual is too well known to need any fresh commendation from us. It offers in one compact volume an interesting, well-written, and clear account, not only of chemistry proper, organic and inorganic, but also of chemical physics. The fourth part, devoted to Animal Chemistry, will be read with advantage by medical students, among whom this volume is justly popular.

The present edition differs in one or two particulars from the preceding issue of the work. In addition to the concise abstract of the history of ammonia and its numerous recently-discovered derivatives which the editors have inserted, they have given in the form of an appendix an account of Gerhardt's equivalents and notation, which, although it hardly answers to our expectations, will be found to be easily understood by students. We shall, on an early occasion, quote part of this essay on the Unitary System, only giving here a word of comment on further most important developments of Gerhardt's views, which the authors of this account have omitted to introduce. The new matter contained in the work before us is, of course, of considerable value; but we do not think that the present edition shows any marked advance over its predecessor in other respects. We did not expect to find, nor would it have been desirable to insert in it, notices of new substances of insignificant character or doubtful formula; but in looking through the volume, we were struck by several omissions of important discoveries the value of which has been generally recognised. As a warrant for this statement, we will point out a few places, selected from a much larger number to which we might refer, which seem to us to exhibit the fault which we have mentioned.

On p. 109 no mention is made of the silicon compounds obtained by Wöhler, particularly of the body $Si_2 H_2 O_3$, which corresponds so closely to all appearance with Brodie's graphic acid $Gr_2 H_2 O_5$ $(Gr_2 = C_{11})$.

The older equivalent of lithium, namely, 6·5 is retained on p. 280: it is not, however, quite certain that the more recent number 7, or rather 6·93 is really more correct.

We confess that we cannot quite grasp the meaning of these directions for obtaining aluminium, given on p. 295; we are told to heat "equal parts of chloride of potassium and cryolite, with 2 parts of sodium and 5 of cryolite."

On p. 353 we observe no reference to the beautiful and effective electrolytic process for the detection of arsenic, lately perfected by Professor Bloxam.

On p. 512 no mention is made of the binoxide of benzoyle described by Professor Brodie; a beautifully crystalline and refractive substance, exploding when gently heated, and possessing representatives in other organic series as that of acetyle; the binoxide of acetyle detonates violently on a very slight elevation of temperature. Not only their physical properties, but their chemical constitution also, causes great interest to attach to these bodies.

On p. 515 no attempt is made to elucidate the constitution of benzophenone as benzoyle-phenyle $C_{12} H_5$, $C_{14} H_5 O_2$, for instance; its formula, $C_{26} H_{10} O_2$, is merely given without a word of explanation.

On p. 517 the almost if not quite obsolete and very costly process for preparing chloride of benzoyle by means of oil of bitter almonds and chlorine is given, no notice being taken of the more recent and perfect process of Gerhardt, by which this important substance is obtained, by acting with pentachloride of phosphorus upon benzoic acid. It is also stated to boil at 196° F., this being the boiling-point on the Centigrade scale.

The interesting formation of toluylic acid from the cyanide of toluenyle obtained from toluole through the intervention of the chloride, $C_{14}\ H_7\ Cl$, is not noticed on p. 523. We have also looked in vain for any reference to that most important reaction, the type of a large class of reactions, by which Maxwell Simpson has succeeded in obtaining the bibasic acid, the succinic, from the bicyanide of ethylene, $C_4\ H_4\ Cy_2$.

On a former occasion we have referred to the intelligible view which Kekulé has given of melam, ammeline, melamine, ammelide, melam, and the mellonides: here, on pp. 570, 571, the crude formulæ are given without comment.

The bases of the chinoline and pyridine series are spoken of on pp. 608—610 as of uncertain constitution; the important observation that they contain no replaceable hydrogen being unnoticed. The fact that only such simple ethyle derivatives as ethyl-pyridyl-ammonium can be formed from these bases limits very materially our uncertainty as to their true nature.

To Borneo camphor is assigned the formula $C_{28}\ H_{18}\ O_2$, a typographical error for $C_{20}\ H_{18}\ O_2$. The interesting relation of Borneo camphor as the alcohol of the camphoric series to ordinary camphor as the aldehyde, and to campholone ($C_{20}\ H_{18}$) as the hydride, is not alluded to.

Of caoutchoucine, the liquid produced by the destructive distillation of india-rubber, we find it affirmed that "it is probably a mixture of several hydrocarbons scarcely to be separated from one another," although Greville Williams has clearly shown that it consists chiefly, if not entirely, of two very definite liquids: isoprene $C_{10}\ H_8$, boiling at 37° C., and having a vapour density of 2·44; and caoutchine $C_{20}\ H_{10}$, boiling at 171° C., and with a vapour density of 4·65.

On p. 647 it is said that to kreosote "the formulæ (*sic*) $C_{14}H_8O_2$ has been given, but is not established beyond doubt;" while on p. 657 it is affirmed that cresylic alcohol, $C_{14}H_8O_2$, is isomeric with kreosote.

Although no list of errata appears in the volume, yet it is by no means free from typographical errors. Of these we have already noticed several, and we find (p. 194) choride of iodine formulated IC instead of I Cl. The melting point of potassium is given as 57° 1·77 C., whatever that may mean, on p. 255.

We have looked in vain for any mention of Anderson's satisfactory experiments with pyrrhole, of the acetylene C_4H_2 of Berthelot, of the graphic acid of Brodie, and of many of the other interesting substances with which chemistry has been enriched during the last year. The recent researches of Andrews and Schönbein on oxygen and ozone scarcely receive any notice; and, although *pelopium* does not exist, we find descriptions of its oxide and chloride. But we will now leave the unpleasant task of enumerating the defects of a substantially good book as the present.

In another place we shall quote from the Appendix the account given of the Notation of Gerhardt, generally termed the Unitary Notation. This account is the more valuable since, while the new notation is constantly used by many distinguished chemists in their papers, and discussion on it is frequent, no compact statement of its main features has before appeared in England.

The defects in the sketch of Gerhardt's Notation, and in the Synopsis of Chemical Substances which follow, are chiefly those which are remedied in the important and able work of Kekulé, and to which we have before alluded in these columns. The manifest improvements introduced by this chemist in 1859 should certainly have found a place in a work published in the present year. The Editors of Fownes' "Manual" retain the hydrochloric acid type as distinct from the hydrogen type, a step of very doubtful propriety, and they ignore the 4th primary type, which includes the compounds of the tetratomic elements, such as carbon. A comparison of the two methods will show clearly what we mean.

Editors of Fownes' Manual.	Kekulé.
Hydrogen Type. $\left.H\right\}H$	Hydrochloric acid Type. HCl
Hydrochloric acid Type. $\left.H\right\}Cl$	Water Type. $\left.\begin{matrix}H\\H\end{matrix}\right\}\Theta$
Water Type. $\left.\begin{matrix}H\\H\end{matrix}\right\}\Theta$	Ammonia Type. $\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N$
Ammonia Type. $\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N$	Marsh-gas Type. $\left.\begin{matrix}H\\H\\H\\H\end{matrix}\right\}\text{Ꞓ}$

We cannot help regarding the omission of all notice o paired or conjugate types, in which two or more types are conjoined into one, as a mistake. By recognising such typical forms as

Hydrogen and Water.	Water and Ammonia.	Methyl-sulphurous Acid.	Oxamic Acid.
$\begin{matrix}\left.\begin{matrix}H\\H\end{matrix}\right\}\\\left.\begin{matrix}H\\H\end{matrix}\right\}\Theta\end{matrix}$	$\begin{matrix}\left.\begin{matrix}H\\H\end{matrix}\right\}\Theta\\\left.\begin{matrix}H\\H\\H\end{matrix}\right\}N\end{matrix}$	$\begin{matrix}\left.\begin{matrix}\text{Ꞓ}H_3\\(S\Theta_2)''\end{matrix}\right\}\\\left.H\right\}\Theta\end{matrix}$	$\begin{matrix}\left.\begin{matrix}H\\(\text{Ꞓ}_2\Theta_2)''\end{matrix}\right\}\Theta\\\left.\begin{matrix}H\\H\end{matrix}\right\}N\end{matrix}$

the system of types becomes much more comprehensive and, at the same time, affords an easier and truer way of representing such compounds as methyl-sulphurous and oxamic acids.

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—I do not usually deem it necessary to reply to the statements or accusations of anonymous critics; but, in the present instance, the letters of a gentleman, with the singularly inappropriate signature of "Verax" (published in Nos. 63 and 64 of your Journal), afford me an opportunity of reiterating some of my former observations on the above subject.

Fair and open discussion I cordially invite upon any published results or remarks of mine. I cannot but regret, however, that "Verax" in his virulent animadversions upon my paper, should have so completely lost sight of that courtesy due to every individual from others of his species, and which alone is the sign of a really independent and unbiassed opinion.

In the first portion of his letter "Verax" inquires if "colours and essences of every description" are "to be henceforth decried," and answers his own query further on in these words—"We must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited." These paragraphs evidently neutralise each other, and could not have been written by any person who had accorded to my paper a moderate amount of attention.

As any one (of competent analytical skill) who likes to take the trouble, can ascertain from actual experience the facts upon which my "sweeping assertion," that of London bread fully eighty-seven per cent. is more or less adulterated, is founded, I consider it unnecessary to say more on this point, than that I have only to repeat my original statement without the slightest qualification.

"Verax" next complains that he has not been told how many specimens I have examined, how many shops furnished those specimens (with the names and addresses of the vendors, doubtless!) what adulterant, and how much of it, was employed in every instance, what processes were adopted for the detection of these adulterants, and a few other little details of a like character. Will "Verax" have the goodness to say how (if he had written the paper) he would contrive to condense the amount of information just indicated, and which I calculate would occupy, at least, 800 pages of quarto, even if written most concisely and printed in brevier type,—into a total bulk of reading matter requiring not more than one hour and twenty minutes for its delivery?

Do authors and lecturers habitually give the exact and detailed grounds for all and every statement, trifling and important, that they may make, as they go on? If I had departed from the general rule in this respect, would not "Verax" have been the first to stigmatise my paper as prolix and tedious to the last degree?

Although I have stated "crumbly texture" and "brittleness of structure" to be indications of the probable presence of potatoes and rice-flour, respectively in bread, who could for a moment suppose that I regarded such characteristics as more than *mere indications?* I might describe a donkey as "a beast with long ears," without necessarily meaning that every long-eared animal must be a donkey! In a word, all the very simple tests for, and characteristics of the presence of, adulterants named in my paper are to be regarded solely as popular tests, no more suited to the delicate researches of a laboratory, than such researches are, to be carried on in a drawing-room, still, I hold that everyone should have it in his power to gain some little insight into what he is eating and drinking day by day, as by a small amount of trouble Paterfamilias might steer clear of many injurious adulterations, without, in every instance, calling in professional assistance.

I have detected the presence of bone-dust in bread and flour in five instances with the aid of the microscope; on this point "Verax" has arrived at a negative result, "in spite of numerous inquiries." In imitation of his peculiar diction, I might ask, How many inquiries made up the sum total he designates "numerous," and whom did he interrogate on the subject?

By no process with which I am acquainted can "brewers' grains be made to bear any resemblance to oats;" nor am I aware that flour possesses many points of similarity to cocoa, which, however, it is constantly employed to falsify. But does "Verax" innocently imagine that an adulterant must always resemble the adulterated article in appearance?

In 1856 a horse was killed, in Berkshire, with adulterated "rape-cake." I decline to publish more particulars, as the sample was sent to me for examination professionally.

"Verax" is of opinion that because I am acquainted, in common with many others, with the extent to which adulteration is practised in the matter of meat, that I ought immediately to acquaint the Inspectors, and become a common informer! However humble my mission in this world may be, it lies in a rather different direction to that just named.

Has "Verax" never heard of the Parliamentary Committees on the Adulteration of Food, and their results? Has he never seen the works of Accum, Mitchell, Pereira, Dodd, Normandy, and Hassall? I cannot otherwise account for the amount of astonishment with which I am flattered to find my paper has impressed him.

Surely "Verax" must peruse the columns of the *Times* and *Daily Telegraph* in a very careless manner, if he is not cognizant of the cases of "Diseased Meat" being exposed for sale at one or other of the London markets—an occurrence by no means rare; only a few days ago such an instance occurred at Newgate Market, and the trial was noted in the principal papers, the defendants being convicted and fined. Now, for every single conviction, how many cases are there which escape prosecution altogether!

Then, again, with regard to confectionery, how long ago is it since two children of the name of *Carroll* were poisoned by the mineral colouring-matters with which their "sugar-sticks" were *ornamented?*

It is, indeed, but too true, that adulteration is carried on throughout the length and breadth of the land to an extent which may justly be denominated "fearful;" and the fact is gradually becoming evident to all classes, despite the ridicule of some, the determined opposition of others, and the ignorance or apathy of the majority, who are too much occupied with their daily avocations to give the subject due consideration.

"Verax" will doubtless be "astonished" to hear that I stated that "about 91 per cent. of the pickles sold in London are more or less adulterated," because out of 1127 samples examined, I found *only* 101 *to be good and pure.*

Now, with regard to tea, I may remark, that my observations are founded upon the result of the examination of several thousand samples, forming, I am inclined to think, a more reliable criterion than either the *ex parte* statements of tea-dealers and others whose interest lies in exactly the opposite direction to that in which *truth* is to be found. The remark that "tea cannot be adulterated with impunity and profit," is too evidently absurd to need comment of any kind.

Then, again, what kind of authority is "one of the largest bakers in Vienna" upon the question of the purity of bread? The adulterated "sugar-apples" exhibited at the Society of Arts, were distinctly "*warranted all pure sugar*" by the seller, although the presence of red lead was clearly demonstrated.

One paragraph of "Verax's" last letter I consider too important to pass without notice; it runs thus:—"It seems to me that Mr. Scott's suggestions as to the course that should be adopted to protect ourselves against the ill effects of an improper dietary, *would do away altogether with any but the best materials for food.*" If "*best*" be held to signify "*pure and wholesome,*" "Verax" has described my object exactly.

In conclusion, I beg to avow my decided opinion that the brown sugars of commerce (except the "crystalline" varieties) are detrimental to the public health, and consequently that their sale (retail) should be prohibited altogether; that certain varieties of unsound grain are also injurious, and should likewise be condemned; and that vegetables of all kinds should be subject to a closer *surveillance* than at present, with the view of preventing the sale of decomposing or otherwise injurious specimens.

I will not, Sir, occupy more of your space to-day in reply to the over-strained criticisms and puerile objections of your correspondent, as all attempts to mislead the public and persuade them that their food and drink is of the purest description imaginable, must inevitably, and of themselves fall to the ground—*Experientiam docet.*

I am, &c. WENTWORTH L. SCOTT.

Bayswater, London, W.

P.S. The fact mentioned by Mr. Horsley, of the presence of copper in bread and flour at Cheltenham, is very interesting. Would he kindly furnish me with a few specimens, and any further particulars he might deem it advisable to afford? I have myself detected copper in two instances only; but the grain in these cases had been affected with the "smut," so I attributed the copper to the probable employment of "bluestone" while in the ear.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Salts of Hyponiobic Acid.—The compounds of hyponiobic acid with bases are said by H. Rose (*Bericht* *der Akademie der Wissenschaften zu Berlin*, 1860, s. 710,) to share in general the properties of the salts of tantalic and niobic acids. The potash salt is soluble in an excess of the hydrate and carbonate of potash; and hence it is difficult to prepare the neutral salt pure. When hyponiobic acid is fused with carbonate of potash, the latter loses just so much carbonic acid that the compound $3KO + Nb_2O_3$ is formed. The soda salt is the most important of the compounds of hyponiobic acid, since it can be used in the preparation of all the insoluble salts. It is somewhat more soluble in water than the niobate and tantalate of soda, and can be obtained in larger and more perfect crystals than these salts. It can be prepared with 5 or 7 atoms of water of crystallisation. Like the soda salts of niobic and tantalic acids, it is insoluble in a solution of hydrate or carbonate of soda, except when the solutions are very dilute, in which case it quickly dissolves on boiling. The niobates and tantalates do the same, so that the best way of separating these is to treat them with a solution of hydrate or carbonate of soda of a certain strength. At 100° C. the hyponiobate of soda loses the greater part of its water of crystallisation, and then is perfectly soluble in water, which essentially distinguishes it from the niobate, and particularly from the tantalate. When the neutral salt is heated it is changed into an acid salt and hydrate of soda, which can exchange its water for carbonic acid if the heating be carried on in an atmosphere of carbonate of ammonia. This breaking up of the hyponiobate takes place at a higher temperature than the similar breaking up of the niobate and tantalate of soda. The neutral hyponiobate of soda contains three times as much oxygen in the acid as in the soda; hence, the composition is $NaONb_2O_3$. A basic salt, or rather a compound of the acid salt with hydrate of soda, can be obtained, as well as an acid salt, both of which are partly soluble in water. They are made by fusing together hyponiobic acid and carbonate of soda. By fusing together hyponiobic acid and carbonate of soda until the mixture no longer loses weight, a salt is formed which has composition $3NaONb_2O_3$. By treating this with water, it is decomposed into a neutral salt, while two atoms of hydrate of soda are set free.

A solution of the neutral hyponiobate of soda remains unchanged in the air, even on boiling.

Neutral hyponiobates of the metals may be made by adding a solution of the soda salt to a solution of a neutral salt of the metals. The author has prepared hyponiobates of magnesia, silver, mercury, and copper, in all of which the oxygen in the acid is three times as much as that of the base.

II. ORGANIC CHEMISTRY.

Test for Cholesterin.—Hugo Schift describes the following reaction of cholesterine (*Annalen d. Chem. und Pharm.* Bd. cxv. s. 313). If cholesterin be moistened with hydrochloric or sulphuric acid containing perchloride of iron, it becomes of a magnificent violet colour. A mixture of two or three volumes of concentrated hydrochloric or sulphuric acid and one of a dilute solution of the perchloride will answer for the experiment, or even the ordinary commercial hydrochloric acid. A small piece of cholesterin should be rubbed by means of a glass rod with a drop of the reagent, and the mixture then gently warmed. It first becomes of a reddish, which as the temperature rises changes to a blue violet, colour. A strong heat destroys the colour. When sulphuric acid is used the mixture must be warmed more cautiously. It first becomes carmine red, then violet, and at a high temperature is carbonised. Nitric and phosphoric with the perchloride of iron will not give the reaction.

Metamorphia.—In an attempt to prepare morphia by Mohr's method from the residue of tincture of opium Scharf, an apothecary at Munich, obtained some crystals whose solution in an acid gave no precipitate with ammonia. These crystals were further examined by Wittstein (*Wittstein's Viertel-jahrsscrift*, Bd. ix. s. 481), who found them to be a salt of a new base which he has named *Meta-morphia*. The hydrochlorate of this base is soluble at the ordinary temperature in twenty-five parts of water; the solution is bitter and has a neutral reaction. It is less soluble in alcohol, and is insoluble in ether. Liquor ammonia dissolves it perfectly as do caustic potash and the carbonate. Perchloride of iron turns the aqueous solution blue; chloride of gold gives a reddish yellow, flocculent precipitate with a strong aqueous solution which disappears on the addition of much water: chloride of platinum gives a yellow precipitate which re-dissolves in a dilute solution. Tannic acid produces a yellowish-white cloudiness. Strong sulphuric acid sets free hydrochloric acid from the salt without colouring it; nitric acid dissolves the salt, giving a yellow solution. The analysis of the hydrochlorate shows it to contain 12·211 per cent. of HCl. The hydrochlorate of morphia contains only 9·7, and the hydrochlorate of codeia only 9·81, per cent. of acid. From the above analysis the author calculates the equivalent of the new alkaloid to = 32·74. With the exception of pseudo-morphia, he says, (which shares many of the properties of the new alkaloid) no opium alkaloid has so low an equivalent. The author prepared the pure base by treating the hydrochlorate with sulphate of silver, and digesting the solution so obtained with carbonate of baryta. It crystallised from an alcoholic solution in stellated flattened prisms, which had very little taste. The new base is almost insoluble in cold water, but more so in hot. Alcohol takes up more; the solution has a weak alkaline re-action, and a very bitter taste. In ether it is almost insoluble. The caustic alkalies dissolve it. Strong sulphuric acid gives a brownish, and nitric acid a yellow colour with it. A strong solution of iodic acid added to the aqueous solution of the new base produces a yellowish colouration, and a strip of starch paper suspended in the mixture is turned violet. Perchloride of iron produces no change in the aqueous solution. Chloride of gold produces a yellowish cloudiness (not a blue colou as with the aqueous solution of morphia), which afterwards deposits as a brownish precipitate. Nitrate of silver gives a grayish-black cloudiness. The author gives no analysis of the base.

LABORATORY MEMORANDA.

On the Action of Sodium on Oil of Turpentine.—When sodium is brought into contact with the ordinary oil of turpentine of commerce, an active disengagement of gas is immediately produced, the action being continued till the sodium has disappeared. If sufficient turpentine be used, two very different precipitates being at the same time thrown down, the one white and gelatinous, the other brown and inclining to red.

The black precipitate frequently so envelopes the sodium as very much to retard the action; but on stirring, the effervescence is continued as before; if, however, very pure oil of turpentine be used, a much smaller quantity of the dark-coloured precipitate is formed, the white precipitate and the gas being produced as before.

But if the turpentine be repeatedly distilled, at the same time preventing as much as possible all contact with the air, on introducing the sodium scarce any reaction takes place, its surface after a considerable time being only slightly covered with a brown film,—for which reason I attribute the action in the first place to the decomposition of the resinous matter held in solution by the oil of turpentine, after which the reaction appears to sustain itself.

sulphate of ammonia is soluble at the ordinary temperature in 1·3 parts of water.

II. ORGANIC CHEMISTRY.

The Nitrile of Leucinic Acid.—O. Hesse and H. Limpricht state (*Annalen der Chem. und Pharm.* Bd. cxi. s. 201) that the so-called leucinic acid nitrile, $C_{12}H_{11}NO_2$, may be easily separated, but only in small quantity, from the rough tyrosin made by boiling horn with sulphuric acid. The tyrosin must be treated with boiling spirit, the alcoholic extract evaporated, and the resulting deposit purified by repeated crystallisations from hot absolute alcohol. So obtained, the body forms long white silky crystalline tufts of needles, which sublime unchanged between 173° and 180° C., are not very soluble in ether, but quickly so in strong alcohol, especially in the warm, and hardly soluble in cold water, but more so in hot.

III. ANALYTICAL CHEMISTRY.

Estimation of Soap.—Dr. Buchner gives (*Polytechnisch Centralblatt*, 1860, s. 1484) a method by which the amount of hard soap in a specimen can be calculated from the amount of fatty acid obtained when a given amount of the specimen is decomposed by a strong acid. The author makes use of a flask, the neck of which is graduated into cubic centimètres; into this flask, half full of water, he puts half-an-ounce of soap and dissolves it. He then adds the acid, either commercial hydrochloric, or dilute sulphuric acid, and warms the mixture, whereupon the fatty acids are set free. He now puts sufficient water to allow him to read off the number of cubic centimètres the acids measure in the neck of the flask. The fatty acids from different sources differ a little in weight; but the author found that the average weight of a cubic centimètre is ·93 gramme, which is near enough to the truth for ordinary practical purposes. As the acids are combined with $\frac{1}{16}$ glycerine, it is easy, knowing the weight of the acids, to calculate the weight of the fat used; and as on the average 100 lbs. of fat give 155 lbs. of good hard soap, the weight of the real soap can be calculated when the weight of the fat is known. These calculations may be made by the use of a table which the author has constructed, from which we extract the important parts. The results are not to be considered scientifically accurate, but are near enough to the truth for ordinary business purposes. The process requires only one weighing, is executed in a few minutes, and is so simple that it can be performed by a common workman.

I. Cubic centimètres of fat acids separated from half-an-ounce of soap.

II. Per-centage of water, ley, glycerine, &c., in the specimen.

III. Per-centage of good hard soap.

I.	II.	III.
½	97	3
5	69	31
6	63	37
7	57	43
8	51	49
9	44	56
10	38	62
11	32	68
12	26	74
13	20	80
14	13	87
15	7	93

IV. TECHNICAL CHEMISTRY.

Application of Hypochlorite of Zinc.—Balard has shown that when hypochlorite of lime and sulphate of zinc are mixed together, sulphate of lime and oxide of zinc are precipitated, and a solution of hypochlorous acid remains. Sacc has availed himself of this reaction in the bleaching of stuffs dyed with madder. He prints a cloth, which has been dyed red, with a mixture of 4 parts of sulphate of zinc, 5 parts of gum, and 10 parts of water, and when dry, he passes it through a cold baths of chloride of lime of 2 degrees Baumé. The printed parts are immediately bleached, and the remaining red takes an increased brilliancy of colour. This energetic bleaching power depends upon the ready decomposibility of the hypochlorite of zinc and the consequent setting free of hypochlorous acid. This process, the author says, has many advantages.—Dr. Varrentrapp in *Mitth. für d. Gewerbever der Herzogth. Braunschweig*, 1860, s. 24.

MISCELLANEOUS.

Seleniferous Deposit from the Sulphuric Acid Chambers.—Ores of Tellurium.—Any gentleman who may be able to place the Editor in possession of some of the above, by purchase or otherwise, will confer a great favour by communicating particulars to Mr. Crookes at the Office of this paper.

Chemical Society.—The next meeting of this Society will be held on Thursday next, the 21st inst., when a lecture will be delivered "On Thermo-Dynamics in Relation to Chemical Affinity," by A. W. Williamson, Ph.D., F.R.S.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Monday, March 18, at 9 o'clock, M. P. Chaillu "On Personal Narrative of Travels in Central Africa." Tuesday, March 19, at 3 o'clock, Professor Owen "On Fishes." Thursday, March 21, at 3 o'clock, Professor Tyndall "On Electricity." Friday, March 22, at 8 o'clock, Professor H. D. Rogers "On the Origin of the Parallel Roads of Glen Roy." Saturday, March 23, at 3 o'clock, Dr. E. Frankland "On Inorganic Chemistry."

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co, at the Office, 10, Stationers' Hall Court, London, E.C.

A Letter from "Verax," on the Adulteration of Food, has been received, but too late to be inserted this week.

Communications declined with thanks.—G. F. R., J. A. Davies, and C. L.

M. A. B.'s request shall meet with consideration, and, if possible, be acceded to.

C. O. M. Wallace.—Apply to the Editor of one of the Photographic periodicals.

A Constant Reader.—The assertion respecting the Electric Light is quite correct. It is now used in lighting up one of the squares in Paris, and is cheaper than gas for that purpose.

H. K. S.—You had better purchase the Magenta dye. It can be got for a much less sum than you could make it for small quantities, even supposing you to be successful in the attempt. The best mode of its preparation is a trade secret.

A. Ashbey.—"Faraday's Chemical Manipulations" has long been out of print. A copy may occasionally be picked up at a second-hand bookseller's, but it always fetches a good price.

A Metallurgist.—We have not space to answer all your questions. Dr. Percy's book on metallurgy is expected to be out shortly and will contain full information on all these points. There are also several good works on metallurgy which would give you an account of what you require.

Silver in Sea-Water.—This occurs in an excessively small proportion. In large bulks of the water, however, it becomes an important quantity; for instance, it has been calculated that a cubic mile of the water of the Atlantic, contains nearly fifty tons' weight of silver dissolved in it in the form of chloride. Whether it can, or will, ever be economically extracted from it, we will not venture to say. The copper bottoms of ships, after long voyages, are found to contain more silver than the metal did originally; but the copper is dissolved in a larger proportion than the silver is precipitated on it.

PULVIS JACOBI *VER.*, NEWBERY'S.

To the Medical Profeſſion of Great Britain and Ireland.

Gentlemen,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports*," *vol.* 1, *p.* 317.

"Since this caſe came into my poſſeſſion, I have been led to make a good many clinical experiments, which have enabled "me to verify the reports which I had heard of the efficacy of JAMES'S POWDER, in ſometimes removing the APOPLECTIC "DIATHESIS in perſons advanced in life. It is, moreover, worthy of obſervation, that JAMES'S POWDER has been of "remarkable utility in certain inſtances of DETERMINATION OF BLOOD TO THE HEAD, which occurred at an early period of life, and "threatened to end by effuſion. Laſtly, in two caſes of GENERAL PLETHORA, in which, however, the head was more affected "than any other part, JAMES'S POWDER was exhibited by me with perfect ſucceſs."

Alſo *vol.* 1, *p.* 320.

"The following very ſimple method of exhibiting JAMES'S POWDER, in caſes of undue determination of blood to the "head, is that which I have generally purſued. The patient is made to begin with a very moderate doſe, not more than "two grains at bedtime, and to increaſe the doſe by half-a-grain every night, until ſome ſenſible effect is produced upon the "ſtomach, bowels, or ſkin. Should the ſtomach be affected with ſickneſs, the doſe muſt be leſſened by one grain on the "following night. By the addition of a little rhubarb to it, a larger quantity of JAMES'S POWDER may be adminiſtered "than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be "increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen "or twenty grains taken for a conſiderable period without any inconvenience."

The following extract (ſhowing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet*," *June* 16, 1860, P. 590,) as follows:—

"If the febrile excitement be ſuch as to require antimonial preparations, a pill containing 2 or 3 grains of James's Powder (prepared "by Newbery) ſhould be taken at intervals according to circumſtances."

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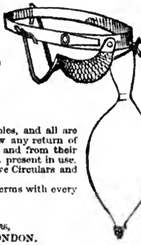

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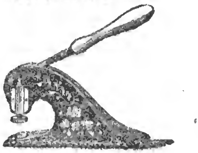

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THE CHEMICAL NEWS AND Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

No. 68.] Saturday, March 23, 1861. {Price 4*d.* {Post Free, 5*d.*

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THE CHEMICAL NEWS.

VOL. III. No. 68.—March 23, 1861.

ON THE METHODS PROPOSED FOR PREVENTING THE FURTHER DECAY OF THE HOUSES OF PARLIAMENT.

SINCE our last article touching on this subject, a tendency to reaction appears to have taken possession of the public. The narrow escape of policeman X. 2999—who, forsaking the ordinary beaten track of his professional brethren, nearly fell a victim to the brittleness of amorphous dolomite—has ceased to impress us. But, although it is certain that the decay has been exaggerated, we are not to suppose that the building is sound. It is, moreover, evident that the authorities are fully resolved to take some steps to remedy the existing evil, be its magnitude less or more than is now generally believed.

It is a melancholy thing for the honour of Chemistry, as a science, that, no matter how simple the matter upon which we may attempt to take the sense of its Professors, they never come to anything approaching to a unanimous verdict; and this, to a great extent, arises from the circumstance that there is no science of which it is so easy to acquire a smattering, or of which it is so difficult to acquire a mastery, as Chemistry. It is also notorious that shocking discredit is beginning to attach itself to numerous teachers, who disgrace themselves and the science they endeavour to identify themselves with, by dubbing themselves "Professors," without any just claim to the title. These men have generally some pet process, to the success of which they devote themselves, and happy are they if a "Company" can be found to back them with funds to carry out their chimerical ideas. Let us hope that the Commission appointed to consider the best method of preserving the Houses of Parliament will steer clear of any plans put forward by such people, and carefully sift the claims of the inventors who may offer their assistance. There are, among the names already before the public in connection with this matter, several men of worth and talent, whose processes deserve the fullest trial and investigation; and there are others whose plans are obviously absurd, and, therefore, may without difficulty be thrown aside.

Among the inventors before the public in connection with stone-hardening, there are some whose only fault has been a too sanguine belief in their own powers. Let us, therefore, in a matter of such moment, pause before deciding, and endeavouring, as far as the weakness of human nature will permit, to throw aside our preconceived ideas, carefully consider how far any of the proposed plans fulfil the demands upon them.

It has been urged by one of the most eminent Chemists in this country that any organic preparation must of necessity fail, owing to its perishability.[1] There is an element of weakness in this argument, owing to the fact that *no* composition can ever be arrived at which will *never* require renewal. No substance, we fear, can be applied to the stone *in situ* which shall penetrate entirely through, and render it as imperishable as the hardest and best material known to builders. The problem, if looked at from this point of view, becomes simplified, because we are only required to select that preparation which shall require the least amount of renewal. This element (renewal) enters as an important item in the cost, inasmuch as it is evident that the mere manual labour of covering so vast a pile will entail an immense expense, to say nothing of the extreme annoyance to the eye of perpetually seeing scaffoldings erected and platforms suspended, breaking every line of beauty, and hiding the lavish ornament upon which such labour and money has almost unavailingly been spent.

In a recent Number we took occasion to animadvert somewhat strongly upon a pamphlet published by M. Szerelmey. In that work the author states that his processes are a revival of those known to the Ancients. He, however, not only offers no real evidence of his methods being even founded upon the ancient ones, but the entire work is a mere vague, rambling treatise on the advantages of the preservative processes of the Ancients, clothed in language affecting to be scientific, but betraying, in numerous instances, an extremely feeble acquaintance with the science which, of all others, is most essential to the elucidation of an ancient, or the construction of a modern method. We think it, however, but just to M. Szerelmey to say, that, however unfavourable an impression his pamphlet and his secrecy may have made upon us, his process has obtained a very favourable verdict from a philosopher whose judgment all scientific men respect,—we allude to our great countryman, Professor Faraday. At a meeting of the Royal Institute of British Architects, held a few weeks back, Mr. Edward M. Barry, in the course of a very manly and straightforward speech, read a correspondence between Mr. Faraday and the First Commissioner of Works. From the reading of these letters it transpired that Mr. Faraday, after examining the work performed upon a certain portion of the Houses of Parliament by Mr. Ransome, and another portion by M. Szerelmey, came to the conclusion that the method of the latter gentleman would prove the best.

Mr. Faraday was requested to inform the First Commissioner whether M. Szerelmey used oleaginous or other organic matter; we have not been able to find whether any or what answer was returned to this important question. After reading his pamphlet we fancied we had a misty idea that he did employ some greasy or oleaginous substance, but it is scarcely necessary to say that we could get no definite information from a work written with the very object of preventing a conclusion from being arrived at.

Dr. Hofmann has expressed his opinions upon the subject freely, and his statements are always entitled to respect. He, like everyone else who has no pet process to push forward, does not attempt to cut the knot, but candidly confesses the numerous difficulties which surround the matter. It is somewhat amusing to observe how his devotion to organic chemistry shows itself in his advocacy of silicic ether as a substance adapted for the purpose of hardening the surface of the stone. It would be easy to draw a parallel between the distinguished Professor's advocacy of silicic ether for defending the Houses of Parliament, and the currier's (in the fable) advocacy of leather for defending the city; but, however ridiculous it may appear to talk of employing a re-agent which only one chemist has as yet been bold enough to offer to manufacture in quantity, and even then at a price of £6 per pound, it is, however, certain that no substance appears to contain within itself so many elements of success. The price, which at present renders its use hopeless, will doubtless one day be no objection. Let those who have received Dr. Hofmann's idea with unsparing ridicule remember that, with chemicals, the rarest to-day is sometimes a drug on the morrow.

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[1] The preservative constituent of silicic ether being, of course, silicic acid, it becomes an exception. The Chemist alluded to meant where the preservative constituent itself was an organic compound.

Professor Tennant, whom the Chairman (Mr. Digby Wyatt) introduced to the meeting previously alluded to as "one of the greatest mineralogists in this country," addressed the audience in a speech from which we have been unable to extract anything of which the least use could be made, or from which anything could be taken. It is true that he drew a parallel between the condition of the Druids and the Moderns as regards the difference in their respective facilities for carrying stone, which parallel must be very gratifying to every lover of progress, and that he "referred to the compact sandstones of the coal measures, compact limestone of the mountain limestone, syenite of Mount Sorrel, near Leicester, and the stone on the Malvern Hills;" but, unless it be the rebuilding the Parliament Houses with one or more of the stones alluded to by the Professor, we do not see that anything is to be gathered from his speech; certainly, nothing as to the object before the meeting.

As regards the experiments yet made, it appears that the following results have been obtained. We get our information from Mr. Scott's report on the experiments made on rapidly-decaying stone in Westminster Abbey.

1. *Water-glass.*—Tried in 1857 and 1858. Stone hardened, but decay only partially arrested.
2. *Paul's Aluminate of Potash.*—Tried same date Same result as No. 1.
3. *Ransome's Silicate of Lime Process.*—Same result.
4. *Szerelmey's Secret Process.*—Stone much hardened. Decay only slightly visible.
5. *Soap and Alum Process.*—Effects appear to have ceased.
6. *Rocha's Silicate.*—Effects remain, but efficiency not reported on.
7. *Shellac in Spirits of Wine.*—Admirably successful where protected from rain. Scarcely so successful where exposed to rain.
8. *White Wax in Turpentine.*—Failure.
9. *Same as* No. 8, *but with addition of Stearine.*—Better, but failing.
10. *Daine's Oil and Sulphur Process.*—Tried since July, 1859. Almost entirely successful.

We submit that the above results are most encouraging. When, in a matter of such import, 30 per cent. of the methods tried are *almost* complete successes, every hope may be entertained that soon some processes may be discovered *quite* equal to the demands upon them.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Extreme Difficulty of Removing the Last Traces of Carbonic Acid from Large Quantities of Air, by Messrs. C. W. Eliot *and* Frank H. Storer.

In the course of our research upon the impurities of zinc, we instituted a series of experiments, in order to ascertain whether the disagreeable odour of hydrogen gas, as generated from common zinc by means of sulphuric acid, could be attributed to the presence of any gaseous compound of carbon,—an apocryphal doctrine which seems to be quite generally credited.

The results of these experiments were entirely negative, in so far as they related to the point in question, and we should not have thought of publishing them had it not been proved to us, by a memoir recently printed in *Poggendorff's Annalen*, that one of the phenomena which we then observed had not been sufficiently dwelt upon by chemists.

In our experiments above referred to, a jet of hydrogen was burned in a glass globe, 30 centimètres in diameter, through which was drawn, by means of an aspirator, a steady current of air. Before entering the globe, this air had passed through an apparatus, described in full below, which was intended to deprive it of all its carbonic acid, and on leaving the globe it was drawn through a bottle containing lime-water, carrying with it, of course, the products of the combustion of the hydrogen. We first used, in order to absorb the carbonic acid completely from the air in which the hydrogen burned, two cylinders, 27 centimètres high and 5 centimètres in diameter, filled with fragments of pummice-stone, moistened with caustic potash; not satisfied with this large absorption surface, we next added to the cylinders two Wolfe bottles, containing a concentrated solution of caustic potash, and finally substituted for the Wolfe bottles three five-bulb potash-tubes. We repeatedly maintained a burning jet of hydrogen in the globe for periods varying from four to six hours, with air purified by passing through the three potash-tubes and two cylinders described, and always obtained the same result,—viz., there was never any perceptible cloud of carbonate of lime in the bottle containing lime-water during the progress of the experiment; but after standing twelve hours, an unmistakeable deposit of crystalline carbonate of lime was invariably found at the bottom of the lime-water. We might have regarded this as sufficient evidence of the presence of an infinitesimal amount of carbon in the hydrogen, had we not found, by repeated trials, that the burning of the hydrogen had no influence whatever on the formation of this crystalline deposit. 156,000 cubic centimètres of air (the contents of our aspirator), passed through the purifiers we have described, still retained sufficient carbonic acid to produce a deposit of crystalline carbonate of lime, when allowed time to separate from the lime-water by crystallisation.[1] In order to render the experiment irreproachable, with reference to the determination of carbon in the hydrogen, we should have been obliged to extend indefinitely our absorbing apparatus, and to force the air through the whole apparatus, instead of sucking it through, as in the experiments above described. The object in view was not worth such trouble and expense, and we, moreover, had not the necessary appliances, so that we were reluctantly forced to give up the inquiry.

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[1] The fact that carbonate of lime may at first be dissolved by lime-water, has been clearly shown by Vogel (*Schweigger's Journ. f. Ch. u. Phys.*, 1821, xxxiii. 207). It is moreover very distinctly affirmed in the following passage from an article which has recently fallen under our notice, by Berthollet (*Annales de Chimie*, 1789, iii. 68):—"I am indebted to M. Welter for an observation upon the use of lime-water, which may be useful in cases where one wishes to detect small quantities of carbonic acid. Lime-water has the property of dissolving a little carbonate of lime, of which one can assure himself by blowing into it with a tube; the air expired produces a cloud which re-dissolves entirely, until the lime-water is saturated with the car-

Our interest in the subject has been awakened anew by some experiments recently published by H. Karsten,[2] upon the oxidation of dry non-nitrogenous organic substances by the action of atmospheric air at ordinary temperatures. The method upon which Karsten chiefly relied in these experiments was, in its essential features, identical with the one employed by ourselves, but the difficulty to which we have referred, of removing carbonic acid from the air employed by any common absorption apparatus, is altogether ignored by him,[3] as will appear from the following description of his apparatus, quoted from page 349 of his article :—" In order to purify from carbonic acid and water the air which I allowed to flow in a slow, continuous stream over the organic substances, I placed before the vessel which contained them chloride of calcium tubes several feet in length, and in front of these a tube containing dry caustic potash, preceded by a bulb-tube filled with concentrated solution of caustic potash; by this solution the air was first washed and freed from carbonic acid; it was then led slowly over the dry caustic potash and through the long chloride of calcium tubes, before it came in contact with the organic substances, which had been dried in the water-bath." It is evident, at all events, that this apparatus was far less adequate than our own for the difficult operation of removing carbonic acid from the air.

We do not in the least seek to deny the truth of Karsten's assertion, that carbonic acid is really formed by the action of air at ordinary temperatures upon the substances in question. The fact is not only probable *à priori*, but would seem to be proved by his incidental statement (p. 348), that carbonic acid was formed when these compounds (sugar, cork, &c.) were exposed during some months to the action of air or oxygen in tubes sealed with mercury in the pneumatic trough. In so far as charcoal is concerned, De Saussure[4] has long ago shown the extreme probability that it is oxidised by the air, even when dry, as it is when wet. Neither do we wish to assert that it is impossible to deprive air of every atom of its carbonic acid. We insist only upon the facts, that it is a matter of no inconsiderable difficulty to do this, that Karsten's apparatus was entirely inadequate, and that nothing in his paper would indicate that he has allowed for this source of error.

It should be distinctly borne in mind that in the experiments of Karsten, as well as in our own, the question raised is not at all whether the amount of carbonic acid which escapes absorption can be estimated with the balance; for so long as the experiments are qualitative only, and conclusions are based upon the precipitate which is formed in lime-water, it is clearly necessary to remove every trace of carbonic acid from the air employed, no matter how "imponderable" this trace may be. We do not believe that the carbonic acid which escapes absorption in ordinary experiments can be of sufficient amount to be mentioned as a source of inaccuracy in the determination of the carbonic acid of the air, by the method which has been used by so many eminent chemists; for the extent of the error thus introduced must be far less than that of several others to which the absorption process, as commonly employed, is exposed, and which have been pointed out by Hlaziwetz,[5] and in part also by the brothers Rogers.[6]

So far as we know, those observers who have previously touched upon this subject have been occupied with quantitative considerations only. They have, therefore, very properly rested content when by experiment they have satisfied themselves that the last potash-tube of their series no longer increased in weight during the space of time occupied by a single experiment.[7] It must, however, be evident to any one who will perform the experiment, that the presence of an amount of carbonic acid which could not be detected by any weighing of potash-tubes may readily be made manifest by precipitating it as *crystallised* carbonate of lime. In this connection it should be mentioned that Brunner[8] has distinctly called attention to the extreme difficulty of completely absorbing carbonic acid from the air. Brunner could accomplish this neither by means of a solution of baryta, nor by a mixed solution of chloride of barium and caustic ammonia,[9] by bits of sponge moistened with baryta or lime-water, nor even by fragments of caustic potash, or asbestos moistened with a solution of potash; he finally chose slightly-moistened hydrate of lime as the best absorbent, and maintains that his method of determining carbonic acid in the air, by this means, is sufficiently accurate for all ordinary cases.[10]—*Proceedings of the American Academy of Arts and Sciences.*

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bonate of lime which has been formed. If, therefore, it is desired to detect small quantities of carbonic acid by means of lime-water, it is necessary to agitate some carbonate of lime with the latter, in order that it may be saturated, before filtering it." That the carbonate of lime separates, after a time, from the lime-water which had previously held it in solution has also been shown by one of us (*Am. J. Sci.*, 1858 [2], xxv. 42),—at that time entirely ignorant of the experiments of Vogel and Wolter.

[2] *Poggendorff's Ann. der Phys. u. Ch.*, 1860, cix. 346.

[3] The reader of Karsten's Memoir will observe that, like ourselves, he obtained for the most part only crystalline carbonate of lime, no immediate cloudiness.

[4] *Gilbert's Ann. der Phys.*, 1814, xlvii., 119, note.

[5] *Wiener Akad. Bericht*, 1856, xx. 189.

[6] *Am. J. Sci.* 1848 [2], v. 115; *Edin. New Phil. Journ.* xliv. 150.

[7] Compare, for example, Dumas and Stass, "Sur la véritable Poids Atomique du Carbone," *Ann. Ch. et Phys.* [3], i. 18.

[8] *Poggendorff's Ann. der Phys. u. Ch.*, 1832, xxiv. 571.

[9] Having had repeated opportunities of observing the great difficulty—not to say impossibility—of absorbing carbonic acid from mixed gases, especially if these contain so much as one or two per cent. of it, by means of these and similar liquids, I am glad to bear witness to the entire accuracy of this much-neglected statement.—F. H. S.

[10] It is a curious fact, which not only corroborates Brunner's observation, but also suggests a more extended use in the laboratory of his favourite absorbent, that manufacturers of coal-gas find in practice, that carbonic acid, when not present in very abnormal quantity, may be readily removed from the impure gas by passing the latter through several layers of dry hydrate of lime, spread in fine powder upon perforated iron grates, or upon shelves of basket-work ("dry-lime purification"); while it is practically impossible to absorb all the carbonic acid from similar gas by the wet system, in which the impure gas is forced through milk of lime, contained and agitated in several successive purifiers. Yet with the other chief impurity of coal-gas, sulphuretted hydrogen, the reverse of this is the case; for, by means of the wet-lime purification, this substance can, in ordinary cases, be very readily and completely removed, with an expenditure of but little lime, while with the dry purification this result is far less easily attained. Moreover, this non-absorption of carbonic acid in the wet-lime purifiers cannot be due to any interference caused by the sulphuretted hydrogen; for it is just as difficult to absorb all the carbonic acid from rosin-gas, which contains no sulphuretted hydrogen, as it is to absorb that in the gas prepared from coal.

Note on Sources of Error in the Employment of Picric Acid to Detect the Presence of Potash,[1] *by M.* Carey Lea.

Picric acid enjoys a high reputation as a test for potash. Employed in its alcoholic solution, or as soda or ammonia salt, sometimes as magnesia salt, it gives with potash solutions a dense yellow crystalline precipitate. If the solution containing potash be very dilute, the precipitate may not make its appearance till after some hours' repose, and it then forms long, delicate needles.

Rose remarks that this re-agent "is even more sensitive than the solution of chloride of platinum." In his summing up, he observes, that, of the various re-agents, chloride of platinum, tartaric acid, picric acid, perchloric acid, sulphate of alumina, and hydrofluosilicic acid, the latter is insufficient to distinguish between potash and soda; and that the chloride of platinum and sulphate of alumina give the same reactions with ammonia as with potash.[2] No qualification is made with respect to the certainty of the indications afforded by picric acid. Plattner makes the same observation as to the greater sensitiveness of picric acid compared with bichloride of platinum and equally without qualification as to its reliability.[3]

I therefore believe that the remarkable insolubility of otherwise soluble alkaline picrates in alkaline solutions has not been before pointed out. If an alcoholic solution of picric acid be poured into a solution of carbonate of soda, it occasions an immediate dense yellow precipitate, not to be distinguished in appearance from a precipitate of picrate of potash, and liable to be mistaken for it with the greatest facility. The picrate of soda formed in the case just mentioned is the most soluble of all the alkaline picrates, and *à priori* we should not expect to find it precipitated under these circumstances. An aqueous solution of picrate of soda added to one of carbonate of soda acts in precisely the same manner.

To ascertain if these reactions were extended to various compounds of picric acid, examinations were made which gave the following results:—

Alcoholic solution of picric acid added to—
  solution of carbonate of ammonia, gave an immediate dense yellow crystalline precipitate.
  ,, carbonate of soda gave not so instantaneous a precipitate, but one which, after standing, became even more dense.

Aqueous solutions of picrate of soda gave with
  solution of carbonate of ammonia }
  ,, sulphate of ammonia } abundant precipitates.
  ,, carbonate of soda }
  ,, phosphate of soda, slight precipitate.

Aqueous solution of picrate of ammonia gave with
  solution of carbonate of ammonia }
  ,, sulphate of ammonia } abundant precipitates.
  ,, carbonate of soda }
  ,, phosphate of soda, none.

Aqueous solution of picrate of magnesia gave with
  solution of carbonate of soda }
  ,, carbonate of ammonia } abundant precipitates.

When the alkaline solutions to be tested are otherwise than very dilute, there is absolutely no difference in the appearance of the precipitates. When the amount of the precipitate is small, the following differences may be observed:—

The potash precipitate forms longer needles, and when these are found only after standing, or when the precipitate is re-dissolved by heat and allowed to crystallise by slow cooling, they exhibit a beautiful play of red and green colours.

The soda salt is of a lighter and brighter yellow than either the potash or ammonia salt. Its needles are also shorter than those of either of the other compounds, and when the precipitate has been re-dissolved by heat, and allowed to crystallise by slow cooling, they tend to agglomerate themselves together in spherical masses. This is a very marked character, and when exhibited is unmistakeable, but is not always shown.

The ammonia salt when crystallised in quantities is very distinct in appearance from the potash salt; but when thrown down in small portions, even by slow crystallisation, cannot be distinguished from it with certainty. The prisms are flatter, but these differences are not apparent in very slender needles. The play of colours, which the ammonia salt exhibits to a less degree than the potash, disappears entirely in the soda salt.

The above reactions indicate that extreme caution must be used in employing picric acid as a test for potash. The precipitates above described do not re-dissolve in any excess of the picrate or of picric acid; nor is it to be supposed that concentrated alkaline solutions are necessary to cause them,—on the contrary, in many cases very dilute ones are sufficient. To determine within what limits such reactions are produced, the following trials were made:—

A. Dilution of the alkaline solutions. Limits obtained,

(1.) With carbonate of soda,

A solution containing 5 per cent. anhydrous carbonate of soda gave
  with alcoholic solution of picric acid, an immediate abundant precipitate; in ten minutes the tube could be inverted without spilling the contents.
  with solution picrate of soda, an immediate precipitate, becoming in the course of an hour a very dense one.

A solution containing 2½ per cent. carb. of soda gave
  with alcoholic picric acid, a slight precipitate, increasing very much by standing, so that after eighteen hours the tube could be inverted without spilling the contents.
  with picrate of soda, none.

A solution containing 1 per cent. carbonate of soda gave
  with alcoholic picric acid, slight precipitate, after eighteen hours' repose.
  with picrate of soda, none.

(2.) With sulphate of ammonia:—

A solution containing 5 per cent. anhydrous sulphate ammonia gave
  with alcoholic picric acid, immediate dense precip.
  with picrate of soda, ,, ,,

Solution containing 2½ per cent. sulph. ammonia gave
  with alcoholic picric acid, immediate dense precip.

Solution containing 1 per cent. sulph. ammonia gave
  with alcoholic picric acid, immediate dense precip.
  with solution picrate of soda, immediate slight precipitate. The solution, on standing, gave a beautiful crystallisation of long needles, with bright play of colours, exactly resembling the reaction of potash salts.
  with picrate of magnesia, by long standing, a very faint precipitate.
  with picrate of ammonia, none.

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[1] *American Journal of Science and Art.*

[2] Rose, "Handbuch der Analytischen Chemie," Band i. s. 6—10.

[3] Plattner, "Die Probirkunst mit dem Lothrohre," Third Edition, p. 178.

Solution containing $\frac{1}{250}$th anh. sulph. ammonia gave with alcoholic picric acid, after a short interval, an abundant precipitate.

Solution containing $\frac{1}{500}$th anh. sulph. ammonia gave with alcoholic picric acid, after a short interval, a considerable precipitate.

Solution containing $\frac{1}{1000}$th anh. sulph. ammonia, gave with alcoholic picric acid, no precipitate, even after twenty-four hours' repose.

B. Limits obtained with respect to indications of diluted solutions of alkaline picrate:—

A solution of picrate of ammonia, in 200 parts water, gave, with an equal volume of strong solution of carbonate of ammonia, an immediate precipitate of small, yellow needles. In ten minutes a considerable quantity settled at the bottom of the vessel.

A solution of same salt in 400 water gave, with an equal volume of solution carbonate ammonia, signs of a precipitate in a few minutes, and, after some hours, an appreciable quantity settled at the bottom of the vessel.

With 1 part picrate ammonia in 800 water no precipitate was produced by solution of carbonate ammonia even after twenty-four hours' repose.

The conclusion to be drawn from these results is,—

That alcoholic solution of picric acid or aqueous solution of picrate of soda will produce a precipitate in almost any alkaline solution, whether of soda, ammonia, or potash, except under circumstances of great dilution, especially if allowed to repose for twenty-four hours.

That picrate of ammonia and picrate of magnesia give the same results, but in a less degree.

That picric acid is, therefore, wholly unreliable as a test for potash, the results obtained being such as would tend altogether to mislead those who are not extremely familiar with the appearance of the precipitates, and that in some cases the results are so deceptive that even eyes most familiar with these reactions might be deceived; for example, in the result obtained above by testing a solution containing 1 per cent. sulphate of ammonia with solution of picrate of soda. In this case a crystallisation of picrate of ammonia was obtained perfectly simulating that of the potash salt.

Picric acid is, in fact, a better test for soda than for potash, because with a soda solution it gives a precipitate which, re-dissolved by heat, generally, but not always, gives a characteristic spherically-radiated, bright canary-yellow crystallisation, whereas the precipitate obtained from a potash solution can never be positively distinguished by its appearance from that afforded by an ammonia solution; and we have just seen that a solution containing $\frac{1}{200}$th of sulphate of ammonia, or even less, is capable of producing such a precipitate.

On the Microscopical and Chemico-Microscopical Characters of various Starches and Grain-meals, by John Horsley, *F.C.S.*

(Continued from page 163.)

Barley-meal.

Under Water, the starch granules are not unlike those of wheat, as at first sight to be easily mistaken for them, but on comparison it will be found that the smallest grains are considerably less in size than the corresponding ones of wheat, whilst here and there some of the larger ones have a slight cleft or groove running through them.

Under Acid Hydrochloric, the starch granules instantly dissolve, leaving behind cellulose matter, and on pressing the over-glass a little closer, globules of oil collect. Here I take occasion to correct an apparent omission when treating of wheat flour, which, like barley, contains oily matter, and would be rendered evident by hydrochloric acid.

Under Acid Sulphuric, the granules are rapidly but irregularly acted on at their edges, and the cellulose reddened.

Under Liq. Potass. Dil., the starch granules immediately swell out, then collapse and curl over, presenting the appearance of a slit or furrow down their entire length, and passing rapidly into a transparent jelly. By this test barley is readily distinguished from wheat, which is less rapidly acted on.

Under Liq. Ammon. F., no reaction beyond greater definition and the discoloration of the cellulose matter.

Under Polariscope, polarises very faintly.

Oatmeal.

Under Water, these starch granules are readily distinguished from every other by certain peculiarities. The larger ones are of a round or oval shape, rather dark shaded, of an aggregate or composite character, as of several smaller granules of a diamond-like or angular form being dove-tailed or clustered together.

Under Acid Hydrochloric, as was the case with barley, the starch is rapidly dissolved, but a larger quantity of oil is produced than with barley or wheat.

Under Acid Sulphuric, the peculiar reticulated appearance of the compound globules is instantly destroyed by the dispersion and gelatinisation of the diamond-shaped particles. The cellular matter and fibrous structure are also rendered beautifully evident, sometimes acquiring a deep-yellow or brown tinge, with patches of pink.

Under Liq. Potass. Dil., is instantly dissolved, forming a viscid but opaque jelly, and, besides the discoloration of the cellulose matter, some small oil globules are seen floating about.

Under Liq. Ammon. F., no reaction is produced beyond the clearer definition and discoloration of cellulose matter.

Under Polariscope, does not polarise.

Indian Corn, or Maize.

Under Water, but for their larger size, the granules of starch resemble the smaller or detached grains of oats, being, like them, angular or diamond-shaped, with cupped central depressions, and sometimes radiating lines.

Under Acid Hydrochloric, the starch granules of the whole-ground corn are rapidly dissolved, with the deep discoloration of the cellulose, and the production of a much larger quantity of oily or fatty matter than was previously noticed in other grains; but the starch in its pure state, free from colour and cellulose, as Polson's corn flour, or Oswego flour, readily dissolves, forming a transparent jelly.

Under Acid Sulphuric, the granules continue more or less intact for a long time; they, however, lose their angular form, and become changed to a flat-round, with a dark spot in the centre. Whole-ground Indian corn (both the white and yellow kind) becomes instantly discoloured of a deep golden-yellow by this re-agent.

Under Liq. Potass. Dil., the pure white starch swells up immensely, retaining its shape more or less for some time, becoming at last a viscid, transparent jelly. The impure or whole-ground kind has the cellulose changed to a deep yellow by this re-agent.

Under Liq. Ammon. F., no reaction, but, as in the previous case, a deep-yellow colour is imparted to the cellulose.

Under Polariscope, polarises very faintly.

Buck Wheat.

Under Water, these starch granules are polygonal, very regular in size, and not unlike those of rice, only somewhat larger, perhaps, between rice and Indian corn. Scarcely any markings are observable, but there is generally some brownish colouring-matter of a very finely-reticulated character visible.

Under Acid Hydrochloric, they are readily dissolved; the cellulose matter discoloured, and oil globules liberated.

Under Acid Sulphuric, the starch is rapidly dissolved and gelatinised, the cellulose matter becoming of very dark brown colour.

Under Liq. Potass. Dil., is rapidly acted on, forming a thick transparent jelly, the membrane or envelope surrounding the masses of starch granules (for in the original seed they are packed together in triangular or conical-shaped parcels), is readily recognised from its becoming highly discoloured by this re-agent.

Under Liq. Ammon. F., no reaction is produced, but the envelope and cellulose matter is rendered of a deep yellow colour.

Under Polariscope, does not polarise.

Rye Meal.

Under Water, consists of nearly equal proportions of very large and small grains, the larger ones being of a full moon-shape, or, rather more protuberant that wheat starch, whilst the smaller ones are of a polygonal shape. The surfaces of the larger ones are sometimes irregular and uneven, at other times a triangular slit is observable across the granule.

Under Acid Hydrochloric, the starch granules readily dissolve, leaving behind cellulose matter with a tolerable quantity of oil globules.

Under Acid Sulphuric, the granules rapidly dissolve and gelatinise, whilst the cellulose matter becomes almost black, with here and there a patch of bright pink colour.

Under Liq. Potass Dil., the starch is rapidly gelatinised, and the cellulose highly discoloured.

Under Liq. Ammon F., no particular reaction beyond the discolouration of the cellulose.

Under Polariscope, polarises but faintly.

Pea Meal.

Under Water, the starch granules are of various shapes, but are principally, when entire, (which is seldom the case with the meal of peas which have been desiccated by heat) of a flat round, long oval, or kidney-shape, of a pearly aspect, with either a deep straight furrow or a triangular slit or crack. They have also strong indentations, and sometimes wrinkled and granulated attachments. One great peculiarity is their concentric lines or markings which (if the grain is perfect and the light good) may be seen to commence at their edges as a kind of long horizontal ring or series of closely-fitting rings up to about the centre of the granule, which may be supposed to be the situation of the hilum, but none appears. In addition to this, the taste and flavour of peas are of easy recognition.

Under Acid Hydrochloric, the starch granules rapidly dissolve, and a very large quantity of oily or fatty matter is separated. The cellulose is also discoloured.

Under Acid Sulphuric, the starch granules retain their form more or less for some time, being (if perfect) but slowly acted on, the cellular matter rapidly acquiring a deep yellow colour, which is succeeded by a bright pink.

Under Liq. Potass. Dil., is gelatinised almost instantly, leaving only cellulose matter (which appears discoloured and full of pin-holes) undissolved.

Under Liq. Ammon. F., scarcely any reaction is produced, but definition is greatly improved, the cellulose matter, as with potash, being discoloured.

Under Polariscope, polarises a little.

Lentils (Ground).

Under Water, the microscopic appearance of the pure starch granules is so closely analogous to those of peas as to render any further description of them unnecessary, but in their mixed state, as meal, they are of reddish colour, by reason of the cellular matter, whilst pea-meal is yellowish.

Under Acid Hydrochloric, the starch granules readily dissolve, and, like peas, furnish a large quantity of oil globules.

Under Acid Sulphuric, reaction the same as with peas.

Under Liq. Potass. Dil., the same as peas, the starch being dissolved, and the cellulose discoloured.

Under Liq. Ammon. F., no reaction, only the cellulose being discoloured.

Under Polariscope, polarise faintly.

Bean Meal.

Under Water, although not unlike pea starch, yet the granules are sufficiently distinguishable by their greater regularity, both in size and form, being for the most part of a long oval or lozenge shape, with strongly-marked slits or furrows running down the granule, which is not unfrequently cracked or split in halves, or nearly so. The concentric lines, however, are certainly less distinct than in peas and lentils. The peculiar taste and flavour of beans are recognisable by the palate. Boiling them with water also developes their odour.

Under Acid Hydrochloric, the starch is readily dissolved, the cellulose matter discoloured, and a large quantity of oil globules liberated.

Under Acid Sulphuric, the reaction is similar to that of peas and lentils.

Under Liq. Potass. Dil., is rapidly gelatinised and the cellulose discoloured.

Under Liq. Ammon. F., no reaction, only the cellulose discoloured.

Under Polariscope, polarises faintly.

TECHNICAL CHEMISTRY.

On the Impossibility of Puddling Iron which contains Copper, by Dr. C. List.

It has been stated as a matter of belief among practical iron-workers in Germany, that pig-iron which contains copper cannot be puddled; assertions having even been made, that when one puddler wishes to annoy another he will sometimes throw a bit of copper—a small coin for example—into the furnace, so that the iron cannot be made to "rise."

Without giving full credence, as yet, to this statement, Dr. List mentions that he has observed two instances which go to prove that it may be correct. In the case which he has more particularly described, none of the phenomena which ordinarily occur when iron is puddled appeared. Some 400 lbs. of pig-iron having been placed in the furnace, were melted in the course of half-an-hour, at which time a sample taken from the molten mass was perfectly white, but the usual evolution of carbonic oxide, and consequent swelling or "rising" up of the mass of scales, &c., about the iron, did not ensue. On the contrary, by the time that the balling together of the iron should have commenced, it was evident that the charge could not be worked off; it was therefore removed from the furnace, after having remained there about three-quarters of an hour. As the melted metal was flowing out it emitted numerous beautiful blue sparks, which were also produced when the metal in the furnace was stirred. The sparks were regarded by the workmen as an indication that the iron contained copper.

The amount of metallic iron which remained weighed 240 lbs., 160 lbs. having been lost in the scales and slag. Analyses [for details of which see the original memoir] were made of the original pig-iron, (I.); of the sample taken, as previously mentioned, from the melted iron, as it lay beneath the scales (II.); and of the iron after it had been removed from the furnace (III.).

	I.	II.	III.
Silicon . . .	1·32	0·29	—
Sulphur . . .	0·28	—	0.20
Manganese . .	3·56	—	0·48
Copper . . .	0·35	0·38	0·57

It was evident, therefore, that the 400 lbs. of pig-iron used did really contain nearly a pound and a-half of copper. It appears, moreover, that copper cannot be removed from iron by puddling. Calculating how much copper ought to be left in the iron which was finally removed from the furnace, in case none had been lost in the slag, it is found that there should be 0·58 per cent., almost exactly the quantity which was obtained in analysis No. III.—*Dingler's polytechnisches Journal.*

On the Preparation of Oxide of Lead free from Copper and Iron, by T. Wichmann.

Of the many plans which have been proposed for utilising the sulphate of lead which is formed in considerable quantity as a secondary product in the preparation of alumina mordants at print-works, the method of reducing it in furnaces to the metallic state has been most frequently resorted to, since the quality of the lead thus prepared is of peculiar excellence. Instead of thus reducing the sulphate of lead, Wichmann proposes to convert it directly into oxide of lead, maintaining that the oxide thus prepared, being uncontaminated with copper or iron, will be well suited for the manufacture of white glaze for pottery—(probably also for making flint glass)—it being somewhat difficult to obtain in commerce oxide of lead which does not contain traces of these impurities. In his method, suggested by that of Mohr[1] for preparing caustic baryta, a quantity of caustic soda lye, of from 28° to 30° B. = about 1·25 specific gravity, having been brought to boil in an iron kettle, the sulphate of lead is added to it little by little, with constant stirring, oxide of lead and sulphate of soda being produced. The amount of sulphate of lead which shall be added to a kettleful of soda, and which is somewhat less than the quantity which theory would indicate, must be determined once for all by direct experiment,[2] sulphate of lead being added until the sharp caustic taste of the soda is no longer manifest. It is well, however, to leave some free soda in the solution, lest a portion of undecomposed sulphate of lead should contaminate the oxide of lead which is formed, although a small amount of the latter will combine with the soda and remain in solution. During the boiling the decomposition is rapid and complete, unless the soda lye contained much carbonate of soda, in which case a white scum of carbonate of lead will separate.

When the proper quantity of sulphate of lead has been added to the lye, and the decomposition is finished, the contents of the kettle must be thrown into a vessel containing water, in order that the sulphate of soda which has been formed may be retained in solution, which would not be the case if no water were added to the mixture. The oxide of lead, which is in the form of exceedingly fine crystalline scales of a light yellowish red colour[3] and of great purity, is readily deposited; after being thoroughly washed, to remove the sulphate of soda, it is dried and finally ignited in a reverberatory furnace—the charge being subsequently allowed to cool as slowly as possible.

As thus prepared, the oxide is an exceedingly soft powder of a yellowish-red colour like prepared litharge; it contains about 2 per cent. of alumina, which cannot readily be removed, but which is not at all detrimental to its use in glazing pottery, and also a larger or smaller quantity of carbonate of lead, according as the soda lye was more or less contaminated with carbonate of soda.

The crude sulphate of lead, as it comes from the print-works, should be agitated and washed with water—or, better, forced through a fine sieve, in order to break up lumps, and to remove any foreign substances—before being used.

The residual solution of sulphate of soda being evaporated, together with the first portions of the wash-water, affords Glauber's salt, containing alumina, plumbate of soda, acetate of soda (the acetic acid and alumina being both derived from the crude sulphate of lead), and chloride of sodium (from the soda lye); being free from iron, it is suitable for glass-makers' use.—*Polytechnisches Centralblatt.*

On Cleansing Mordanted Cloth before Dyeing, by J. Lœwenthal.

It is well known that cotton cloth upon which alumina or iron mordants have been printed must be submitted to the so-called dunging process before it can be dyed.

The action of the ingredients used for dunging is:—1, To completely fix the mordants; 2, to remove the substance (starch, gum, &c.) with which the mordant was thickened for printing; (3, as well as to remove the excess of mordant, and by combining with it prevent it from soiling those portions of the cloth which are to remain uncoloured.—E. Kopp.)

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[1] To a boiling solution of caustic soda, of 1·10 to 1·15 sp. gr., of known strength, an equivalent weight of powdered nitrate of baryta is added. As soon as the baryta salt has completely dissolved, the hot solution should be rapidly filtered in order to separate any sulphate or carbonate of baryta which may have been formed from impurities of the soda, and the filtrate set aside in a cool place. An abundant crop of crystals of hydrate of baryta soon forms. These crystals are to be drained from the solution of nitrate of soda, upon a funnel loosely plugged with cotton and the drying completed [according to Mohr] by means of a centrifugal machine. If required for purposes where the adhering nitrate of soda should be avoided the hydrate of baryta may be re-crystallized. In place of the nitrate of baryta one may use chlorid of barium—or in place of the soda, potash; but as a rule the presence of chlorine in the product would be more objectionable than that of nitric acid. On the small scale the process of boiling is best conducted in a glass flask, but where larger quantities are desired a covered iron kettle may be used—as in the common method of preparing caustic alkalies.—From *Archiv der Pharmacie,* lxxxviii. 38; in *Chemisches Centralblatt,* 1856, [N. F.,] i. 834.

[2] For a ready method of obtaining the weight of moist sulphate of lead, see Mr. F. Mayer's paper in the March No. of the *American Journal of Science.*

[3] If a more concentrated soda lye be used, say of 40° B., somewhat larger crystals of a dark red colour are formed.

Giving special prominence to the second of these desiderata, that of removing the thickening agent, Lœwenthal proposes to employ diastase, as contained in malt, in order to convert the starch into sugar, thus rendering it readily soluble so that it can be easily removed from the cloth.

By direct experiment he has satisfied himself that the addition of malt to baths of bran (previously boiled) or of cowdung is decidedly beneficial; cloths which have been printed with an alumina mordant thickened with starch being very rapidly cleansed in this manner. With iron mordants a somewhat longer time is required than with those of alumina, but they are nevertheless cleaned much more rapidly than when no malt is used.

A temperature of 35° to 40° R. [= 111° to 122° F.] is sufficient that the malt shall act powerfully.

When the dunging-bath contained water-glass (sesquisilicate of soda) the use of malt gave rise to the formation of spots upon the cloth in the process of dyeing. The author thinks, however, that this objection might perhaps be obviated if purer diastase were used instead of malt.

In this connection Barreswil remarks that he has employed pepsine with success in removing colours fixed with albumen.—*Journal für praktische Chemie.*

On the Value of Different Kinds of Soap,

by R. Graeger.

Complaints of consumers in regard to the value, or rather efficacy, of samples of soap, which to the best of the manufacturer's knowledge have been well prepared, are not uncommon.

It is very probable that the usual explanation which is offered, whenever a soap fails to fulfil the expectations of its consumer, viz. that it contains too much water, may be in many cases correct. Admitting this, and various other contingencies, which are of importance in deciding upon the value of a soap, there appears to be another obvious reason why different soaps containing equal amounts of water may still possess different degrees of efficacy.

It is evident from the different equivalent weights of the various fatty acids, that the amounts of caustic alkali taken up by them in the formation of soap must be of unlike magnitude.

If it be true, that the detergent power of soap is entirely dependent upon the amount of alkali which it contains, of course it follows that those soaps which contain the largest proportion of alkali,—or in other words, those containing a fatty acid, the equivalent weight of which is small,—must be the most efficacious.

Since the difference between the equivalents of the common fatty acids are not large, these considerations are perhaps of little or no importance in so far as concerns the consumption of soap in household economy—the total amount used in a single family being but small. In a manufacturing establishment, however, where fifty or a hundred thousand pounds of soap may be used in the course of a year, differences which cannot be deemed insignificant must exhibit themselves.

For example, the equivalent weights of several soaps (regarded as anhydrous), in common use, are as follows:—

Oleic acid (red oil) soap . . = 3800·95
Palm oil „ . . = 3588·85
Tallow „ . . = 3300·95
Cocoa-nut oil „ . . = 3065·45

Calculating from these weights how much of each of the other soaps would be required to replace 1000 pounds of tallow soap, the following quantities will be found:—

Pounds of			Per cent.		
1151	oleic acid soap,	*i.e.*,	15·1	more than tallow soap.	
1087	palm oil „	*i.e.*,	8·7	„	„
928	cocoa-nut oil „	*i.e.*,	7·2	less than	„

Differences like these must certainly be of importance in practice; and could, doubtless, be detected by direct experiment, if any one would undertake a comparison of the various kinds of soap—a research which would not be easy, however. — *Bœttger's polytechnisches Notizblatt.*

PHYSICAL SCIENCE.

Early Researches on the Spectra of Artificial Light from Different Sources.

Now that the attention of scientific men is being drawn to the method of analysis by means of spectrum observations, our readers will feel an interest in knowing that many of the observations which are now being followed up by Continental *savans*, have been investigated in a more or less perfect manner by English experimentalists. Wheatstone and Talbot, whose names have been unaccountably passed over in silence by recent writers on the subject, have, in fact, observed and recorded so much connected with the spectra of artificial light from different sources, that we have thought it advisable to collect together some of the more important results which these experimentalists have obtained. They will be read with more interest, inasmuch as Professor Wheatstone has placed at our disposal several papers and memoranda of experiments made by him nearly thirty years ago, many of which have either not been published at all, or have only appeared in abstract in the *Transactions* of learned Societies.

The first public account of any of these researches was communicated by the Professor to the British Association for the Advancement of Science, at their meeting in Dublin in August, 1835. With the exception of a short abstract of the paper which was furnished by the author for the volume of *Transactions* published by the Association, the only account of this paper which appeared was a short notice in the "Proceedings of the Fifth Meeting of the Association"—a local publication, —issued without the sanction of the Sections, and consequently uncorrected by the authors. In this it states that on the 12th of August, 1835:—

> "Professor Wheatstone detailed the results obtained by him when attempting to form by the prism a spectrum or coloured image of the spark obtained from an electro-magnetic apparatus, as also of the sparks produced under various circumstances from simple galvanic influence, and from an electrical machine. In the spectrum or coloured image formed by a beam of solar light when analysed by a prism, the colours succeed each other without any interruption, except that when the prism is of very good glass, certain dark bars cross the image at fixed though unequal distances.
>
> "Mr. Wheatstone examined the light produced by a

revolving electro-magnet, expecting to find something of a similar effect; but to his surprise he found that the simple coloured lines that presented themselves were separated by great intervals, in which no light whatever could be seen,—and this with such regularity as to afford a test, not only of the substance through the instrumentality of which the light was procured, but also a test of the strict identity of the light produced by galvanism and by electro-magnetism, and a proof that the light of common electricity was essentially distinct from either. When mercury was used as the means of producing the spark, Professor Wheatstone found the series of colours and their distances from each other, to be readily distinguished from the series and distances of the colours resulting from the spark produced when any other metal was used; and the same circumstances for each of the other metals which he was able to use in the fluid state, were so characteristic as to afford means of distinguishing them. He also found that the light produced by the combustion of each of these metals was so dissimilar from that produced from the electro-magnetic, galvanic, or electric spark, that Professor Wheatstone came to the important conclusion that these sparks cannot result from the combustion of the metals, but rather from a portion of the metallic conductor carried off by the electric discharge and ignited. This communication created much interest; and Professor Wheatstone was kind enough to promise on a future occasion to exhibit these curious appearances to any members of the Association who might feel an interest in the subject. This he accordingly did on two succeeding occasions, when we had the pleasure of seeing the various phenomena, and had an opportunity of observing how very accurately the Professor had detailed the appearances."

The following abstract was prepared by Professor Wheatstone himself for the purpose of being included in the official account of the transactions. It is taken from the fifth report of the British Association for the Advancement of Science, 1835, page 11 of the "Transactions of the Sections":—

"The following is a brief notice of the principal results stated in this communication: 1. The spectrum of the electro-magnetic spark taken from mercury consists of seven definite rays only, separated by dark intervals from each other. These visible rays are two orange lines close together, a bright green line, two bluish-green lines near each other, a very bright purple line, and lastly a violet line. The observations were made with a telescope furnished with a measuring apparatus; and to ensure the appearance of the spark invariably in the same place, an appropriate modification of the electro-magnet was employed. 2. The spark taken in the same manner from zinc, cadmium, tin, bismuth, and lead, in the melted state, gives similar results; but the number, position, and colours of the lines vary in each case. The appearances are so different that, by this mode of examination, the metals may be readily distinguished from each other. A table accompanied the paper, showing the position and colour of the lines in the various metals used. The spectra of zinc and cadmium are characterised by the presence of a red line in each, which occurs in neither of the other metals. 3. When the spark of a voltaic pile is taken from the same metals still in the melted state, precisely the same appearances are presented. 4. The voltaic spark from mercury was taken successively in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, &c., and the same results were obtained as when the experiment was performed in the air or in oxygen gas. The light, therefore, does not arise from the combustion of the metal. Professor Wheatstone also examined, by the prism, the light which accompanies the ordinary combustion of the metals in oxygen gas, and by other means, and found the appearances totally dissimilar to the above. 5. Frauenhofer having found that the ordinary electric spark examined by a prism presented a spectrum crossed by numerous bright lines, Professor Wheatstone examined the phenomena in different metals, and found that these bright lines differ in number and position in every different metal employed. When the spark is taken between balls of dissimilar metals the lines appertaining to both are simultaneously seen. 6. The peculiar phenomena observed in the voltaic spark, taken between different metallic wires connected with a powerful battery, were then described, and the paper concluded with a review of the various theories which have been advanced to explain the origin of electric light. Professor Wheatstone infers from his researches that electric light results from the volatilization and ignition, not combustion, of the ponderable matter of the conductor itself—a conclusion closely resembling that arrived at by Fusinieri from his experiments on the transport of ponderable matter in electric discharges."

The original paper, of which only the above brief abstracts were made, was never published. Engrossed by his more important telegraphic researches, the Professor, after reading it before the meeting, put it on one side, intending to continue the experiments when he had more leisure, and from that day to the present it has scarcely seen the light until it was placed by the author in our possession. Subsequent researches of the Professor on the same subject might have been added to it, and the illustration, which is an exact copy of the one exhibited at Dublin, might have been enlarged and improved, but it has been thought better to print it *verbatim et literatim* as it was read before the Association, and to embody the subsequent observations in a separate paper which will shortly follow in this Journal. The complete paper will be given in our next number.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

Lecture V.

Crystallization—Influence of Cohesion on Affinity—Precipitation—Normal Constituents of Water as it Occurs in Nature—Sources of Impurity in Water for Domestic Purposes.

We have seen that when certain solid substances are placed in contact with water they undergo a gradual disintegration and finally disappear altogether, in other words, they *dissolve*. The cohesion between their particles is broken up, and the cohesion between those particles and the particles of water becomes stronger than that between the solids themselves. If by evaporation we reduce the quantity of water present we recover the substance in a solid form, but in thus passing from the liquid to the solid form it generally undergoes the process of crystallization; it assumes certain general geometrical shapes which we term crystals. The same effect is generally produced when, instead of evaporating the water, we let the substance dissolve at a higher temperature, for generally speaking the higher the temperature of the water the more solid matter will it dissolve. There are a few exceptions to this rule, but they are very few. There are several other modes of forming crystals besides this one of solution; and the solution, I need scarcely say, may take place in any other solvent besides water. When a solid body is formed by evaporating a solution, the body in thus passing from the liquid to the solid state, assumes a crystalline form, as a general rule. So in passing from the gaseous state to the solid state it assumes a crystalline form In the case in which we produced a solid from two gases in a previous lecture we should have seen, if we had closely examined the substance, that it was in the crystalline form. However, at present it will not be convenient to consider this mode of forming crystals more in detail. The crystals which separate from water generally carry along with them, or combine with, at the time of crystallization, a certain quantity of that liquid which afterwards exists in the solid crystal. Some of these crystals when exposed to the dry air lose a portion or the whole of the water with which they have combined during crystallization, they fall to powder, and are said to *effloresce*. On the other hand, some crystals will attract a larger quantity of water, and will actually dissolve in the water which they thus attract from the atmosphere. Such substances are said to be *deliquescent*. Here we have in this vessel a sample of both of these kinds of salts. There is the first on the floor which is parting with its water to the air, whilst the other is absorbing the water which is given off by the salt on the floor, and the air in this jar is kept dry by the action of the deliquescent salt. This lower salt will finally crumble into a fine powder, and the other will dissolve in the upper part of the apparatus.

But there is another point connected with crystallization that we must just notice in passing before leaving this part of our subject; it is this—that crystals, in being formed by any of the processes to which I have just alluded, are liable to assume two or more forms—very frequently two forms. They crystallize in two shapes, and such bodies are said to be *dimorphous*, or crystallizable in two forms. One of these forms is generally less stable than the other—less firm in the cohesion which knits particles together; and this less stable form very frequently resolves itself into crystals of the more stable form. We have a curious instance of this kind of crystallization—this dimorphism—in iodide of mercury, some of which we have spread here upon a sheet of paper. This salt is capable of crystallizing in two forms; one of those forms is stable, or permanent, and the other is comparatively unstable. These scarlet crystals we have upon the paper constitute the stable form of this salt; but if we apply heat to it by means of this spirit-lamp I think we shall find that we shall transform this scarlet salt into a beautiful yellow salt. [The lecturer passed the plain side of the sheet of paper backwards and forwards at a short distance above the flame of the lamp. The change of colour gradually became apparent.] I want to get a larger portion of this transformed, I think we have now a sufficient surface transformed. You see the difference—the remarkable difference between the colour of the two forms—the two crystalline forms of this same salt. Now, the yellow salt we have thus obtained is an unstable crystal; and if I were to scratch one of these yellow crystals with a pin's point it would be transformed into a mass of scarlet crystals, and the scarlet crystallization would propagate itself over the whole surface; that, however, would require hours to take place. You see wherever I draw a line across this sheet of yellow crystals that line is red again. Wherever this pressure is applied, there they crumble down into the scarlet form.

A similar alteration in crystalline form, and sometimes accompanied also by remarkable changes of colour, occurs when salts are made to crystallize with different amounts of water in them. Here, for instance, is a drawing of a cockatoo, and it is coloured with some preparations of cobalt. The colour you see is a very faint pink, so faint, indeed, that it is hardly visible. Now we will expose this to heat so as to alter the crystalline form of those salts with which it has been painted; and we will see whether we produce any effect upon our cockatoo. [The picture was warmed before the fire.] There, I think we have effected a considerable transformation in our bird. You see the pink salt with which the wings were painted has become intensely blue, whilst the pink of the other parts, which were painted with nitrate of cobalt, has become much stronger. The change is owing to the different forms of crystallization reflecting different rays of light. The crystals on the wings which before reflected the pink rays now reflect the blue rays.

This separation of solid substances from solution is due to the action of a force called *cohesion;* and this force under such circumstances very frequently exercises considerable influence upon chemical affinity. It is possible, in fact, so to arrange the affinities of two different compounds that when mixed together a mere touch—the slightest exertion of this force of cohesion—will be sufficient to determine the decomposition of those bodies and form two new compounds. For instance, I will pour upon a glass plate here—upon a smooth glass plate—a mixed solution of acetate of potash and tartrate of soda. You see they remain perfectly transparent when first mixed; and I hope we shall be able to get them on the glass still in a perfectly transparent condition. [Some of the solution was then poured over the surface of the glass.] You see this solution is still transparent, I want to get it as much spread over the glass as possible; and now the salts are in such an unstable state that I can determine a new form of combination simply by the touch of my finger. I have written there the word "cohesion" upon that glass; and I dare say that the simple motion of my finger over the glass, through presenting surfaces favourable to the exertion of this force, cohesion, will be sufficient to produce the figure of that word distinctly developed. We shall have a bitartrate of potash crystallized upon the surface of the glass. I will pour off the residual fluid, and here you have the word distinctly brought out. [The word "cohesion" appeared, written in white crystals on the glass plate.] Let me explain this action a little more clearly. We had, in the first place, acetate of potash and tartrate of soda; and simply by this action of cohesion we get an entire reversion of the combination. Here [in the residual fluid] we get acetate of soda; and there where the word is written is tartrate of potash by itself, simply owing to the insolubility of the tartrate of potash. Here we have the cubes which will show us the reaction. This [arranging the cubes according to the original compounds] was the state of affairs before this trifling amount of cohesion was brought into play. This mere movement of the finger through the solution effected a transfer of the acetic acid from the potash to the soda, and of the tartaric acid from the soda to the potash.

Now, this modifying influence of cohesion upon chemical affinity is due entirely to the formation of a body comparatively insoluble in the liquid in which this force is made to act; and we have a great number of chemical decompositions which occur in solution almost entirely through this action of cohesion.

If, by any possibility, on the mixture of two or more solutions, an insoluble body can be produced by the mixture, that body will be almost sure to be formed. For instance, here we have a clear and transparent solution of acetate of lead, and here we have a similar transparent solution of chromate of potash. I pour them together,—you see we have the chromic acid leaving the potash, and combining with the lead of the acetate of lead. We get a yellow-coloured insoluble compound, which, upon examination, we should find to be chromate of lead. The chromic acid has gone over to the lead, and the acetic acid has gone over to the potash. To give yourselves a clear idea of the mode in which this takes place, you must imagine—which, in fact, can be proved to be the case—that when you mix these solutions you have there not only two salts, but four salts. There is, then, a division of the two acids between the two bases, and of the two bases between the two acids. There would then be produced acetate of potash and acetate of lead, and chromate of potash and chromate of lead. The chromate of lead is, however, insoluble; it is thrown down, a further portion of the acetic acid combines with some more of the potash, forming more of the acetate of potash; the chromic acid and lead thus set at liberty produce a further quantity of the chromate of lead, which, like that first formed, is precipitated; and so the decomposition continues till, in a time too short to be appreciated, all the chromate of potash and lead have united, and the compound is precipitated, leaving only the acetate of potash in solution. That this really takes place quite independent of the action of cohesion may be proved by adding together colourless salts which are not capable of producing in this way a solid salt. For instance, I will add to a solution containing perchloride of iron, another solution containing sulphocyanide of potassium. If this action of which I have been speaking takes place the sulphocyanic acid will divide itself between the iron and the potassium. If this be the case, then, sulphocyanide of iron will at once be formed, and if this be formed we shall see it by its colour, which is an intense blood-red. [The solutions were then poured together.] Here we have it, proving that we have really this transfer between the acids and the bases quite independent of precipitation; for the mixed liquid is quite clear, and might be filtered without leaving any trace upon the filter through which it passed.

Water, in its chemical relations, is what is termed a neutral body—a neutral substance. It possesses neither acid nor alkaline reaction. Although it is capable sometimes of playing the part of an acid, and sometimes of a base, yet it is, in its reactions what chemists call a "neutral substance." Let me explain what I mean by this. All solutions are said to be acid when they possess the property of reddening litmus paper, paper which is coloured blue by an infusion of litmus, a kind of colouring matter which is obtained from a description of lichen. If I dip this paper into a solution which reddens it I know at once that the solution has an acid reaction. If I first redden the paper with acid and then immerse it into an alkali the blue is restored, or, if instead of using the litmus paper, I take this yellow turmeric paper and dip it into a solution which browns it, this proves alkalinity of reaction. By dipping the browned turmeric paper into an acid solution the yellow colour is again restored. Now, solutions which do not manifest either of these properties—either the reddening of the litmus, or the browning of the turmeric—are said to be neutral. Such a solution we shall get, I dare say, by pouring the acid solution into the alkaline solution, if we have the proper strength. [The acid and alkaline solutions were then poured together.] If we have now produced a neutral solution it will have no effect upon our papers. It will not affect the blue nor the red litmus; and it does not affect the turmeric paper. It is *neutral*, in fact. So by these simple tests we can ascertain at once whether a solution is acid, alkaline, or neutral.

Water is, however, as I have just mentioned capable of uniting both with acids and with bases. It unites with such acids as sulphuric acid and nitric acid with so much force that it is almost impossible to separate it again, except by indirect means, from these acids when it has once combined with them. With phosphoric acid also, for instance, it unites with such force that we can expose the hydrate of phosphoric acid to a most intense heat without driving off the water from the compound. Water plays the part of an alkali or base, when it unites with an acid, and of an acid, when it unites with an alkali. Water sometimes unites with salts, and it then constitutes what is called the *water of crystallization*; and when water is thus combined with salts in a crystalline form, as with that carbonate of soda, for instance, and with this sulphate of soda, although these salts have the temperature of this room and we can raise their temperature without liquefying them, still the water which is combined with them is not liquid water, but is contained in the salts in the solid form of ice. Now, I think we shall be able to demonstrate that. If the water be in the position of ice it must be capable of absorbing a large amount of heat when the salt is made to dissolve in any liquid. The ice will then pass into the liquid condition, and we have seen that ice in passing into the liquid condition absorbs a great amount of heat—142 degrees. In the same way, if it be really ice it must give out as much heat when it crystallizes as it is afterwards capable of absorbing when it is subsequently dissolved. Now, here is some of this same salt, the sulphate of soda, reduced to a rather fine powder, and I will cause it rapidly to dissolve by adding to it an acid which will not merely dissolve it but also decompose it. I shall add to it some hydrochloric acid; and we will now place it opposite this thermo-electric pile. If we have cold produced, the red end of the needle moves towards you; if we have heat produced, we have an opposite effect; then the blue end of the needle moves towards you. You see if I place this vessel containing sulphate of soda in front of the pile it has no effect before the acid is added. We will now make the same experiment with the hydrochloric acid poured upon the salt. This acid itself has the same temperature as the room; it fumes, but the fuming is not due to heat; it merely arises from its combining with atmospheric moisture. Now we will see if we get any effect upon that magnetic needle. You see it is beginning to move; the red end is moving towards you. We have cold produced here; we are liquefying the ice in the sulphate of soda. It is now no longer ice; it is water, and it has absorbed 142 degrees of heat, from the hydrochloric acid, from the water of solution, and from its own self. Now let us see whether we can get a similar evidence of the heat coming out of the water when it becomes solid. This sulphate of soda is peculiarly capable of demonstrating this point, for it can suspend its crystallization. If you take a saturated solution of this sulphate of soda you can allow it to get perfectly cold without crystallising. Here is such a super-saturated solution, and if we just throw into it a crystal of sulphate of soda the crystallization of this whole mass is determined. [The flask containing the sulphate of soda was placed opposite the pile.] I want you to see that at the present time this flask does not affect the pile. If we have heat produced by the crystallization of this solution the blue end of the needle will move towards you, and the red end towards me. [The piece of bladder covering the mouth of the flask was then cut.] There, even by the admission of air, and by the slight tremor caused by the cutting of the bladder at the top of the neck, the crystallization has commenced. You see how beautifully these crystals are shooting down. The mass is gradually solidifying. The water is, in fact, solidifying, and you can see the expansion caused by that action. The needle is also coming round, indicating an elevation of temperature.

Here we have had, in one case, water cooled by the melting of a salt; and in the other case heat emitted by the solidifying of a salt in solution.

(To be continued.)

CHEMICAL SOCIETY.

Thursday, March 7, 1861.

Dr. Hofmann, *F.R.S., Foreign Secretary, in the Chair.*

Mr. J. J. Coleman was elected a Fellow.

Professor Field read a Paper "*On some Minerals from Chili.*" Black oxide of copper was for a long time known only in Vesuvian lavas, until its discovery in North

America, in the Valley of the Mississipi, and on the borders of Lake Superior. The author met with a dark-brown earthy mineral at the extreme north of Chili which contained 43 per cent. of black oxide of copper, mixed with carbonate of lime and horn blende. He also met with, a few miles north-west of Coquimbo, a black, amorphous, earthy mineral, which proved to be sulphate of lead, coloured with protoxide of iron. Its specific gravity was 6·20. It contained 96·7 per cent. sulphate of lead, and 3·2 per cent. protoxide of iron. Fibro-ferrite occurred in botyroidal masses, each nodule being built up of fine, radiating, silky fibres, of a pale golden-green colour. Its composition corresponded to the formula $Fe_2O_3\ 2SO_3\ 10HO$. It lost 2 equivalents of water by a comparatively short exposure to air, after which its weight remained permanent. It retained 3 atoms of water when dried at 212° F., and became anhydrous between 500° and 600° F. When acted upon by hot water it was decomposed, with separation of an insoluble compound, according to the equation, $3(Fe_2O_3\ 2SO_3\ 10HO) = 2Fe_2O_3\ SO_3 + Fe_2O_3\ 3SO_3 + 2HO\ SO_3 + xHO$. The insoluble compound is also found native. Bournonite was found in South America for the first time in 1858, by the author, in a mine near Huasco, in the northern part of Chili. Its composition agreed exactly with that of Cornish bournonite, and was represented by the formula $Cu_2S\ PbS\ SbS_3$. The author found that when sulpharsenites were treated first with strong nitric acid, and then with an excess of potash and an alkaline hypochlorite, the sulphuric and arsenic acids formed could be completely dissolved away from the insoluble oxides of copper, nickel, &c., but that the separation of antimony from sulphuric antimonides, by the same process, was very imperfect. Lastly, the author described a new double sulphide of lead and copper, which he termed Alisonite, having the formula, $3Cu_2S\ PbS$, Chili cuproblumbite being $Cu_2S\ 2PbS$.

Dr. Hofmann gave an account of Mr. Greiss's further researches upon Nitrogen Substitutions.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

PHYSICAL AND MATHEMATICAL SECTION.

February 28, 1861.

Mr. G. V. Vernon, F.R.A.S., read a Paper, "*On the Irregular Oscillations of the Barometer at Manchester.*"

This Paper is an addition to one upon the same subject brought before the Society by Mr. Baxendell, F.R.A.S.

The tables appended to it give the total amount and number of the barometer oscillations at eight a.m., for each month from 1849 to 1860, inclusive; the mean daily amount; the monthly fall of rain, and its difference from the average of twelve years, and Dr. Dalton's forty-seven years' average. For the month of August, the Author was indebted to the kindness of Mr. John Curtis, in supplying him with barometer observations, which were deficient in his own register.

The maximum amount of oscillation takes place in January, and the minimum in July, to which may be added a second maximum appearing to take place in October. The mean total amount of oscillation for the year is 61·410 inches, and the total number 175·5. It appears that a fall of rain, in excess of the average, is generally accompanied by an increase in the amount of oscillation. In the months of February, April, May, June, July, October, and December, a temperature below the mean for the month appears to increase the amount of oscillation; whilst in the remaining months of January, March, August, September, and November, the opposite appears to hold good. A number of oscillations above the average appears to be accompanied by a greater fall of rain than that accompanying a number of oscillations below the average, in the months of January, February, March, June, October, November, and December. In the remaining months of April, May, July, and September, a number of oscillations above the average appears to be accompanied by a diminished fall of rain.

If the month of June is regarded as abnormal (which is probable as I have no observations for this month in 1851, 1857, 1859, and 1860), we appear to have two distinct laws, one of which holds good in the winter months, and the other its direct converse in the summer months. Taking the entire year, these two laws appear very nearly to balance one another.

Mr. Baxendell, F.R.A.S., read a Paper "*On the Irregular Oscillations of the Barometer at Lisbon.*"

In this Paper the Author gives the results derived from a valuable series of barometrical observations made at Lisbon during the twelve years 1849-60, by John Martin, M.D., and kindly presented by him to the Section through Mr. Mosley. These observations were made daily at nine a.m., the barometer being permanently fixed at about fifty feet above the mean level of the Tagus, and the readings uniformly reduced to the temperature of 32° Fahrenheit.

From the tables which accompany the Paper it appears that at Lisbon the maximum amount of oscillation occurs in January, and the minimum in July, precisely as in the British Islands, and therefore agreeing with the law of disturbance announced by the Author in a former Paper. There is a small maximum in March, and a rather sudden and considerable increase of disturbance in October. The mean annual amount of oscillation is 31·686 inches, and the mean annual number of oscillations 156·9. The mean range and mean duration of oscillation are both greatest in winter, and least in the summer months. The greatest range occurs in January, but the greatest duration of oscillation is in November. It is remarkable, too, that the range is also greater in November than in any other month except January. These results for November are probably due to the great barometric wave which Mr. Birt some years ago pointed out as frequently occurring in or about this month.

In England the mean duration of oscillation is greater in the summer than in the winter half of the year, and, assuming that the breadth of a barometric wave is some function of the time taken to complete an oscillation, it follows that at Lisbon the areas of disturbance or breadths of the barometric waves are greatest in the winter and least in the summer months, whilst in England, on the contrary, they are greatest in the summer and least in the winter months. It seems probable, therefore, that at some intermediate latitude they are the same in both halves of the year, and this latitude may possibly be the nodal line separating a zone of high barometer from one of low barometer.

Comparing the six years of greatest amount of oscillation with the six years of least amount, it is shown that in years when the total amount of oscillation is above the average, the distribution of the increase in the different months is by no means in the proportion of the numbers representing the mean daily range of oscillation. The increase is much greater in the winter than in the summer months, but in April and September there is a diminution instead of an increase in the amount of oscillation. The total increase for the six winter months is 20·84 inches against 4·06 inches for the six summer months, while the total amounts of disturbance for the respective periods are in the ratio of 126·13 to 76·44. The Author adds, that the ratio of the amounts of increase in the winter and summer halves of the year appears to increase rapidly with increase of latitude; thus at Lisbon it is as 5 to 1; at Brussels as 6 to 1; and at Stockholm as 8½ to 1.

Mr. Atkinson exhibited a Chart, showing at one view the barometric oscillations during the month of February last, as observed by him at Thelwall, and also the strength of the wind, corresponding to each observed altitude of the borometer.

Mr. Mosley read an extract from the *Gibraltar Chronicle* of the 11th February, 1861, giving details of a violent gale experienced at that place, and on the coast of Morocco, on the 9th of February. The barometer fell to 29·261, and it oscillated violently during the gale, the mercury being jerked up and down more than ·100 inch. The range of the barometer between the 26th January and 10th February, was 1·240 inches, the range for the whole year 1860 having been only ·865. The range for twenty-four hours, commencing 9th February, at eight a.m., was ·625 inch. The storm appears to have been a cyclone moving on a southerly course. It was encountered off the coast of Morocco by the steamer *Egyptian*, veering from S.E. by S.S.W. to N.W. It was met with by the *Indus* mail steamer between Lisbon and Cape St. Vincent, on Saturday, the 9th February, and blew a hurricane from N. and N.E. It did not reach the coast of Morocco until eight o'clock on the following morning. As the occurrence of a cyclone on the Portuguese coast, moving in a southerly direction, is unprecedented, this gale will be further investigated.

MICROSCOPICAL SECTION.

February 18, 1861.

Letters were read from Captain Anderson, R.M.S. *Canada*, and from Dr. Wallich, respecting the pamphlet on "*Life in the Deep Seas.*"

Mr. Sidebotham described his experience in mounting Desmidiæ, and the difficulty he found in discovering a suitable medium for their preservation. He had tried syrup, Goadby's fluid, and a number of other chemical preparations, but the specimens, in course of time, were spoiled from one cause or other; the fluid which has best withstood the effects of time is simple distilled water; the cells being made of gold size and Japan black. Mr. Sidebotham exhibited desmidiæ, mounted in distilled water, in the years 1842 to 1846, in which the chlorophyll is comparatively little altered.

Professor Williamson observed that Dr. Carpenter had mounted star-fishes in glycerine, and had found the colours well preserved. He himself had used a mixture of glycerine and distilled water for volvox, and had found it to answer well.

Mr. Sidebotham also exhibited specimens of Diatomaceæ, mounted in 1844. The specimens (Isthmia enervis, Biddulphia, &c.) were obtained fresh, immersed in spirits of wine to absorb the water, and mounted in balsam; the green colour of the cell contents is yet perfectly preserved.

Professor Williamson exhibited some scales of fish, prepared by Dr. Kölliker, of Wurzburg, containing remarkable examples of fusiform lacunæ. He also pointed out how these and other similar discoveries, to which he referred, confirmed his previous conclusions in the Philosophical *Transactions*, viz., that fusiform lacunæ were not characteristic of reptilian bones, as some had supposed, but that they existed in many fishes; he especially referred to the salmonidæ as presenting this oblong form of bone corpuscle.

Mr. Brothers exhibited a modification of the kaloscope, and objects to illustrate the same.

Soundings were received from the steamers *Canada*, from New York; *Armenian*, coast of Africa; *Tagus*, from Lisbon, and from several vessels of war, from different parts of the world, which were duly acknowledged. Incrustations from the boilers of several sea-going steamers were also presented by Mr. W. A. Hayman, of Liverpool.

NOTICES OF PATENTS.

Obtaining Colouring Matter from the Lentisc or Mastic Tree. By Edward T. Hughes. Provisional Protection only.

The Patentee takes the leaves or berries, and boils them in water. He then filters and precipitates the fluid with a solution of a salt of iron. He thus obtains a deep blue liquid, which will communicate to fabrics a good black dye, provided they are left in the solution while in a state of effervescence until a reaction takes place. The word effervescence is evidently a ludicrously feeble attempt at an English rendering of a French word for boiling. To understand this, it is necessary to mention that the patent is a communication from "Christopher Muratore, Merchant, of Algiers, Africa, Empire of France." The solution above mentioned, may be used instead of Campeachy wood, nut galls, or other tanning materials. It also gives a black dye to all articles which have been immersed in a solution of iron. To facilitate the carriage of the dye, it may be reduced to powder. The powder may also be used for painting by being mixed with oil, &c. The colouring matter of the mastic tree will produce other colours than black, according as it is mixed with other salts or acids. Iron yields yellowish colours; lead, white; copper, brown; mercury yellow. The branches or stems yield the same colouring matter, but not so much as the leaves or berries. "So that the whole of this tree, which is very plentiful, almost inexhaustible, in Africa, and very cheap, may be made to supersede Campeachy wood, gall nuts, and other expensive colouring matters." Without going so far as the patentee in his ideas of the usefulness of the lentisc tree as a source of colouring matters, it is at least probable that the patent will do good by directing attention to an abundant source of a useful astringent.

Improvements in the Production of Colours for Dyeing and Printing. By William Stevens Squire. Provisional Protection only.

The patent is for the use of peroxide of mercury in conjunction with a neutral salt of aniline, or its homologues, for the production of colours. The patentee takes an intimate mixture of a salt of aniline and peroxide of mercury, and heats them together until effervescence takes place; he then continues the heat for some time until the action is complete. He then dissolves the mass in spirit, and filters. This solution, the patentee states, may be used for dyeing, when diluted with hot water; but, in order to purify it, he precipitates the colouring matter from the alcoholic solution by means of water. The precipitate is then to be dissolved in alcohol, and diluted with hot water for dyeing purposes. When the salt of aniline is not easily fusible he adds paraffine oil or wax to the mixture, and heats as before, until the action is complete. The paraffine oil or wax is poured off from the mass containing the colouring matter previous to dissolving the latter in spirits. In making the mixture of the salt of aniline and peroxide of mercury, the patentee prefers freshly-precipitated peroxide, and adds it to a warm saturated solution of the salt of aniline, and evaporates the mixture to dryness.

This patent was evidently taken out under the idea that oxide of mercury and a salt of aniline would react like aniline itself and a salt of mercury. If it was intended to make Magenta by this process, it is pretty evident that the scheme has proved a failure. No processes of this kind yield products in the quantity, and of the brilliancy and purity, now required for commercial purposes. Moreover, even if the process described afforded a good colour, the method of purification is obviously insufficient to give a dye of the requisite solubility in cold water, and freedom from tarry impurities.

CORRESPONDENCE.

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—I am at a loss to conceive to what portions of my letters, published in Nos. 63 and 64 of your Journal, Mr. Scott alludes when he speaks of "virulent animadversions," nor am I aware in what respect I have lost sight of that courtesy "due to every individual from others of his species." If for public as well as scientific ends a fair and open discussion of the Adulteration question is invited by Mr. Scott, I cannot but think that he might have favoured us with fuller information on the points to which I more particularly called attention, and that his remarks generally might have been a little more explicit.

I have yet to learn that to write under an assumed name is in any way discreditable, unless such a course is adopted as a screen for false statements to the detriment of others, or for the use of language which the writer would be ashamed to avow openly, and I cannot think that to discuss a question of general interest anonymously is at all undesirable, inasmuch as in such a case the value of the communications necessarily depends upon the force of the arguments used and is not influenced by the name of the individual by whom they are advanced. If I have been so unfortunate as to select a "singularly inappropriate" *soubriquet* it is to be regretted; although a matter of no great consequence, and one which cannot have the slightest bearing on the discussion. Passing over Mr. Scott's remarks as to my "peculiar diction, overstrained criticisms, and puerile objections," as equally irrelevant to the subject, let me come at once to the points at issue.

Mr. Scott states that my enquiry whether "colours and essences of every description are to be henceforth decried," is answered by my own remark that "we must all agree that the admixture of poisonous matter in any shape with food cannot be too strictly prohibited." This can be only understood as an indirect affirmative, implying that Mr. Scott regards all colours and essences employed to please the eye or taste as poisonous, and would recommend the prohibition of their use. I cannot allow that the employment in any article of food of such substances as cochineal, turmeric, saffron, almond, orange flower, lemon, vanilla, and other essences of a similar character, can be considered as injurious; but at the same time all people of common-sense will admit that the practice of introducing acknowledged poisons, such as chrome yellow, Scheele's green, vermilion, and acetate of copper, into food of any description ought not to go unpunished.

As regards the adulteration of bread, the most important of the subjects treated of, and respecting which we are anxiously looking on all sides for such further details as may tend to throw some light upon the contradictory and conflicting evidence now before the public, Mr. Scott still withholds any information and has "only to repeat his original statement without the slightest qualification," informing us that "any one of competent analytical skill" can ascertain its truth for himself, if he will take the trouble to do so. If this is the case, how are we to reconcile with it the very opposite results obtained by Dr. Odling? When we consider the immense amount of bread consumed in London and that the number of bakers is more than two thousand four hundred, I think it can scarcely be using too strong language to term the statement made by Mr. Scott that 87 per cent of all this vast quantity is more or less adulterated, a "sweeping assertion," for I very much question whether any one person has examined a sufficient number of samples to justify the publication of so positive a statement.

It would be interesting to know whether the "87 per cent." was obtained from the examination of samples from any particular district, whether they were of a special quality of bread or procured from the lower class of tradesmen exclusively?

Mr. Scott alleges that the necessity of condensing his paper for the Society of Arts prevented him from entering into details, but it would have scarcely increased its length perceptibly had he put us in possession of some two or three important data. A very few words would have sufficed to have told us in what proportion of the 87 per cent. the adulterant was alum, and what was the average amount of this substance found in a quartern loaf. Admitting, however, that the time would not allow of this, why does he still refuse the desired information, now that he has ample opportunity of affording it? Mr. Scott states that "the simple tests for, and characteristics of, the presence of adulterants" named in his paper "are to be regarded solely as popular tests." Whilst it is very desirable that simple tests capable of being made by every one and which may serve as indications of the presence of foreign substances in our food should be made known to the public, it is at the same time of the utmost importance that such tests should be thoroughly reliable, and have no tendency to mislead; otherwise they can only serve to create unnecessary alarm. Now, I maintain that the process of testing bread for alum as described by Mr. Scott, would, in the hands of ordinary and unscientific operators, indicate its presence in bread made from pure flour. The testing of bread for alum by searching for alumina is not a process that can be performed, even so far as to give reliable indications of its presence (unless the quantity used has been very large), by any one unaccustomed to operations of this description. In following the directions given, the ordinary potash of the shops would naturally be employed, which every chemist knows almost invariably contains alumina—the very substance the presence or absence of which it is desired to ascertain. Moreover, the method given is incomplete, even were pure potash employed and the operation performed by a skilful chemist.

I also greatly question whether "brittleness of structure" is any indication of rice, or "crumbly texture" of potatoes, inasmuch as bread made from pure wheat frequently possesses these properties, while on the other hand, bread containing rice or potatoes certainly does not always exhibit either of the indications alluded to.

The question of brewers' grains in oats, and the danger to horses arising from adulterated rape-cake, need not be further noticed, since it seems that the former is a deception that can only be practised on the blind, and the latter appears to have arisen from an unsuccessful attempt to feed a horse upon an unusual description of diet.

Mr. Scott appears to be somewhat indignant at my conceiving it possible he could have pursued a course so far beneath his "mission in the world" as to have made the authorities aware of his discovery of the common practice of poisoning meat with arsenic. Are we to suppose that Mr. Scott would quietly look on while a robbery was being committed, or that he would consider himself a "common informer" if he communicated the fact to the police? I think I shall express the opinion of most of your readers in holding it to be our duty when we discover the perpetration of any crime against society, to do all in our power to put a stop to it, and one would scarcely expect, in pursuing the most effectual and straightforward course towards this end, that we should be regarded as having acted in a manner derogatory to the character of a gentleman.

The fact that Mr. Scott has examined 1127 samples of pickles and several thousand samples of tea, shows that he must have paid great attention to his subject, and the evidence he could give would doubtless be both interesting and instructive, but his estimation that 91 per cent. of all pickles are adulterated, is of no practical value unless we know what he means by the terms "good and pure." If he considers pickles to be adulterated, to which any substance (although perfectly harmless) has been added to improve the colour or flavour, then we can scarcely be surprised at his finding so large a proportion of adulteraated samples, but I apprehend that the public only care to know what proportion contain poisonous or injurious ingredients.

However numerous may be the samples of tea he has examined, yet his results afford us no criterion of the extent of the mischief against which we wish to guard ourselves without some information as to the nature and amount of the adulterants employed. If other leaves are substituted for those of the tea-plant, what are they? Is their nature positively injurious, or is it simply that they do not possess the valuable qualities of the tea-plant?

Mr. Scott asks "What kind of authority is one of the largest bakers in Vienna upon the question of the purity of bread?" and speaks of the statements of tea-dealers as *ex parte*. Now, I think it will be admitted that the interest of respectable dealers, so far from lying in "exactly the opposite direction to that in which truth is to be found," consists in giving every information calculated to put a stop to those frauds practised by unprincipled men, who are thereby enabled to undersell them in the market. If the evidence of no person in trade is to be considered as reliable, how comes it that Mr. Scott quotes Messrs. Huntley and Palmer as to the purity of their own biscuits? Why does he consult Messrs. Phillips on the question of tea? and why does he speak of Messrs. Crosse and Blackwell as "no mean authority" on the question of pickles? It is difficult to understand what Mr. Scott considers as a "pure and wholesome" food; but judging from his proposal to prohibit altogether the sale (retail) of the whole of the "brown sugars of commerce, except the crystalline varieties," we have every reason to believe that his ideas on this subject are not of a very practical character.

In conclusion, it is perhaps scarcely necessary to disclaim any intention on my part of making the discussion of this question in your columns a matter of personal controversy, my sole object being to prevent the publication of what I conceive to be mistaken views from passing unnoticed, and thus allowing it to be assumed that they have met with the approval and concurrence of all your readers.—I am, &c.

Verax.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Wood's Fusible Metal.—Lipowitz has made some experiments on the cadmium-alloy, described by Dr. Wood (see Chemical News, vol. ii., p. 257). He found that an alloy composed of 8 parts lead, 15 parts bismuth, 4 parts tin, and 3 parts cadmium, possessed the following properties:—It is permanently silver white, and has a brilliant metallic lustre; it is not so brittle or hard but that it may be obtained in thin leaves or flexible plates; it has a fine-grained fracture, and may be filed without stopping up the file. In dry air it keeps its polish. It expands in cooling, but not so much as bismuth or antimony. Its specific gravity is from 9·4 to 9·41. It softens between 131° and 140° Fahr., and near 140° becomes perfectly fluid. No change in the condition of the metallic mass was observed on remelting after rapidly cooling the alloy. The above properties show that the alloy may be applied to some useful purposes. It may supersede all the quicksilver alloys for stopping teeth: it may be used as a solder whenever the metals soldered are not likely to be exposed to heat. Tin, lead, and Britannia-metal may be soldered together under water above 160° Fahr. Zinc, iron, copper and brass may also be soldered with the greatest ease under water, to which a little hydrochloric acid has been previously added. The alloy is so easily fusible that it may be melted on a piece of paper over a spirit lamp. In the preparation of the alloy the author recommends the use of the purest bismuth.—*Dingler's Polytech. Journ.*, Bd. clviii. s. 271 and 376.

II. ORGANIC CHEMISTRY.

The Spontaneous Decomposition of Alloxan.—W. Heintz publishes (*Poggendorf's Annalen*, Bd. cxi. s. 436 and cxii. s. 79) a long paper on the nature of the changes which take place in the spontaneous decomposition of alloxan. Gregory first observed that some crystallised alloxan, which had been two or three years in a bottle, had become changed into a fluid and some crystals which no longer possessed the properties of alloxan. Heintz observed a similar change in some alloxan which had been prepared twelve years, but this still produced some of the reactions of alloxan. It caused a deep blue colour with sulphate of iron, when a drop or two of dilute ammonia was added, showing that alloxan was still present with some acid. With a specimen, however, which had been made fifteen or twenty years, the author could produce none of the reactions of alloxan, but found that it possessed the following properties:—It gave a yellow colour with sulphate of iron, and on the addition of ammonia only a reddish brown precipitate. It did not reduce a silver solution. The warm solution gave no purple red with ammonia. Baryta water gave a white precipitate, and the liquor smelt of ammonia. Chloride of calcium gave a precipitate which did not dissolve in acetic acid—it was, in fact, oxalate of lime, showing the presence of oxalic acid, and the absence of alloxan and alloxantin. Further researches proved that the alloxan had become converted into oxalic and oxaluric acids and urea, the latter forming the largest proportion. Baumert had found alloxantin, carbonic acid, and parabanic acid in a specimen of decomposed alloxan which he examined, and Heintz, comparing his own results with these, explains the decomposition which takes place to be as follows:—First. The crystallised alloxan breaks up into carbonic acid, alloxantin and parabanic acid, as Baumert has described. The latter acid and part of the alloxantin dissolve in the water of crystallisation of the alloxan. In this solution the alloxantin takes slowly oxygen from the air, whereby alloxan is again formed, to be again decomposed as before, the process continuing until all the alloxan and alloxantin is converted into parabanic acid. This acid then slowly takes up water, and becomes changed into oxaluric acid, which easily passes into oxalic acid and urea.

Products of the Oxidation of Fats and Sebacic Acid.—Arppe denies (*Annal. der Chem. und Pharm.* Bd. cxv. s. 57) the existence of azelaic, adipic, lipic, and lepargylic acids, described by Laurent and Wirz as among the products of the oxidation of oleic acid. These bodies, the author says, are only mixtures. He has separated suberic and succinic acids, a new acid something like the oxypyrolic previously described by the author, and pimelic acid.

III. ANALYTICAL CHEMISTRY.

Estimation of Iodine and Bromine in Mixture.—Reiman shows (*Annal. der Chem. und Pharm.* Bd. cxv. s. 140.) that iodine and bromine may be estimated when existing together in solution by means of a solution of chlorine of known strength. If a solution containing both the iodide and bromide of potassium be shaken with a standard solution of chlorine, and a small quantity of chloroform, the chloroform becomes of a blue, or if the solution of the salts be very weak, a rose colour. On continuing to add chlorine water, the colouration disappears when six equivalents of chlorine have been added to one of the iodide. Then, if the solution contains bromide, on a further addition of chlorine a new reaction takes place, and the chloroform becomes yellow, then orange, afterwards again yellow, and finally a yellowish white, when two equivalents of chlorine have been added to one of bromine. The bottle must be placed on a sheet of white paper to see these changes clearly. On a further addition of chlorine, the chloroform becomes colourless on the formation of the pentachloride of bromine. Founded on these reactions, the author has constructed a formula for making an approximative quantitative estimation of the iodine and bromine, but the process is evidently of but little practical use. The author explains that a bromine reaction is finished when the chloroform has the colour of a weak solution of chromate of potash. If the liquid tested contains organic matter, it must be got rid of by calcination with caustic soda.

Molybdenate of Ammonia a Test for Sulphur.—Schlossberger states (*Zeitschrift für Chem. und Pharm.* 1860. s. 423.) that a dilute solution of molybdenate of ammonia supersaturated with hydrochloric acid, gives a beautiful blue colour with a solution of sulphuretted hydrogen or the sulphide of a metal. This test, he says, is more delicate than the nitro-prusside of sodium.

IV. TECHNICAL CHEMISTRY.

Mineral Green Free from Arsenic.—Struvo (*Archiv d. Pharm.* Bd. cliv. s. 42) gives an analysis of this colour which has been in use in Germany for the last three years as a substitute for Schweinfurt green. Its composition was as follows :—

Chromate of lead	13·65
Basic carbonate of copper	80·24
Oxide of iron	0·77
Carbonate of lime	2·65
Moisture	2·58
	99·89

This colour, although it has not all the beauty of the Schweinfurt green, is recommended as being free from arsenic; but it is not altogether harmless, and therefore must not be used in confectionery or the like.

MISCELLANEOUS.

Shipment of Gas from America to London.—We saw yesterday at the passenger station of the Pennsylvania Railroad, at Eleventh and Market-street, a number of wrought iron cylindrical vessels, which, we were informed, contained compressed gas. Upon further inquiry we learned that Mr. Williams, who had fitted up the cars on the Pennsylvania Central Railroad for burning gas, had also taken a contract from Hathaway and Leach, of this city, for fitting out a number of cars to be used on the street railways in London. These cylinders had been sent to the railroad workshops in Altoona, to be filled with gas by means of the company's apparatus there; so that before long we may expect to hear of American street railway cars in London, lighted with gas made at the foot of the Alleghany mountains.—*Philadelphia Gazette.*

Great Salt Lake.—Away out in the Western wilds, some three hundred miles beyond the Rocky Mountains, and amidst other and loftier mountains still, there exists one of the most remarkable natural curiosities in the world—the Great Salt Lake. All the streams and rivers which run into Salt Lake have their sources in the Great Basin, and what is remarkable, none of them find their way out of it. There are no outlets to the great lake. It receives the waters of several large rivers, swollen annually by their mountain tributaries, but in no very extensive degree are the waters of the lake increased during the seasons of the most copious flowing of these rivers. Great Salt Lake, according to the United States Government survey, which was completed in 1850, is two hundred and ninety-one miles in circumference on the shore line. The water of the lake is salter than any other upon the face of the earth. Persons engaged in boiling salt on the shores of the lake say that three buckets of the water dipped out of the lake and boiled in an open wooden trough, with a sheet iron bottom, will yield one bucket of salt; or, in other words, that it is one-third salt. The analysis of the water made under the United States survey in 1850 says that the water contains more than twenty per cent. of pure chloride of sodium, and about two per cent. of other salts, making one of the purest and most concentrated brines known to the world. The specific gravity of the water is very great; this in the same analysis is given at 1·170, water being 1000. The water is so heavy or buoyant that a person bathing in it can sit upright, with head, shoulders, and arms out of the water, like sitting on a rocking chair; and a person can lie on the surface with head, hands, and feet out of the water. In the lake the colour of the water is a very deep dark blue, much more so than the ocean, but when taken in the hand it is transparent.

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⁂ All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

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Erratum.—It was obviously a slip of the pen on the part of the author.

Querist.—We will enquire into the matter, meantime you will be quite safe in denying it. It is a disgraceful trade puff.

Wm. Fox.—We do not think it is yet published, but will enquire.

Shop-keeper.—Heat it with water containing a bichromate of potash and sulphuric acid sufficient to colour it an orange yellow,

W. Flight.—Received. Apply at a second-hand bookseller's.

Apollo.—Try lard and cod-liver oil properly scented.

X. Y. Z.—"The Giesson Outlines of Chemical Analysis," and "Fownes' Chemistry," are the best books you can have for your purpose. Study Dr. Frankland's Lectures, which we are now publishing, and spend as much time as possible in experimenting and taking notes in the laboratory.

G. F. R.—*J. A. D.*—We regret that we must again decline our correspondents' communications. There are too many demands upon our space to allow of its being filled with purely theoretical speculations, unsupported by experiments.

W. L. C.—1. Apply to Messrs. Horne and Thornthwaite, Newgate Street, or to Caselia, Hatton Garden. 2. Chromate of lime is a definite compound; it may be prepared by mixing monochromate of potash with chloride of calcium, when it falls down as a pale yellow precipitate.

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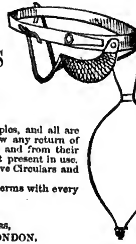

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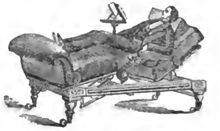
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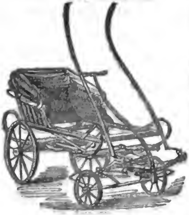
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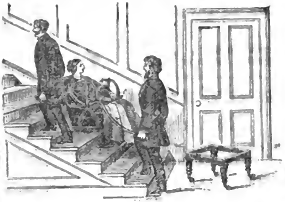
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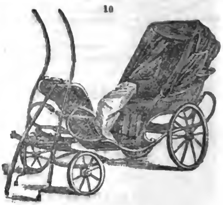
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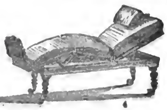
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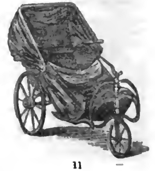
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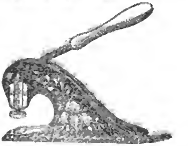

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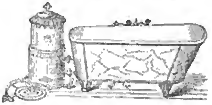

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THE CHEMICAL NEWS AND Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

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THE CHEMICAL NEWS.

Vol. III. No. 69.—March 30, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Existence of a New Element, probably of the Sulphur Group, by William Crookes.

In the year 1850 Professor Hofmann placed at my disposal upwards of ten pounds of the seleniferous deposit from the sulphuric acid manufactory at Tilkerode, in the Hartz Mountains, for the purpose of extracting from it the selenium, which was afterwards employed in an investigation upon the selenocyanides.[1] Some residues which were left in the purification of the crude selenium, and which, from their reactions, appeared to contain tellurium, were collected together and placed aside for examination at a more convenient opportunity. They remained unnoticed until the beginning of the present year, when, requiring some tellurium for experimental purposes, I attempted its extraction from these residues. Knowing that the spectra of the incandescent vapours of both selenium and tellurium were free from any strongly-marked lines which might lead to the identification of either of these elements, it was not until I had in vain tried numerous chemical methods for isolating the tellurinm which I supposed to be present, that the method of spectrum analysis was used. A portion of the residue introduced into a gas-flame gave abundant evidence of selenium; but as the alternate light and dark bands due to this element became fainter, and I was expecting the appearance of the somewhat similar but closer bands of tellurium, suddenly a *bright-green line* flashed into view and quickly disappeared. An isolated green line in this portion of the spectrum was new to me. I had become intimately acquainted with the appearance of most of the artificial spectra during many years' investigation, and had never before met with a similar line to this; and as, from the chemical processes through which this residue had passed, the elements which could possibly be present were limited to a few, it became of interest to discover which of them occasioned this green line.

After numerous experiments, I have been led to the conclusion, that it is caused by the presence of a new element belonging to the sulphur group; but, unfortunately, the quantity of material upon which I have been able to experiment has been so small, that I hesitate to assert this very positively. I am, however, at work upon some of the seleniferous deposit itself, and hope shortly to be able to speak more confidently upon this point, as well as to give some account of its properties.

In the purest state that I have as yet succeeded in obtaining this substance, it communicates as definite a reaction to the flame as soda. The smallest trace introduced into the burner of the spectrum apparatus giving rise to a brilliant green line, perfectly sharp and well defined, upon a black ground, and almost rivalling the Na line in brilliancy. It is not, however, very lasting: owing to its volatility, which is almost as great as that of selenium, a portion introduced at once into a flame merely shows the line as a brilliant flash, remaining only a fraction of a second; but if it be introduced into the flame gradually, the line continues present for a much longer time.

The properties of the substance both in solution and in the dry state, as nearly as I can make out from the small quantity at my disposal, are the following:—

1. It is completely volatile below a red heat both in the elementary state and in combination (except when united with a heavy fixed metal). 2. From its hydrochloric solution, it is readily precipitated by metallic zinc in the form of a heavy black powder, insoluble in the acid liquid. 3. Ammonia added very gradually until in slight excess to its acid solution gives no precipitate or colouration whatever, neither does the addition of carbonate or oxalate of ammonia to this alkaline solution. 4. Dry chlorine passed over it at a dull red heat unites with it, forming a readily volatile chloride soluble in water. 5. Sulphuretted hydrogen passed through its hydrochloric solution precipitates it incompletely, unless only a trace of free acid is present; but in an alkaline solution an immediate precipitation of a heavy black powder takes place. 6. Fused with nitre and carbonate of soda it becomes soluble in water, hydrochloric acid added in excess to this liquid producing a solution which answers to the above tests 2, 3, and 5.

An examination of these reactions shows that there are very few elements which could by the remotest possibility be mistaken for it.

The accompanying list includes every element, with the exception of the gases, bromine, iodine, and carbon. Opposite the name of each I have placed the number of the reaction which eliminates it from the list of possible substances, taking great care, in every case, to give the benefit of any doubt which might arise, on account of an imperfectly known or doubtful reaction, in favour of the opposite opinion which I desire to prove, and in cases where several reactions would prove the same thing only making use of the most trustworthy.

1, 5,	Aluminium.	1, 5,	Didymium.
	Antimony.	1, 5,	Erbium.
	Arsenic.	1,	Gold.
2, 3, 5,	Barium.	1,	Ilmenium.
2, 3, 5,	Beryllium.	1,	Iridium.
1,	Bismuth.	1,	Iron.
1, 2, 5,	Boron.	1, 5,	Lanthanium.
6,	Cadmium.	1,	Lead.
2, 5,	Cæsium.	2, 5,	Lithium.
2, 3, 5,	Calcium.	2, 5,	Magnesium.
1, 5,	Cerium.	1,	Manganese.
1,	Chromium.	3, 6,	Mercury.
1,	Cobalt.	1,	Molybdenum.
1,	Copper.	1,	Nickel.

---

[1] *Quarterly Journal of the Chemical Society*, iv. 12, and "Gmelin's Handbook" (Cavendish Society's translation), viii. 122.

1,	Niobium.	5,	Sulphur.
1,	Norium.	1,	Tantalum.
	Osmium.		Tellurium.
1,	Palladium.	1, 5,	Terbium.
5,	Phosphorus.	1, 5,	Thorium.
1,	Platinum.	1,	Tin.
2, 5,	Potassium.	1,	Titanium.
1,	Rhodium.	1,	Tungsten.
1,	Ruthenium.	1,	Uranium.
	Selenium.	1,	Vanadium.
1, 5,	Silicium.	1, 5,	Yttrium.
1,	Silver.	2,	Zinc.
2, 5,	Sodium.	1, 5,	Zirconium.
2, 3, 5,	Strontium.		

There are, therefore, left the following, amongst which, if already known, it must occur:—Antimony, arsenic, osmium, selenium, and tellurium; and although, to my own mind, many of the reactions detailed above are sufficient proof that it cannot be one of the first three elements, yet I have thought it better to let them pass.

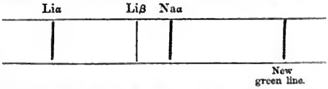
Each of the above five bodies, both in the elementary state and in combination, has been rigidly scrutinised, in the spectrum apparatus, by myself and many friends. Not a trace of such a line is shown by either of them in the green part of the spectrum. Antimony, arsenic, and osmium, in fact, giving continuous spectra, in which every colour is visible. The remaining elements, selenium and tellurium, might almost be dismissed unchallenged, inasmuch as I was first led to the examination by finding that it was *not* either of these. Nevertheless, I have, as stated at the commencement of this paper, repeatedly examined their spectra, and find no trace of such a line, the alternate light and dark bands in the almost continuous spectra of selenium and tellurium forming, in fact, so strong a contrast to the one single green ray of the new substance, that the latter may readily be detected in the presence of an enormous excess of either of the former.

In order to remove any remaining doubt which there might be as to the green line being due to any of the elements mentioned in the above list, I have moreover specially examined the spectra produced by each of these bodies in detail, either in their elementary state or in their most important compounds. Many of them give rise to spectra of great and characteristic beauty, but none give anything like the green line, nor, in fact, is there any artificial spectrum except that of sodium which equals it in simplicity.

There still may be urged the possibility of its being a compound of two or more known elements, or an allotropic condition of one of them; a moment's thought will, however, show that neither of these hypotheses are tenable. They would in reality prove what they are raised to oppose, for nothing less could follow than a veritable transmutation of one body into another, and a consequent annihilation of all the groundwork upon which modern science is based. If an element can be so changed as to have totally different chemical reactions, and to have the spectrum of its incandescent vapour (which is, *par excellence*, an elementary property) altered to an appearance totally unlike that given by its former self, it must have been changed into something which it originally was not.

The method of exhaustion, which I have adopted to prove the elementary character of the body which communicates this green line to the spectrum of the blue gas flame,[1] may seem unnecessary, as well as unchemical in the present state of the science. I was, however, obliged to rely upon what I may call circumstantial evidence of its not being a known element, owing to the very small quantity of substance at my command—I believe I over-estimate the amount which I have as yet obtained at two grains—which precluded me from trying many reactions. The method of spectrum analysis, adopted to prove the same fact, although perfectly conclusive to my own mind, might not have been so to others, unsupported by chemical evidence.

The following diagram will serve to show the position in the spectrum which the new green line occupies with respect to the two lithium and the sodium lines:—



For confirmatory experiments on many of the observations mentioned in this paper I am indebted to my friend Mr. C. Greville Williams. The detailed examination of the various spectra are at present being jointly pursued by us, and will be published as soon as completed.

On Certain Properties of the Platinum Metals, particularly Ruthenium, by Dr. C. Claus.

In my paper[2] on the platinum residues, I observed that if M. Deville had had the idea of presenting to the Paris Exposition large quantities of iridium instead of aluminium, and had he bestowed on the former all the trouble and expense he had given to the latter, it is probable that iridium would have ranked among the most useful metals in a way impossible to indicate beforehand. Scarcely a year has passed, and this conjecture has been confirmed in the most positive manner, thanks to the ingenuity of M. Deville and his colleague.

The new work of these remarkable chemists ought undoubtedly to mark an epoch in the history of platinum, and promises a brilliant future to this metal. Platinum, in fact, in consequence of these discoveries, will be applied to many more purposes that at present; for its high price ought to be greatly reduced, by the new method of fusing the ore. The old method for obtaining this metal in a solid and malleable state, was so complicated, expensive, and protracted, that it considerably raised the price of the metal relatively to that of the ore, and, consequently, in spite of its valuable qualities, the use of platinum was almost entirely restricted to chemical laboratories. The price of platinum ought to be still further diminished by a circumstance observed by the above-mentioned chemists,—that is to say, that the metals associated with platinum in the ore, with the exception of iron and osmium, cause no diminution by their mixture, to its valuable properties; they even enhance them, so much so, indeed, that platinum alloys with from 10 to 25 per cent of iridium, a metal not utilised till then, resist almost all reagents. Whence the residuums of platinum, so rich in iridium, as well as the useless osmide of iridium, are used for the preparation of platinum utensils. We have thus discovered an unexpected use to which iridium can be put; and it is likely, that ere long we shall learn other applications of it.

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[1] I need scarcely add that the line is quite distinct from either of the green or blue lines seen in a gas flame which is undergoing complete combustion. It is, moreover, far more brilliant than these.

[2] Liebig and Woehler's, *Annalen der Chemie und Pharmacie*, c. vii. p. 146.

But there are reasons why the price of platinum is maintained so high as greatly to limit its application; these reasons are,—first, the rarity of platinum ore, and then the expense of extracting it. As to the first objection, Russia (which yields most platinum) contains a quantity of this metal, the amount of which is not yet estimated. With the increasing demand for this metal, careful researches will multiply the number of known platiniferous localities, and will cause a reduction in its price. As to the expense of washing platiniferous sands, it can be notably diminished by establishing new and less imperfect machines than those at present in operation. The chemico-metallurgic question having been resolved in so brilliant a manner by the French chemists, the Russian technologists and the members of our mining corps should aid in perfecting the mechanical appliances.

For the application of platinum for divers purposes the purely chemical study of its properties and the accompanying metals is not less important than the metallurgic question. The great Swedish chemist, Berzelius, who, at the suggestion of the Russian Government, occupied himself with this question, treated it with great ability. He succeeded, in fact, in a comparatively short time, and with rare skill, in creating a completely new science in respect of these metals, in spite of the little material available for his researches. His very exact works form the only basis of all subsequent researches. Nevertheless, the modesty of this great chemist made him add, at the end of his long paper:—"My experiments have thus elucidated this question. However, they can be regarded only as the first outline of the history of the metals associated with platinum, and also of those which may yet be met with, —a history whose development is as extended as difficult." The French chemists, besides their fortunate solution of the metallurgic question, are also occupied with the exclusively chemical one, and have set forth, in this respect, many important data.

Their intention is, in the first place, to obtain general results by comparing these metals with others; they admit that the six known platiniferous metals, from a certain point of view, may be rightly considered as forming a separate and well-defined group. According to this view osmium, so remarkable for its properties, would be the metalloid of the platinum series, a consideration which, though very suggestive, appears illusive when the question is examined more closely. On the other hand, each metal of the platinum series is allied by certain analogies with some metal of another series. Thus platinum is allied to gold and iridium, palladium to silver, ruthenium to tin. We cannot deny the existence of these analogies, nevertheless I think I am justified in attaching no great importance to them, and I am very decidedly in favour of the first part of our author's account; in fact, as I have always expressed myself on this subject, I consider the platiniferous metals as forming an isolated metallic group, inseparable, and solidly constituted. Nature itself, however, gives indications which furnish us with the means of comparing them with the other metals. In fact, in natural platinum mines, as well as in all those which generally contain platinum, osmium, and iridium, we constantly find iron, manganese, copper, and often even gold. It cannot be imagined that these metals, so constantly associated together in platinum mines, should come there in an accidental manner, but form alloys according to certain relations of analogy. These analogical relations, of which I shall have occasion to speak by-and-by, do undoubtedly exist. It has been proved that osmide of iridium is not, as was at first believed, a chemical combination of equal parts of osmium and iridium, by equivalent, but rather a platinum ore, in which the metals generally found in soluble platinum ores exist in excess: thus, in the osmide of iridium, either osmium or iridium predominates, while it is platinum in the soluble ore. There is yet a third platinum ore, *irite*, which my friend M. Herrmann, of Moscow, has studied, and which, according to his analysis, is found to be a combination of oxidised platiniferous metals with the oxides of iron and chromium. Nevertheless, I think myself justified in doubting the existence of this ore, not only because such a combination does not appear likely to take place, but because that so complicated a mixture as the platinum residuum should, by so simple a process, eliminate a pure mineral. In short, by exactly following M. Herrmann's method, I have obtained a substance in spangled powder possessing the properties which M. Herrmann attributes to irite, and which, upon close examination, proves to be a very complex mixture, the greatest part of which consists of fine spangles of osmide of iridium and of chrome iron ore.

In my assays of 1854 I attempted the grouping of the platiniferous metals. Having since enlarged my experience on this subject, I again undertake this labour, with certain modifications.

(To be continued.)

On a Series of New Combinations of Ammonia, Picric Acid, and Metallic Bases,[1] *by M.* Carey Lea.

In a paper published in this Journal, vol. xxvi. p. 379, for 1858, I described two compounds of picric acid and ammonia with copper and nickel respectively, and mentioned their instability and the great difficulty of obtaining them in a state of tolerable purity. Since then I have found that many other metals form compounds of a similar nature, and have succeeded in obtaining several of them sufficiently pure for approximate analysis. The results obtained are given below. Other investigations are in progress, and I hope to communicate them hereafter.

When a metallic salt is precipitated by ammonia, and a large excess of the precipitant added, a more or less complete solution of the precipitate is frequently obtained, especially if a considerable quantity of ammoniacal salt be present. I have found that these ammoniacal solutions, when treated with an alkaline picrate, for the most part yield an immediate precipitate, containing the elements of a metallic picrate united with those of ammonia. These precipitates are often very beautiful; they are generally yellow or yellowish, somewhat soluble in the mother liquid, or in strong ammoniacal solutions, by aid of heat, nearly insoluble in water, and decomposed by it very readily, especially if it be present in quantity, or heat be applied. Thrown on a filter and washed, the picric acid, ammonia, and some portion of undecomposed salt in solution pass through, while the greater part of the metallic oxide remains on the filter, and with sufficient washing is perfectly freed from picric acid and ammonia, and presents the appearance of a pure hydrate.

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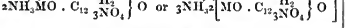
[1] *American Journal of Science and Art.*

The analyses of these substances were made in the following manner:—It was found desirable that before the ammonia and metallic oxide were determined, the whole of the picric acid should first be removed. This was effected by adding chlorhydric acid to a weighed quantity of the substance, carefully dried, and evaporating to dryness at a gentle heat. Ether, which must be anhydrous, or nearly so, was added in the proportion of 15 to 18 grammes to 6 or 7 grammes of the salt used, detaching the dry crust and breaking it up to powder with a glass rod, that every part might be brought thoroughly in contact with the ether. After standing ten minutes, the ether was poured off, a fresh quantity added, and a large beaker was inverted over that which contained the substance undergoing analysis. After a lapse of three or four hours, the liquid was poured off, a third, and, if necessary, a fourth quantity added in the same way. The decantation is effected with the greatest ease, as, if the ether is sufficiently strong, the chlorides remain nearly or quite dry at the bottom. It was found that by this means the picric acid could be separated with sufficient exactness to afford a tolerably correct estimation of its quantity, a careful trial with a weighed quantity of perfectly pure picric acid showed that the result fell a little under the truth by reason of a small loss arising from minute portions of the acid remaining undissolved. On the other hand, no chlorides are dissolved when the operation is properly performed; the etherial solution evaporated to dryness, and the residue dissolved in water, was not troubled by solution of nitrate of silver.

The operation requires nice manipulation, otherwise the loss of picric acid is considerable, and if the ether be not sufficiently anhydrous, portions of the chloride may be removed by it.

In this manner the quantity of picric acid was determined, and from it was calculated the quantity of the group $C_{12}H_2N_3O_{14}$, or picric acid minus the equivalent of hydrogen, which is replaceable by a metallic oxide; in other words, the group which, by union with a base, forms a picrate, or, by union with hydrogen, forms picric acid. From the residue undissolved by the ether, the ammonia was determined, either by distillation with solution of caustic soda, or by precipitation with bichloride of platinum. From another portion the metallic oxide was found by appropriate means.

As far as examined, the ammonia picrates all contain more than one equivalent of ammonia, and are represented either by the formula:—



$$2\overset{\frown}{NH_3MO} \, . \, C_{12} \left.\begin{matrix} H_2 \\ 3NO_4 \end{matrix}\right\} O \text{ or } 3\overset{\frown}{NH_{3}2}\left[MO \, . \, C_{12} \left.\begin{matrix} H_2 \\ 3NO_4 \end{matrix}\right\} O \right]$$

Ammonia-picrate of Silver.

Nitrate of silver, dissolved in water, and treated first with excess of ammonia, and the resulting clear solution mixed with cold saturated solution of picrate of ammonia, gives immediately an abundant, highly-crystalline, light yellow precipitate of ammonia-picrate of silver, which is to be thrown upon a filter, thoroughly washed with dilute ammonia, and dried *in vacuo* over sulphuric acid.

It is to be observed that in the formation of this, as well as of all the other salts here described, the metallic solution must invariably be in excess, to check as far as possible the precipitation of picrate of ammonia together with the compound sought. I have shown in a paper intended to be read at the same time as this,[1] that if an alkaline picrate be poured into an alkaline solution, a precipitate of alkaline picrate takes place. When the alkaline solution is strong the precipitate is immediate, and in the ammoniacal solutions used in the preparation of these salts any excess of alkaline picrate would fall with the ammonia-picrate. Even with every precaution, it is difficult, and in some cases apparently impossible, to obtain the ammonia-picrate perfectly free from this impurity.

(To be continued.)

On the Incineration of Filters.

The great difficulty with which, in many cases, the last portions of the carbon of a filter are consumed when ignited in a porcelain crucible, is a fact well known to analysts. It does not appear, however, that the following simple method of obviating the difficulty—as practised in the laboratory of Professor Scheerer, in Freiberg—has ever received the publicity which it deserves.

Whenever a filter upon which a substance capable of injuring platinum has been collected has to be incinerated, the porcelain crucible or capsule in which the process is to be conducted should be placed within a vessel of platinum of similar form, and the whole ignited in the usual way.

Whether the greatly accelerated rapidity of combustion of the carbon which ensues depends upon a more equable distribution of heat brought about by the greater conducting power of the metal—an explanation which is current for the somewhat analogous case of copper-coated glass flasks, or whether, as seems probable, the power of the porcelain vessel to absorb heat be really increased by the interposition of the platinum; whether both these causes be of influence, or the result depend upon another less apparent reason, or, finally, whether vessels of some other metal would not be preferable to those of platinum, are questions which we do not propose to discuss in this connection. We desire only to give currency to the fact to which attention has been called, feeling assured that the chemist who has once employed this method to destroy a refractory filter will ever afterwards bear it in mind.—*American Journal of Science and Art.*

TECHNICAL CHEMISTRY.

On the Relations of certain Yellow Vegetable Dyes,

by John Galletly.

Professor Bolley has recently published a paper[2] to show that Hlasiwetz was right in regarding the colouring matter of Persian berries as identical with that of quercitrin bark. Hlasiwetz assumes their identity mainly from their similar elementary composition, having only examined quercitrin; while Professor Bolley, at a former period, has made an examination of quercitrin, and has now undertaken an investigation of Persian berries, with the special object in view of removing any discrepancies in the way of these colouring matters being considered identical. I wonder, then, that he has furnished no explanation of the very considerable differences which exist between the published accounts of these bodies.

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[1] See *American Journal of Science and Art*, p. 75.

[2] Read before the Chemical Society, and published in the *Quarterly Journal of the Chemical Society*, January, 1861. See also an abstract in Chemical News, December 8, 1860.

Hlasiwetz has made many valuable observations on various glucosides, and, amongst others, the important fact that when several of them are decomposed, by boiling with a little dilute sulphuric acid, they yield, besides the glucose, a body which, on heating with potash, gives phloroglucine; and this phloroglucine has the same composition, whether obtained from phloridzin or any variety of quercitrin; and when it is known that phloridzin is obtained from several species, and quercitrin from plants very distantly related, the subject appears well worthy of attention.

Hlasiwetz regards as a variety of quercitrin a body which yields glucose and quercitrin, in whatever proportions these are obtained from it. Thus, one variety may contain only one equivalent of glucose, others two or three equivalents, combined with quercetin, the quercetin not being subject to any variation. It is, therefore, perhaps of little use to point out that xanthorhamnin contains, when dried at the same temperature, one-half per cent. more hydrogen, and one per cent. less carbon than the quercetin of which the analyses are given in Hlasiwetz's paper. It would appear also that quercitrin does not lose its water of crystallisation at temperatures corresponding with xanthorhamnin.

Professor Bolley has not remarked that Rigaud found quercitrin to be nearly insoluble in cold, and sparingly soluble in boiling water, while xanthorhamnin dissolves in cold water in large quantity, and in boiling water so abundantly that it is difficult to make a saturated solution.

The proportion of sugar which xanthorhamnin yields is very different from that of any variety of quercitrin, thus:—

	First kind. Hlasiwetz.	Second kind. Rigaud.	Third kind. Kawalier.
Glucose .	27·87	44·35	56·3 per cent.
Quercetin	—	61·40	— ,,

Xanthorhamnin yields:—

Glucose . . .	68 per cent.
Rhamnetin . .	41 ,,

Supposing quercetin and rhamnetin the same, it would require about four atoms of glucose to one of rhamnetin, and then the calculation for the elementary composition would not correspond with the analyses so closely as the closely-agreeing numbers obtained on analysis require, while the water of crystallisation would be fully twenty atoms,—an unusually large number for this class of substances.

Quercetin and rhamnetin agree well enough in elementary composition, but not in properties, the former dissolving readily in alcohol, giving small, lemon yellow crystals; while, on the other hand, rhamnetin is almost insoluble, and gives at most a little flocculent matter, which the microscope alone could resolve into crystals.

Altogether, the bodies are quite distinct, as far as they have yet been investigated. Elementary composition cannot be considered of much value in identifying bodies of this class, as they mostly have very closely-corresponding compositions.

Professor Bolley remarks that Hlasiwetz regards chrysorhamnin and rhamnetin as identical. I did not observe this in reading over Hlasiwetz's paper, but Professor Bolley seems to regard it as accurate. If this were the case the unripe berries would contain rhamnetin, which, as they ripened, combined with glucose, and became converted into xanthorhamnin, which can again yield the rhamnin by boiling with dilute acid. So important a conclusion has as yet no foundation whatever, and is very improbable. In fact, the chrysorhamnin got by Kane, which he converted, by boiling for a few minutes in water, into xanthorhamnin, and then analysed the xanthorhamnin, corresponds not in the least with the crystals obtained by Professor Bolley with crude ether. Kane could not possibly make the mistakes necessary for Bolley's substance to be identical with his. It is a pity crude ether was used by Professor Bolley, as this might be a mixture very difficult to obtain again exactly the same, and as the result differs from Kane's and my own, it is hard to say what the particular substance Professor Bolley obtained might be.

On the Action of Carbonate of Soda on Cast Iron.[1]

C. Tissier, director of the Aluminium Works, at Amfreville, has published in the *Technologiste* (p. 357), some interesting experiments on this subject, an abstract of which we now make from *Dingler's Polytechnisches Journal*, cxlvi. p. 118 (1857). Tissier's attention was first drawn to the fact that the mixture of carbonate of soda, chalk, and charcoal used in the manufacture of sodium,—notwithstanding the great excess of carbon it contains—does not affect the malleability of the iron of the retorts which are employed. He found, further, when a piece of cast iron was submitted to a high temperature, with this mixture, that it was first converted into steel, and finally into malleable iron. This led him to try the effect of the action of carbonate of soda alone on both malleable and cast iron—malleable iron thus treated remained unchanged, while the soda salt extracted the carbon and silicium from the cast iron, thereby converting it into infusible malleable iron.

If a specimen of pig-iron (that experimented on contained 6·6 per cent. of silicium and free carbon) be exposed for several hours at a red heat, in a crucible containing an excess of carbonate of soda, the following reactions may be observed. As soon as the temperature is sufficiently elevated, the mass commences to rise, and large bubbles of carbonic oxide are given off, burning with a yellow flame. If after the evolution of carbonic oxide has ceased the fire is allowed to go down, and the iron taken out with tongs and freed from adhering salts by means of a hammer and by water, the metal exhibits a completely etched surface, although the form of the mass is unaltered—it may be drawn out under the hammer and forged either hot or cold, and the granular fracture of the cast iron is replaced by a fibrous crystalline texture—the mass is porous, the little cavities being filled with white silicate of soda, formed from the silicium contained in the metal. The iron thus obtained is scarcely attacked at all by chlorhydric acid in the cold, and but slowly even when heated. Dilute nitric acid acts upon it with energy, but still not so rapidly as in the case of ordinary malleable or cast iron.

Tissier considers it conceivable that this action of carbonate of soda not only extracts the carbon and silicium, but also must remove the phosphorus and sulphur contained in the iron. It is further possible that the iron takes up a portion of sodium, which does not injure it, but on the contrary the metal acquires desirable properties, as evinced by the fact of the dealers being very willing to purchase the worn out retorts from the sodium works. If sodium be not thus taken up, we must assume that for every equivalent of carbonic oxide an equivalent amount of anhydrous soda is formed. Tissier suggests that in the process of annealing or the conversion of cast iron into malleable iron, now accomplished by packing and heating for a long period with substances rich in oxide of iron, it may be possible to substitute fusion with carbonate of soda, it possessing the advantage that the metal can be removed from time to time to watch the progress of the conversion into steel or malleable iron.

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[1] *American Journal of Science and Art.*

The author hoped by this method to convert large masses of cast-iron into malleable metal, such as heretofore could only be obtained by forging, but the length of time required for the conversion of a mass of any considerable thickness, and the porosity of the iron obtained, present practical difficulties requiring some modification to overcome them. With smaller castings, however, the action, even when superficial, imparts to them great toughness, so that they no longer are liable to fracture. The amount of carbonate of soda required, when properly used, is inconsiderable, but it should be pure; or, if the commercial article be used, it must first be heated with carbon in order to reduce the sulphate of soda to sulphide of sodium, as the alkaline sulphates have a powerful action on iron at a red-heat.

On a New Re-agent for Aniline, by M. CH. MÈNE.

THE only re-agent hitherto employed for the detection of aniline has been hypochlorite of lime, which instantaneously imparts to it a deep violet colour. On account of the dyes which are produced by aniline, I am led to believe that the following reaction which accurately characterises this body may be utilised. When nitrous gas is passed into aniline, either anhydrous or dissolved in cold alcohol, the aniline assumes a yellow-brown tinge. If, then, nitric, sulphuric, hydrochloric, or oxalic acid, &c., is added to it, a magnificent red colour, very soluble, is developed. A large quantity of water changes it into yellow; a drop of acid restores its colour. It dyes silk, cotton, &c., perfectly. In employing Hofmann's method for detecting benzine by the aniline and chloride of lime method, nitrous gas and an acid will also serve to find this body. As this red body crystallises very definitely, it will be thoroughly investigated in my laboratory.—*Comptes-Rendus*, vol. lii.

PHYSICAL SCIENCE.

On the Prismatic Decomposition of the Electric, Voltaic, and Electro-Magnetic Sparks, by CHARLES WHEATSTONE, *F.R.S., Professor of Experimental Philosophy in King's College, London.*

(*Read August 12, 1835, at the Meeting of the British Association, held at Dublin.*)

§ I.

FRAUENHOFER examined the ordinary electric spark with his prismatic telescope, and found that it presented a perfect spectrum crossed by numerous bright lines, as if certain definite rays were exalted in intensity, thus essentially differing from that of the solar light, which he ascertained was traversed by innumerable dark lines, indicating the absence of so many definite rays. The brilliant lines in the prismatic spectrum of the electric spark seem to have been but cursorily observed by this eminent optician; he gives no measurements of the lines, and confines himself to a simple statement of the fact. I have examined, in a similar way, the brilliant spark produced between two charcoal points terminating the poles of a voltaic battery, and find it to resemble the ordinary electric spark, exhibiting, as might have been expected from the longer continuance of the light, a spectrum of intense brilliancy, intersected by still brighter lines. These lines did not seem of a deeper colour than the parts of the spectrum immediately adjacent to them, but had rather the appearance of brilliant white lines superposed on the spectrum.

§ II.

The electro-magnetic spark obtained between the two ends of the wire in which the current is induced from the magnet, is, in all cases which have yet been observed, very minute; but it is well known that the brilliancy of the spark is greatly increased by causing it to occur between one of the poles and a cup of mercury connected with the opposite end of the wire. This arrangement is adopted in all the electro-magnets now made, and affords, on the rapid repetition of the spark, a considerable degree of illumination. The intensity of the light thus produced has occasioned it to be generally supposed that it is in part owing to the combustion of the mercury, a supposition which I shall hereafter have occasion to consider.

On analysing by the prism the electro-magnetic spark taken from the surface of mercury, I observed with the telescope the following singular and beautiful phenomenon. Instead of the appearance just described as characterising the ordinary electric spark, and the voltaic spark obtained between two charcoal points, I saw a few definite lines of light, separated by very wide dark intervals from each other, some of great brightness. The first was a double line of orange, and the smallest interval of any was between this line and the next, which was green; the interval between this and the succeeding one, which was greenish-blue, and when attentively examined was seen to consist of two lines close to each other, was about twice as great; after an interval of four this was succeeded by a purple line; and a violet line followed at the distance of three. The order of brightness of the lines was as follows:—green, very bright; purple; orange; violet; the double line of bluish-green; the last is so faint that in small electro-magnets it is not seen. When the spark is viewed near with a common prism, the orange line appears tinged on its lower part with red; but this red is by no means so apparent when the telescope is used; this arises from the red not being a definite ray, and its parts, consequently, being differently refrangible are diffused over a considerable space when the telescope is employed.

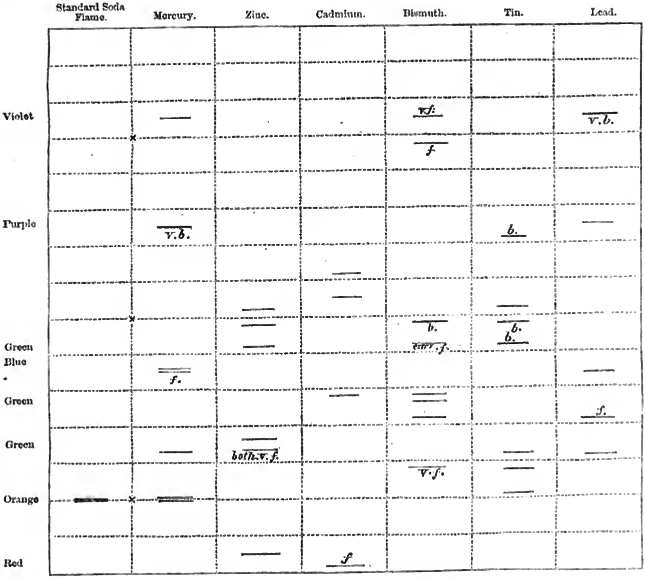
The electro-magnet employed for this and the succeeding experiments, was the powerful one constructed by Mr. Saxton for the Gallery of Practical Science in Adelaide Street. To render it convenient for these experiments, that is, to ensure the appearance of the spark always at the same point, the following temporary alteration was made:—The place of the revolving star was supplied by a small oval disc, which while it revolved acted on one arm of a lever, beneath which a spring was placed like that to a flute key; this lever was fixed on a plate of sheet-iron, having at its extremity a small cup to hold the mercury, and the other end of the lever was bent at right angles, so that as the oval disc revolved the bent end of the lever was alternately dipped into the metal and removed, and the spark always occurred precisely at the same point. Another advantage was obtained by this arrangement,—

the mercury was not thrown out of the cup as it is in the ordinary disposition of the apparatus.

§ III.

I next proceeded to observe the prismatic analysis of the electro magnetic spark taken from different metals while in a fluid state. For this purpose I employed the following metals in the purest state I could obtain them :—Zinc, cadmiam, bismuth, tin, and lead. I placed the metal intended to be the subject of experiment in the cup formed in the iron plate, and melted it by the application of a spirit-lamp placed beneath; the spark was then taken as above described. Not having at my disposal an instrument like that which Frauenhofer employed in his experiments, by which the degrees of refrangibility might be absolutely measured, I was obliged to content myself with an ordinary telescope-prism, furnished with a micrometer eye-piece, which affords only comparative results. The eye-piece was graduated with parallel lines, in one direction only, the fortieth of an inch apart. The spark was taken precisely at the same point, and the telescope remained in the same position during the whole of the experiments with the different metals; the spark was also obtained under exactly similar circumstances from carefully-distilled mercury. None of these metals gave an uninterrupted spectrum, but each presented a few bright, definite lines, widely separated from each other; the number, position, and colour of these lines differ in each of the metals employed. These differences are so obvious that any one metal may instantly be distinguished from the others by the appearance of its spark; and we have here a mode of discriminating metallic bodies more ready even than a chemical examination, and which may be hereafter employed for useful purposes. A Table accompanies this paper, in which I have laid down the positions of the bright lines observed in the six metals above mentioned. It will be seen that red lines characterise only zinc and cadmium, and these metals may be known from each other by the red ray in the cadmium being most refrangible. Mercury and lead exhibit bright violet lines, while zinc, cadmium, and tin have

Table of the Bright Lines in the Spectrum of the Magneto-Electric Spark, taken from different melted Metals, and observed with the Prismatic Telescope.



Abbreviations :—*b*, bright; *v b*, very bright; *f*, faint; *v f*, very faint.

The above relative measurements were taken by means of a ruled glass micrometer, placed in the focus of the eye-piece of the telescope.

none. The homogeneous light of a spirit-lamp occupied almost exactly the place of the orange lines in the mercury spectrum. The brilliancy of the spark in different metals appears to be in proportion to their volatility; cadmium and zinc are next in brightness to mercury.

On trying experiments with various amalgams and alloys, it was found that when the metals differed much in volatility the lines appertaining to the most volatile metal only was observed; in other cases lines belonging to both were seen.

§ IV.

The voltaic spark, taken between one of the poles of the pile and mercury, or between two globules of mercury, or between amalgamated points, presented exactly the same appearances as the electro-magnetic spark from mercury. Experiments were made with batteries of different kinds with the same results; the only difference observed was that the lines were brighter as the intensity of the spark was increased.

No difference was perceived in the prismatic analysis of the spark when the positive or negative pole was brought in contact with the mercury. Neither did the nature of the conductor produce any change in the colour or situation of the lines. Copper, iron, and platina were successively employed.

§ V.

Having ascertained this proof of the identity, by prismatic analysis, of the voltaic and electro-magnetic sparks, and having it in our power to obtain a strong spark from metals in their solid state by making use of an energetic pile, I now commenced a series of experiments with the intention of extending my Table to all the metals I could procure. For this purpose I put in action the large battery of King's College, consisting of 200 pairs of plates, on the Wollaston construction, with double coppers. These experiments were in progress when I left London, and I intend to resume them, with improved instrumental aids, on my return; I shall, therefore, not dilate upon them at present. One result is, however, too curious to be passed over without notice. Bringing the end of one of the wires of the battery in contact with different parts of the opposite wire, I observed that the spark seen by the naked eye varied in colour at different times, appearing now violet, now green, now red. I have not yet been able to ascertain whether this change of colour in the spark arises from variations in the intensity of the battery, or from the condition of the surface of the wire from oxidation, or any other cause. It might naturally have been expected that the prismatic decomposition of the spark would have varied under these different circumstances, but this, to my surprise, I found not to be the case. The number and position of the lines remained invariably the same; but as my assistant called out the colour the spark appeared to him, I saw in the telescope a line of the same colour, which was already visible in the spectrum flash on the eye with intense brilliancy, while the others remained unaltered. It is, therefore, evident that no mistake in identifying the metals can arise from this cause.

§ VI.

It has been supposed by Dr. Faraday and others that the bright spark from mercury is, in part at least, owing to a true combustion of the metal occasioned by the intense heat developed by the current. To determine whether this is or is not the case, I instituted two series of experiments: the first to ascertain whether the spark is produced and presents the same prismatic appearances in media where combustion cannot be expected to take place; and the second, to be informed whether the same prismatic decomposition occurs in the flame produced by the ordinary combustion of the metals as in the electric sparks which are drawn from them. With regard to the first point, Sir H. Davy had already proved that the voltaic light obtained by two charcoal points was evolved with equal splendour when the experiment was made in gases in which no combustion could be maintained; and it is also known that the spark occurs, though with diminished intensity, under water, oil, alcohol, ether, and other liquids. To determine whether the light from mercury was equally independent of this cause, I obtained the spark successively in the ordinary vacuum of the air-pump, in the Torricellian vacuum, in carbonic acid gas, and in oxygen, with scarcely any difference of effect, and the lines observed by the prism were the same in all cases,—a proof that the medium had no influence on the nature of the light. It will be unnecessary in this abridged account to enter into any detail of these experiments. The spark from mercury was also taken under water, alcohol, and oil. In these cases, by continuing the production of the spark for a long time, a quantity of black powder was formed, which, on examination, proved to be pure mercury, in a very finely-divided state. The same powder was observed on the receivers when the spark was obtained *in vacuo* or the gases, but it could not well be collected for examination.

These experiments leave no ground for supposing that the electric light is in *any* case a consequence of combustion. I may add to these proofs the observation of my friend, Professor Daniell, that what is called the combustion of gold-leaf and platinum foil is effected as well in hydrogen gas as in the air.

With respect to the second series of experiments above alluded to, I have examined with the prism the light of different metals while undergoing ordinary combustion. Iron, copper, bismuth, lead and tin were successively burned on charcoal by directing a stream of oxygen upon them. Examined by the prism they all presented bright uninterrupted spectra, in which no redundant or defective lines were visible, the same thing was observed when zinc foil was burned in the flame of a spirit lamp. There is, therefore, a marked difference in the physical properties of light obtained from the same metal by combustion and the action of electricity.

Wires ignited by the action of the voltaic pile, when observed by the prism presented spectra perfectly uninterrupted, and destitute of lines.

§ VII.

The prismatic analysis of the spark developed from different metals by ordinary electricity, brings to view many obvious differences from that excited by voltaic electricity. I have not yet made much progress in this part of the inquiry, and the only results at which I have arrived are the following:—1. Each metal presents lines differing in position and number; the metals I have tried are mercury, lead, tin, zinc, bismuth, copper, and iron. 2. The lines are less bright and more numerous than those which occur in the electro-magnetic or voltaic spark from the same metal. 3. It appears to me that all the lines of the voltaic spark are present in the electric spark, with the addition of many others. In the spark from a small jar taken between two amalgamated balls of tin, I observed lines corresponding in situation, colour, and relative brightness to those seen in the voltaic spark from mercury; the additional lines were—1, a red line; 2, two fine orange lines between the orange and green; 3, a faint purple line between the bluish green and purple; and 4, a violet line between the purple and violet, but nearer the purple line. Similar observations were made on sparks taken from tin, lead, &c. As I did not determine the exact positions of these lines, I cannot be certain of their perfect identity with those of the voltaic spark. The apparatus I am now constructing will enable me to ascertain this point, and to obtain accurate measurements of the lines. 4. When the spark was taken between two balls of dissimilar metals, the lines which belonged to each metal appeared together.

I intend to construct a table exhibiting the lines in the electric spark obtained from all the simple metals, and shall probably extend the experiments to include many of the imperfect conductors.

§ VIII.

I shall terminate this notice with a few observations on the bearing which the preceding experiments have on the theories which have been advanced to account for the origin of electric light. Having already proved that combustion cannot be its cause, I shall not resume the consideration of this subject here.

The electric light was for a long time considered by philosophèrs as a modification of electricity itself, which possessed the property of becoming luminous when accumulated to a certain degree. The experiments are decidedly against this hypothesis; were it true the character of the spark might be found to differ according to the source whence the electricity had emanated, which we, however, find not to be the case; but it would be improbable that the accumulated electricity having passed through solid conductors without any change should experience such great modifications simply from the limiting surface from which it passes into the air—the nature of this surface determining the character of the light.

Another theory was advanced by Biot, and was for some time very prevalent. This eminent philosopher having been engaged in some experiments on light produced by mechanical pressure, thought that the electric light might have a similar cause, and be merely the effect of the compression produced on the air by the explosion of the electricity. To account for the production of the spark *in vacuo*, he supposes that the space is filled with disseminated molecules, which constitute a medium offering a very sensible resistance and inertia to a velocity so great as that of electricity. According to this theory, if any change should be observable in the character of the spark, it should depend on the medium which is supposed to produce the light by its compression, and not on the conducting substances from which the electricity escapes; whereas we find the direct contrary to be the case. The experiments of Thenard are also fatal to Biot's hypothesis; this celebrated chemist has proved that light is never disengaged by the compression of gases, and that whenever light is apparently thus produced, it is owing to the presence of foreign bodies.

A different theory was formed by Ritter, which since developed by Davy, Oersted, Berzelius, and others, has very generally prevailed among chemical writers. In this theory the atoms of ponderable matter are considered as each possessing a peculiar electric state, some being positive, others negative; the action of the electricity of these particles is dissimulated externally by an atmosphere of the contrary electricity which completely envelopes it. Electricity is supposed to be communicated along a row either of positive or negative particles, by a series of decompositions and recompositions of neutral electricity, and the light and heat which accompany this phenomenon are the result of these actions. According to this hypothesis, sparks are produced between the atoms, in the same manner as in a string of metal beads, and the electric light is the result of the sparks between all the atoms. This theory affords no explanation of the modifications in electric light observed by prismatic analysis.

Seeing the insufficiency of all these theories to account for the observed phenomena of electric light, I am strongly induced to believe that it results solely from the volatilization and ignition of the ponderable matter of the conductor itself. The difference between the appearance of the prismatic spectra of the same metal electrically ignited and ignited by ordinary combustion, I conceive to consist in this,—in the first case the particles are by volatilization attenuated to the highest possible degree; while in the second, that of ordinary combustion, the light is occasioned by incandescent particles of sensible magnitude.

Fusinieri has proved by a series of beautiful and decisive experiments, too well known to be more than alluded to here, that in all cases when the electric spark traverses the air, it transports with it the ponderable matter of the conductor it has left. The views to which these experiments led him respecting the nature of electric light, are nearly coincident with those above stated; he assumes that the light arises from the ignition and combustion of the fused particles of the metallic conductor carried away in its passage; but this mode of considering the subject allows of no distinction between electric ignition and that of ordinary combustion; a distinction which my experiments so fully prove to exist.

The peculiar luminous effects produced by electrical action on different metals, depend, no doubt, on their molecular structure; and we have hence a new optical means of examining the internal mechanism of matter, in addition to those which Sir D. Brewster and other philosophers have already placed at our disposal.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

Lecture V.

Crystallization—Influence of Cohesion on Affinity—Precipitation—Normal Constituents of Water as it Occurs in Nature—Sources of Impurity in Water for Domestic Purposes.

(*Continued from page* 187.)

In addition to the solution of solid matter, water can also dissolve gases. We have here a table showing the solubility of different gases in water. Those which dissolve in a less proportion than 250 parts in 100 volumes of water are generally regarded as being merely dissolved in the water without any chemical effect taking place between the liquid and the gas; while those which are dissolved in larger quantities, such as ammonia, hydrochloric acid, and sulphurous acid are aided in their solution by a simultaneous action of chemical affinity. Now here I have a vessel by which I will try to show you one of the extreme cases. This vessel contains ammonia. One hundred volumes of water can dissolve 78,000 volumes of that gas. [The vessel used for the experiment was a long glass cylinder. The mouth was held downwards, and was plunged into a vessel of water.] I will now open the mouth of the cylinder, and there you see the water rushes up as into a vacuum; and with the exception of a little air with which the ammonia was contaminated the jar is now full of water. Water, from the moment of its condensation from steam, contains gas. Here is an apparatus by which the volume of a gas dissolved by a certain quantity of water can be ascertained. The gas is driven off by heat, and collected over mercury. I will not stop to explain the arrangement, but if any feel particularly interested in the apparatus, I shall be happy to explain it to them after the lecture. In this apparatus, when the air is exhausted, and the pressure upon the surface of the water is reduced, you see those gases dissolved in the water bubble out, and the water seems to be in a state of ebullition.

Now, from what I have said regarding the solvent powers of water, both upon gaseous and solid matter, you will easily conclude that pure water is almost an impossibility; we never meet with it in nature. If we trace water from the moment when it assumes the liquid condition, to the time when it finally makes its way to the ocean, we find it gradually accumulating impurities of various kinds. At the moment of condensation it exerts its solvent power and absorbs gases from the air. When it falls to the earth it percolates through strata more or less soluble and more or less pervious to water, and dissolves, according to circumstances, various quantities of the solid matters which it there meets with in those strata. If the surface of the earth where it falls be very hard and insoluble the water becomes only very slightly contaminated with solid matter. Loch Katrine, for instance, contains only 2½ grains of this solid matter in a gallon of the water; the Dee at Aberdeen contains 4 grains; and the Tay at Perth contains 5 grains. Frequently, also, when the strata through which the water percolates are pervious, supposing they are at the same time comparatively insoluble, very little solid matter gets into solution. Such is the case with the green sand formation where the water is very free from mineral matters of a solid kind. Generally, however, water meets with more constituents than those I have just indicated in these exceptional cases; and the quantity varies from 5 to 50 grains per gallon. When this quantity is exceeded the water generally acquires a taste, and may be regarded as *abnormal* water. The water of the ocean is in this condition. The rivers which flow into the sea carry with them matters dissolved, and leave them there, for the solid matters are not carried back by evaporation. Therefore, the sea contains a larger proportion of these substances than is contained in the water of rivers; and we get this effect on a still more exaggerated scale when in hot climates rivers empty themselves into lakes which have no outlet. This is the case with the Dead Sea. The river Jordan which is constantly flowing into it contains 75 grains of solid matter in the gallon; and none of this matter is returned again into the atmosphere, or no practical amount; therefore there is a constant accumulation of the saline matters going on there, and in this Dead Sea we have no less than 2600 grains of solid constituents to the gallon of water. The same effect takes place in a very recently-discovered lake in the north of Australia, a lake discovered a few months ago. It is highly charged with saline matters, and is known to possess no outlet. There is a similar instance in the celebrated Elton Lake in Russia which is 11 miles long, 8 miles broad, and on an average only 15 inches deep. In summer it appears to be covered with snow in consequence of the evaporation of the water forming a crust of saline matter. No less than 200,000 tons of salt are yearly extracted from this lake.

In addition to these mineral and saline constituents, water also dissolves certain organic substances, so that we may classify the impurities contained in water as *mechanical*, or those that are merely suspended in the water mechanically, and not dissolved, *saline* impurities and *organic* impurities. We will consider these separately.

First, as regards *mechanical* impurities. They consist, in the first place, of mineral substances which are usually innocuous, and have no effect upon the drinker. Then we have vegetable or animal matters, which are generally of an exceedingly noxious character; and then we have living organisms, which generally attract the attention of the water-drinker more than the other substances contained in the water. These organisms consist of animalculæ and animals of a larger size. We have in the New River water twenty-six species of those animals, in the Thames water twenty-nine species, and twenty-four species have been detected in the West Middlesex water, and so on, varying in number, in different waters. Perhaps you might be interested to make a somewhat nearer acquaintance with these beings which often come into close contact with us; and therefore I have obtained from my friend Mr. Warrington some two or three species, which I will endeavour to throw upon the screen. I have here a microscope by which I can produce a very greatly magnified image of them, I hope large enough to be seen by all of you. [The room was then darkened.] There are some of them on the screen. When they get a little more accustomed to the light I dare say they will be a little more steady in their movements, so as to give us a better opportunity of examining them. This is, I think, the *water-flea*. There is one of them, and there [pointing to a smaller] is a younger one. (Laughter.) You see they are very lively in their habits; and this is a kind of creature which is met with, I may say, in all good waters—(a laugh)—for instance, the water supplied to Manchester, which is well known to be one of the best waters in this country, is quite full of this kind of animals in summer. Here [changing the species in the microscope] I think we have got the *cyclops*, which is also a very commom organism in these waters. This is an interesting organism, if we can get one tolerably still, because the beating of the heart is very perceptible. We must get one upon its side; that is the best position. The animals are not at all exposed to the heat of the lamp; it is simply the light that frightens them; the heating rays are completely cut off by a thick stratum of water. Therefore they are not subject to any pain, but there is more light than they are used to. I must pass on to another specimen. Both of the specimens which I have shown are to be met with in good waters. I am now going to show you one which occurs only in waters of a very low character indeed. It is the little blood-worm. This is one which I got out of the Thames the other day, and you may get thousands of them out of the mud of the river almost any where, but especially near Westminster-bridge. This is obtained from a locality about a mile distant from where, a few years ago, a great amount of water supplied to London was pumped; therefore, I think we may say that this is one of the organisms which may possibly occur in water used for domestic purposes. See how beautifully transparent the body of this little creature is. A streak of crimson runs through the body in a spiral form. That [pointing], is the head of the animal. However, this is a monster which I trust we do not often meet with in water, but still it is an inhabitant of the Thames water which is yet supplied to some parts of London. It has been stated that some of these little animals are very deleterious to health, but I do not think that this has been at all clearly made out. Looked at from a chemical point of view, they are exceedingly useful in the water, especially if it be at all impure. Each little animal is a small furnace, which occupies itself in consuming the organic matter and converting it into its ultimate compounds,—carbonic acid, water, and ammonia, or nitric acid. They feed upon the organic matter in the water, and remove it far more readily than by the oxidising property of the air. From a physiological point of view, I do not know what sort of a case can be made out against them, but I do not think they have been fairly condemned. However, it is desirable to remove them before the water is used as a beverage, and this can be done by filtration. The animals themselves cannot pass through the filter, but the ova can; and therefore this has an important bearing upon the storing up of water, to which I shall have occasion again to allude. These are, then, the mechanical impurities.

We come now to the *saline* impurities. The nature of these will be readily seen by an inspection of this large diagram.

Solid Constituents Contained in One Gallon of Various Waters Supplied to Towns.

	New River Company.	East London Company.	Kent Water Company.	Thames Ditton and Gnd. Junction Co.	Farnham.	Watford.	Loch Katrine.	Aberdeen.	Liverpool.
Carbonate of lime	7·82	10·16	7·02	11·79	·23	16·13	—	·85	·76
Sulphate of lime	3·23	2·33	11·03	3·06	1·31	—	·38	·12	1·
Nitrate of lime	·02	·72	·07	·27	Trace	·23	—	—	—
Carbonate of magnesia	1·09	1·51	3·42	1·27	·64	·75	·15	—	·11
Sulphate of magnesia	—	—	—	—	—	—	—	·32	—
Chloride of sodium	1·73	1·76	3·50	1·10	·93	1·01	·45	·67	1·56
Sulphate of soda	1·49	·94	—	—	·07	1·31	·28	—	—
Chloride of potassium	—	—	—	·67	—	—	—	—	—
Sulphate of potash	1·11	1·25	·70	·17	·43	·44	Trace	—	—
Iron alumina and phosphates	Traces	·47	Traces	·09	·88	—	Traces	·08	·24
Silica	·50	·62	·76	·62	·99	1·59	Trace	·14	·15
Organic matter	2·79	4·12	2·61	2·29	1·78	1·26	·82	1·82	1·40
	19·78	23·88	29·55	21·33	7·26	22·72	2·08	4·00	5·42

We have here the proportion of solid matter in the New River Company's water; the East London Company's; the Thames Ditton; the Kent Water Company's; the Farnham water, which it was proposed some time ago to bring to London, and which is the green sand water; the Watford; and the Loch Katrine water, which is the purest perhaps ever supplied to towns. Of these constituents you will see that some of them are what are termed *earthy salts*, and some of them are *alkaline salts*. We have carbonate of lime, sulphate of lime, and nitrate of lime, carbonate of magnesia, and occasionally chloride of calcium and chloride of magnesium. These are salts of the alkaline earths, and the others are alkaline salts.

The property that we all know as "hardness" in water is due to the first of these classes of constituents—to the alkaline earths—the lime and magnesia salts present in the water. Hard water may be defined as a water capable of decomposing soap, and forming with the soap an insoluble precipitate. These salts of lime combine with the stearic acid, and form an insoluble stearate of lime, the curdy matter which is produced by washing in such waters. The difference between washing in hard and soft water I think I shall be able to demonstrate to you by means of the electric microscope. This is the water supplied to the Institution—I don't know by what Company. I will wash my hands in this water, and we shall see what sort of result will be produced. I shall get, I have no doubt, some of this stearate of lime precipitated, and we will collect a little of it upon the glass plate and put it in the microscope. In washing you are rubbing the skin with this precipitate—this stearate of lime; and it is important to know what sort of substance it is. It is impossible with water of this kind, in which this curdy, fatty matter is produced, to clean the hands, you may get them nearly colourless; but still the pores of the skin are filled up with this greasy substance, which I will endeavour to show you under the microscope. [A portion of the precipitate formed in the hard water was collected on a glass plate, and a magnified image was thrown upon the screen. The theatre was again darkened.] That is the kind of greasy matter—the stearate of lime—which is there separated, and which we rub into the pores of our skin when washing in water of that kind. I will now show you some of the lather which is formed in washing the hands with distilled water. Now I think we shall have here a collection of bubbles, but they will be transparant to light and will present a very different appearance from that solid greasy matter which we had formed in the other case. [An image of the lather formed by washing in the distilled water was then projected upon the screen.] You see here what a very different result we have. There is the scum formed in the distilled water. We have none of that thick greasy matter which penetrates into the pores of the skin and renders cleansing with hard waters utterly impossible. When you rub soap upon your hands in the process of washing you form a solution of soap, then you immerse your hands into the water and in washing off that solution you precipitate the stearate of lime within the pores of the skin. This is, in fact, a counterpart of the processes of the dyer and calico-printer. He first of all impregnates the fabric with one solution, and then precipitates a colouring matter from it by means of another solution which is applied afterwards; thus he fixes the colour within the fibre, and thus we fix the greasy stearate of lime within the pores of the skin, and render it incapable of removal by any amount of subseqent washing.

NOTICES OF BOOKS.

How to Work with the Microscope. By Lionel S. Beale. Churchill, New Burlington Street.

There are two instruments which man has invented both of which are calculated to excite in his mind the most ennobling and the most exalted ideas; one of these is the telescope, the other the microscope. By means of the former he is enabled to gaze into space and obtain views of the most stupendous character, by means of the latter he is enabled to examine the infinitely small. If the magnitude, grandeur, and mystery which surrounds the objects revealed by the telescope impress the imagination more powerfully, the studious examination of the wondrously beautiful, and perfect living organisations revealed to us by the microscope brings us, we say it with all reverence, in closer communion with the mighty Creator, whose power is manifested more impressively in forming and endowing with life myriads of creatures too small to be visible to man's unassisted eye, than in the formation of globes of inanimate matter of the uses of which we can form no conception.

To derive the greatest amount of pleasure and instruction from the microscope, it is necessary that we should know how to use it in the best manner. Amusement, but very little information, would be gained by using it to inspect objects in a desultory manner during occasional half hours; a system must be adopted, and, first of all, the student must acquire a thorough knowledge of the best method of operating with the instrument. This preliminary study is rendered exceedingly easy by the lucid manner in which Dr. Beale has treated the subject in the work before us. He has not fallen into the too common error of dwelling almost exclusively on the best method of performing the more important microscopical manipulations, leaving the student to find out for himself elementary details; but has descended into these matters with a minuteness which cannot be too highly praised, seeing that it is at the very beginning, where even the most intelligent students chiefly require instruction. On this part of the subject he very justly remarks, "I feel very strongly how intimately success in microscopical, as well as chemical, inquiries, is connected with a readiness in surmounting comparatively small difficulties, and with the possession of mechanical dexterity; and I should be doing great injustice to my pupils if I did not bring these processes under their notice, and endeavour to facilitate, as far as possible, the performance of those operations which are essential to the successful demonstration of the arrangement of textures under the microscope. . . . . He who is most fully conversant with elementary detail will be the most successful in the consideration of the higher and more abstruse problems, while he will feel a real love for his work, which is denied to the mere superficial inquirer." Much valuable information is given, also, with respect to the choice of an instrument, and what are the absolute requisites which one must possess to pursue researches in a satisfactory manner, together with the ordinary cost of such instruments.

Dr. Beale's work is one which we would strongly recommend to any of our readers who may desire to make microscopical investigations in a serious and systematic manner. It contains nothing to amuse the general reader, it does not describe the wonders the microscope reveals, but it tells the earnest student the best manner of making these observations for himself and of preserving the objects he may examine; in short, it instructs him fully on those points without a thorough knowledge of which he can never take a foremost place as a microscopist.

CORRESPONDENCE.

Molybdenate of Ammonia a Test for Sulphur.

To the Editor of the Chemical News.

Sir,—In the last Number of your excellent Journal I see a statement of Schlossberger's, "that a dilute solution of molybdenate of ammonia, supersaturated with hydrochloric acid, gives a beautiful blue colour with a solution of sulphide of hydrogen or the sulphide of a metal. This test is more delicate than the nitro-prusside of sodium." In order that others may not be led astray by the above, I have to remark, that I tried in every possible way to obtain the reaction, but in every instance failed. The result indicated a dirty-bluish liquid, which might be mistaken for various refuse menstrua, *e.g.*, the runnings from a foul indigo vat. I expected to obtain a magnificent azure tint, which would at once prove an excellent test. I should never employ Schlossberger's method in seeking for sulphur, especially when we possess so sensitive a test as the lead one.—I am, &c.

Sheridan Muspratt, M.D.

Liverpool College of Chemistry, March 25.

Aniline Dyes.

To the Editor of the Chemical News.

Sir,—The patent laws really require serious modification. Constantly some one throws a sum of money uselessly away in patenting a process known and practised secretly before. Secretly, I mean, merely as regards its trade application: or else, by patenting an invention or discovery, the whole matter becomes easy to copy without actual infringement.

Last August I first succeeded in producing and dyeing a good colour by heating a salt of aniline with peroxide of mercury; the mass becomes green, froths up, and requires very careful management of the heat. I could not perfect the process till late in September, and, on the 4th of October, to avoid disputes, I communicated the entire process to Professor Calvert of Manchester.

There can be no doubt that aniline decomposes chloride of mercury—chloride of aniline being formed and oxide of mercury; and starting from this I succeeded in perfecting my process. I may state that its success depends on proper and careful purification. I have devised a ready and economical way of doing this, by which a great quantity of aniline is recovered.

In my own opinion aniline is very prone, in contact with substances containing oxygen in not very energetic combination, to form the colour, and to reduce the oxide, &c.

If sulphate of aniline be heated to a low temperature with oxide of mercury, a dark blue shade is procured; a higher heat makes a redder shade. In fact an almost pure scarlet may be got.

I have observed a curious fact. If aniline and oxide of mercury be heated together, no colour is produced; but if aniline and bichloride of mercury and ammonia be heated together, small quantities of colour are produced, and mercury is precipitated in globules. Does this point to the conclusion that the presence of a salt is useful in the reaction? If so, perhaps the reaction that takes place may be something similar to this, which is confirmed by a long and tedious series of experiments I have lately made.

The aniline decomposes the metallic salt, and forms sulphate, or chloride, &c.; then, by the heat, the aniline becomes as it were nascent in the vaporous state, and reduces the metallic oxide, the chloride of aniline forming the salt, similar to chloride of mercury. I am unable to say what becomes of the chlorine; but after continuing the action of heat on an aniline salt and the mercuric oxide for days together, large quantities of chloride of mercury are invariably found present. Perhaps the oxide of mercury parts with its oxygen to the aniline in incipient vapour, and transforms it into fuchsic acid, while the chlorine goes back to the mercury. Perhaps the reaction is due to the evolution of chlorine and the mercury salt becoming decomposed into subchloride.

I have been led thus far into an almost unknown subject, and must apologise for troubling you with so long an epistle. I look forward with joy to the day of my friend Perkin's lecture on the matter. Perhaps he will, with his superior opportunity and talents, and having only a scientific end in view, develope more fully than I can do the reaction that takes place. I am compelled to avoid mentioning more of my experience in the matter for fear of intruding upon trade secrets.—I am, &c.

John S. Blockey.

Heckmondwike.

Royal Institution.—The General Monthly Meeting of the above Institution will be held on April 1, at 2 o'clock.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

The Amount of Carbonic Acid which Silicic Acid expels at High Temperatures, and the Formula of Silicic Acid.—M. Scheerer finds (*Annalen der Chem. und Pharm.*, bd. cxvi. s. 129) that the amount of carbonic acid which is expelled from pure carbonate of soda depends upon—1. The temperature at which the fusion takes place. 2. The duration of the fusion. 3. The relative proportions of acid and base fused together; and 4. On a peculiar relation of the carbonic acid to the base at a high temperature. The author's researches show that carbonate of soda loses carbonic acid as the temperature rises, and takes it up again as the heat sinks. By fusing together silicic acid and carbonate of soda it appears that three different silicates may be formed—1. The primary silicate, $3 NaO\ 2 SiO_3$, when one equivalent SiO_3 is fused with one and a-half equivalents of $NaO\ CO_2$, in which case the carbonate of soda is completely decomposed. 2. The intermediate silicate $2NaO\ SiO_3$, when one equivalent SiO_3 is fused with three equivalents $NaO\ CO_2$, in which case one-third of the carbonate remains undecomposed. 3. The maximum silicate, $3NaO\ SiO_3$, formed when one equivalent of SiO_3 is fused with an unlimited amount of carbonate of soda. Experiments with carbonate of potash yielded similar results. The maximum amount of potash which combines with one atom of silicic acid, when this is fused with carbonate of potash, does not appear to exceed two atoms. No higher silicate of potash seems to be formed than $2KO\ SiO_3$. The lowest formed is $KO\ SiO_3$. Between these an intermediate silicate may exist. Considering the combining proportions of silicic acid with potash and soda, the author gives the preference to the formula SiO_3 for silica.

Phosphide of Hydrogen.—Landolt shows (*Annal. der Chem. und Pharm.* bd. cxvi. s. 193) that when the non-spontaneously inflammable phosphide of hydrogen prepared by Rose's method is passed into pure nitric acid, sp. gr. 1·34, contained in a small porcelain capsule, the bubbles do not ignite when they come in contact with the air; but when only a drop or two of red fuming nitric acid is added, ignition takes place. A large addition of nitrous acid prevents the ignition. It appears, in the former case, that the nitrous vapour reduces the compound H_3P and gives rise to the formation of the spontaneously inflammable gas H_2P. A small quantity of chlorine or bromine added to water or pure nitric acid, causes no ignition.

II. ORGANIC CHEMISTRY.

The Constitution of Scammony Resin.—The researches of H. Spirgatis (*Ibid.* s. 289) confirm the results obtained by Mayer, and also by Kosman (Chemical News, vol. ii. p. 167), which proved the identity of the composition of the resins of scammony and jalap $C_{68}H_{56}O_{32}$. The author obtained pure resin of scammony by dissolving the rough in alcohol, then adding water until the solution became cloudy, afterwards decolorising by animal charcoal and then distilling off the alcohol until the resin separated. The separated resin was afterwards treated a long time with hot water, whereby a small quantity of a volatile acid, which perhaps gives the odour to the scammony, was driven off. The pure resin is amorphous, perfectly colourless, transparent, odourless, and tasteless. About 150° C. it melts into a clear, colourless fluid, and at a stronger heat begins to decompose, giving off a pungent smell. When burnt, it leaves no trace of ash. It is soluble in alcohol in all proportions, and the solution has an acid reaction. It is also easily soluble in ether, benzol, and chloroform, and, with difficulty, in naphtha and oil of turpentine. Warm acetic acid also dissolves it without any decomposition. From eight analyses the author gives the following as the composition of the pure resin:—

Carbon	56·50
Hydrogen	7·97
Oxygen	35·53
	100·00

Scammonic Acid, $C_{68}\ H_{56}\ O_{32} + 3HO$. When the resin of scammony is dissolved in potash, soda, ammonia, or baryta water, no resin can afterwards be separated from the solution. Under such treatment the resin takes up three equivalents of water, and becomes an acid. The experiment is best made with baryta water, because the baryta is easily removed. The acid forms a shining, amorphous, transparent, brittle, yellowish coloured mass, which resembles the original resin. It is very deliquescent, has a sharp acid and then bitter taste, but no smell. It is easily soluble in water and alcohol, but hardly so in ether; the solutions react strongly acid. It expels carbonic acid from the carbonates of the alkalies. Acetate of lead gives a dense precipitate with the aqueous solution. The analysis of the acid gave as the per-centage composition C54·59 H8·16 O37·25. By treating the resin of scammony and scammonic acid with a strong acid, sulphuric or hydrochloric, they are split up into sugar, and a new acid, which the author names *scammonolic acid*, the *scammoneol* of Kosman, but our authors do not agree as to the composition. Pure scammonolic acid—$C_{32}\ H_{30}\ O_6$—says Spirgatis, forms shining white tufts of needles, hardly perceptible to the naked eye. It is odourless, but has a sharp acid taste, is easily soluble in alcohol and ether, and the solutions have an acid reaction. It melts at 64° or 65° C., and makes a greasy stain on paper, but congeals again at 60°, and then forms a hard, brittle, crystalline mass. When burnt on platinum foil it gives off white fumes, which are exceedingly offensive to the eyes and nose. The mean of five analyses gave as the per-centage composition C70·95 H11·51 O17·54. The author describes several scammonolates, which we need not notice. By treating the scammonolic with nitric acid the author obtained another acid, which Mayer has named *ipomic acid*, the properties of which seem greatly to resemble the scammonolic.

MISCELLANEOUS.

The Houses of Parliament.—The Committee appointed by the Government to inquire into the causes and remedy of the decay in the stone used in the Palace of Westminster met on Saturday, the 23rd instant, at Richmond Terrace. There were present:—Sir R. Murchison, Messrs. Tite, M.P., G. G. Scott, Sidney Smirke, G. Godwin, C. H. Smith, G. R. Burneil; Professors Frankland, Hofmann, F. A. Abel, and Ansted; and Mr. D. Wyatt. The Committee appointed Mr. Tite Chairman, Mr. Bonham-Carter, M.P., being the Secretary. The Committee adjourned to Monday, the 8th of April, at twelve o'clock, when they propose to make a thorough examination of the building itself.

Diminution of Salt in certain Seas.—At a recent sitting of the Academy, M. Babinet read a paper on the diminution of salt in the waters of certain seas. Those inland seas, like the Euxine, which are constantly receiving fresh waters from the rivers, while their salt is being successively abstracted by some outlet, such as the Bosphorus, must, in course of time, be transformed into fresh-water lakes. This has been the case with the Lake of Baikal, in Siberia, whose waters were formerly salt, but are now, according to the learned member, as pure as distilled water. During this process, which it has taken ages to accomplish, the fish which were indigenous before have continued so, although the water is now fresh. Among them are a kind of herring called *omoul*, and also seals of the same kind as those to be found in the Polar Seas. M. Babinet, therefore, concludes that the acclimatization of herrings and seals in rivers is possible, and strongly recommends the Society of Acclimatization to try the experiment, especially with the latter species, which might very well be reared in the lakes of the Bois de Boulogne, giving as a reason that its flesh constitutes the exclusive food of the Esquimaux and Samoyedes.

A New Canadian Dye.—Professor Lawson has prepared a new dye of great richness, in the Laboratory of Queen's College, Canada, from an insect, a species of Coccus, found for the first time last summer on a tree of the common black spruce (*Abies nigra*, Poir), in the neighbourhood of Kingston. This new dye closely resembles true Cochineal, a most expensive colouring matter capable of being produced in warm countries only, and which is used to give a fine and permanent dye in red, crimson, and scarlets, to wool and silks. Unlike Cochineal, the new dye, discovered at Kingston, is a native Canadian product, and capable of being produced in temperate countries. Having been but recently observed, a sufficient quantity has not yet been obtained for a complete series of experiments as to its nature and uses; but the habits of the insect, as well as the properties of the dye, seem to indicate that it may become of practical importance. In colour it closely resembles ordinary Cochineal, having rather more the scarlet hue of the flowers of *Adonis autumnalis*, and no doubt other shades will be obtained. The true Mexican Cochineal is now being cultivated in Teneriffe and other vine-growing countries of Europe and Africa, with such success as to displace the culture of the grape vine.

What to observe in Lichens.—There are no plants so variable in character as the lichens; none in which it is consequently so difficult to decide what are species and what are varieties. In order to a comprehensive knowledge of species, it is necessary to study individuals in every condition of growth and from every possible habitat. Hence, the commonest species and varieties become of value—the more so if collected in countries comparatively unexplored botanically, for lichens are no exceptions to the rule that geographical differences are attended by corresponding differences in the characters of the same plants. Every collector of lichens—however common and well-known the latter may be—many therefore consider himself as contributing towards a more scientific and philosophical, because more comprehensive, knowledge of a very Protean, but interesting, group of plants. If the collector make a point of gathering specimens of everything he meets which belongs to the lichen family, he will run a good chance of including some novelties, perhaps new species or varieties. New species are most likely to be met with among the very minute crustaceous lichens which grow on rocks or trees, and which cannot be properly studied without the aid of the microscope; among species, belonging, for instance, to such genera as Lecidea, Lecanora, Grophis, Opegropha, Calicium. It is not to be expected that the tyro should make these microscopical examinations or discoveries for himself: he will probably require the assistance of some experienced microscopist or lichenologist. The applications of lichens to the arts are daily becoming more numerous and important. New dye-lichens are being discovered in India and the East. Again recently the probability has been shown, on good ground, that a lichen—the Lecanora esculenta of Pallas—was the Manna of the Bible. The colorific capability of a lichen, so far as regards a red or purple dye of the nature of orchill or cudbear, may be readily discovered by simply macerating the lichen—chopped into small fragments or pulverised according to the nature of its thallus—in a weakish solution of liquor ammonia—allowing the mixture to stand a few days in a warm part of the house, and shaking it frequently, so as to expose the mass to the action of the air. The colorific capability of a lichen, so far as regards other colors—chiefly brown and yellow—may be easily ascertained by simply boiling the lichen, chopped or pulverized as before, in a small quantity of water. Whether and how much mucilage or starch a lichen contains may be ascertained by the same means as last mentioned, and allowing the mixture to cool, when it will gelatinise more or less, if it contain much mucilage. Lichens are very easily collected and transported; they require no sort of preparation; they may be simply allowed to dry in the open air and packed as convenient. Those growing on trees generally require the piece of bark on which they grow to be sliced off with a knife, and those on stones the piece of rock to be broken with a hammer. Both may be wrapped in paper like mineralogical specimens. In all cases the localities and dates of collection should be mentioned, and any further information as to uses, &c., which may be known to the collector.—*Dr. W. Lauder Lindsay.*

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W. P——n.—By applying to the patentee you could probably get some; but we do not know his address.

F. B., Son, and H.—We trust they will shortly be resumed. It, however, depends upon the author, not upon ourselves.

J. Driver.—Use a solvent in which each is soluble. Benzol, for instance.

A Workman.—See answer to *Metallurgist* last week.

A Constant Reader.—1. It is a simple matter of calculation, which our correspondent can do as well as ourselves. The price would be over 20s. per 1000 cubic feet: for oxygen, that is very cheap. 2. A clean plate of copper placed in the solution, will precipitate all the silver on to its surface, no special galvanic arrangement is needed.

Books Received.—"Elements of Agricultural Chemistry." By Thos. Anderson, M.D. Edinburgh: Adam and Charles Black. "The Journal of the Board of Arts and Manufactures for Upper Canada" (in exchange).

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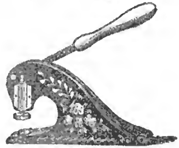

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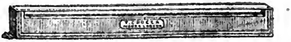

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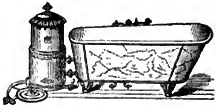

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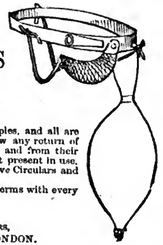

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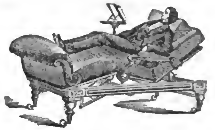
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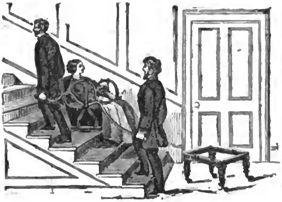
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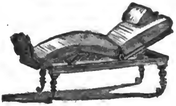
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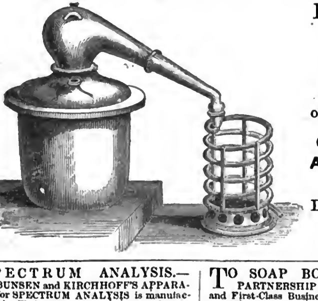

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THE CHEMICAL NEWS.

Vol. III. No. 70.—April 6, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Detection of Alum in Bread,

by William Crookes.

This problem is one of far more difficulty than is generally imagined, and it is, doubtless, to this fact that the discordant results obtained by different analysts are to be attributed: one stating that out of sixty-four samples of bread purchased at various shops in poor neighbourhoods at the East of London, where, if anywhere, adulteration would be practised in the most barefaced manner, not a single one was found to contain alum: whilst another analyst, with equal positiveness, mentions the name of a baker who is, in his opinion, almost the only person in a large district at the West End of London who sells unadulterated bread, and proceeds to state that more than 87 per cent. of the bread in London is adulterated. Very few of those who have published anything on this subject give details respecting the process they adopt, but in most instances it seems to be somewhat to the following effect:—The bread is first charred and burnt nearly to an ash; the latter is then boiled in diluted hydrochloric acid, with which a little nitric acid has been mixed; ammonia is then added, and the precipitate which it produces is boiled in potassa. After filtration, hydrochloric acid is to be added in excess, and then ammonia, when it is supposed that the precipitate will consist of alumina. With a pure solution of alumina to start with, doubtless this process would give accurate results; but it must be remembered that in bread the alumina would be accompanied by phosphoric acid, as well as phosphate of lime and phosphate of magnesia, each of which would make its appearance in the last precipitate with ammonia, and would, consequently, pass for alumina. But, granting that this tendency of the phosphates of lime and magnesia to simulate the reactions of alumina had been provided against, it seems to have been almost entirely overlooked by popular writers on this subject that whenever alumina and phosphoric acid meet together in solution, they adhere with the greatest pertinacity, and will infallibly appear together in the last precipitation. I should not have deemed these points worthy of mention did I not know that many analysts are habitually employing similar processes to the above, and are even estimating quantitatively the amount of adulteration in bread by weighing this precipitate of the mixed phosphates of lime, magnesia, and alumina, and calculating it as pure alumina.

My attention was first drawn to this subject by the fact that a sample of bread which was known to be entirely free from adulteration had been pronounced by a somewhat experienced analyst as being largely adulterated with alum. My assistance was asked in order to disprove this injurious allegation, and, having accordingly submitted the subject to a somewhat lengthened examination, I am induced to lay the results before the readers of the Chemical News, in the hope that when the attention of chemists is drawn to the subject, it may be investigated as fully as its commercial importance deserves.

The great difficulty in my hands has been to devise a process which should not confound other things with alumina. It was easy to frame various modes of operating by which a minute trace of alumina could be detected, but I was for a long time baffled by finding that they were equally delicate in their reactions, whether alumina were present or not. In fact, I do not hesitate to say that the accurate analysis of a mixture of those phosphates which are precipitated from an acid solution by ammonia is one of the most difficult problems in inorganic chemistry that the chemist is liable to meet with in technical analysis. I do not pretend to have yet solved the difficulty, but the process which I have at last adopted has at least the merit of not showing the presence of alumina when that body is absent. It has, on the other hand, the inconvenience of being rather tedious in its manipulation, and to some may seem to be needlessly complicated. No one can be sensible of this fact more than myself; but of the numerous methods which I have tried, both with and without separating the phosphoric acid, this was the only one which invariably gave me trustworthy results.

The bread, of which at least 500 grains should be taken, is first to be incinerated in a platinum or porcelain dish, until all volatile organic matter has been expelled and a black carbonaceous ash remains. The temperature must not be raised much beyond the point necessary to effect this. Powder the coal thus obtained and add about thirty drops of oil of vitriol, and heat until vapours begin to rise; when sufficiently cool, add water and boil for ten minutes. Filter and evaporate the filtrate until the fumes of sulphuric acid begin to be evolved, when ten grains of metallic tin and an excess of nitric acid must be added, together with water, drop by drop, until action between the acid and metal commences. When all the tin is oxidised, add water and filter. Evaporate the filtrate until fumes of sulphuric acid are again visible, when more water must be added, and the liquid again filtered if necessary. To the clear solution now add tartaric acid, then ammonia in excess and sulphide of ammonium. Evaporate the liquid, containing the precipitate suspended in it, in a dish until all the smell of sulphide of ammonium has disappeared. Filter, evaporate to dryness, and ignite to get rid of the organic matter. Powder the black ash, boil it in moderately strong hydrochloric acid, filter, add a crystal of chlorate of potash, and boil for a minute. Now add chloride of ammonium and ammonia and boil for five minutes. If, at the end of that time, anty precipitate is observed it will be alumina. From he filtered solution, if oxalate of ammonia be added, the lime will be precipitated, and if to the filtrate from this, ammonia and phosphate of soda be added, the magnesia will come down.

The following is a synopsis of the process :—

Incinerate to black ash. Powder. Moisten with $HO.SO_3$, add HO. Boil. Filter, and evaporate nearly to dryness. Add $HO.NO_5$, Sn and HO if necessary. When action ceases add HO and filter.

<table>
<tr><td rowspan="3">Precipitate contains all the PO_5 in conjunction with the SnO_2.</td><td colspan="3">Evaporate filtrate nearly to dryness, add HO, filter if necessary, add $2HO.\overline{T}$, AmO, and AmS., evaporate and filter, evaporate filtrate to dryness, ignite, boil black ash in HCl. Add $KO.ClO_5$. Boil. Add AmCl and AmO. Boil. Filter.</td></tr>
<tr><td rowspan="2">Precipitate contains all the Al_2O_3.</td><td colspan="2">To solution add $AmO.\overline{O}$, boil, filter.</td></tr>
<tr><td>Precipitate $CaO.\overline{O}$</td><td>To solutn. add AmO and $2NaO.HO.PO_5$, precipitate of $2MgO.NH_4O.PO_5$.</td></tr>
</table>

On a Series of New Combinations of Ammonia, Picric Acid, and Metallic Bases,[1] *by* M. Carey Lea.

(*Continued from page* 196.)

Analysis.

·4245 substance gave ·2632 picric acid.
·5865 " " ·3600 "
·4245 " " ·1577 chloride of silver.
·5865 " " ·2177 " "
·3788 " " ·4932 chloroplatinate of ammonia.

This leads to the formula :—

$$2N\overset{\frown}{H_3Ag}OC_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.$$

Dried over sulphuric acid *in vacuo* :—

	Calculated.	Found.		
		I.	II.	Mean.
$C_{12}H_2N_3O_{14}$ . .	61·62	61·73	61·11	61·42
Ag	29·19	27·96	27·94	27·95
$2NH_3$	9·19	9·93	—	9·93
	100·00	99·62		99·30

The silver falls below, and the ammonia exceeds the calculated amount in consequence of the impossibility of obtaining the salt quite free from admixed picrate of ammonia.

This beautiful salt appears to be one of the most permanent of this very unstable class of substances. It dissolves readily in hot water containing ammonia, sparingly in cold, and crystallizes in fine needles from the hot solution. Heated on platinum foil it detonates and leaves a brilliant spot of metallic silver.

Ammonia-picrate of copper.

A cold saturated solution of picrate of ammonia added to an ammoniacal solution of sulphate or nitrate of copper, immediately throws down an abundant precipitate, which, after washing with strong solution of carbonate of ammonia, and afterward with dilute ammonia, exhibits a beautiful pale greenish yellow colour with a shade of red through it. It is in fine scales, and much resembles the dust of the wings of butterflies.

Analysis gave for this salt a constitution similar to that of the silver salt.

·6059 gms. substance gave ·4652 picric acid.
·9310 " " ·1279 oxide of copper.
1·4240 " " ·1577 metallic copper.
·6059 " " ·2190 chlorhydrate of ammonia.

The copper was determined in the one case by precipitating the cupric solution while boiling with hyposulphite of soda, dissolving in aqua regia, evaporating to dryness, redissolving in dilute chlorhydric acid, and precipitating with distilled zinc and estimating as metallic copper; in the other by precipitating at 212° by caustic soda.

These results lead to the formula :—

$$2N\overline{\overset{\frown}{H_3Cu}}O\;.\;C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.$$

Dried over sulphuric acid *in vacuo* :—

	Calculated.	Found.		
		I.	II.	Mean.
$C_{12}H_2N_3O_{14}$ . .	77·63	76·44	—	76·44
Cu	10·79	10·97	11·07	11·02
$2NH_3$	11·58	11·52	—	11·52
	100·00			98·98

In an experiment to ascertain whether all the moisture was removed by drying *in vacuo* over SO_3, a quantity of about two grammes by exposure to a temperature of 210° to 212° for nine hours in the exsiccator lost a little less than one and a-half milligrammes.

Heated on platinum foil the ammonia picrate of copper explodes with some violence and with a dazzling light.

Ammonia-picrate of Cobalt.

To a solution of protochloride of cobalt prepared from chloride of purpureo-cobalt in the manner recommended by Gibbs and Genth in their interesting and valuable paper on the ammonia-cobalt bases, a large excess of ammonia was added, and to the filtrate, cold saturated solution of picrate of ammonia. An abundant highly crystalline precipitate falls at once, which after drying is yellowish green; while moist decomposes with great facility with separation of cobaltous oxide.

·6293 substance gave ·4784 picric acid.
·6075 " " ·4594 " "
·6067 " " ·1641 protosulphate of cobalt.
·5416 " " ·1475 " "
·6075 " " ·2065 chlorhydrate of ammonia.

Which results lead to the formula :—

$$2N\overset{\frown}{H_3Co}O\;.\;C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O+HO.$$

	Calculated.	Found.		
		I	II.	Mean.
$C_{12}H_2N_3O_{14}$ . .	75·87	75·69	75·29	75·49
Co	9·82	10·13	10·29	10·21
$2NH_3$	11·32	10·81	—	10.81
HO	2·99	—	—	—
	100·00			

This salt is much more unstable than the foregoing, and undergoes a commencement of decomposition in washing. Even when this part of the operation is performed with great care, using carbonate of ammonia and dilute liquid ammonia, a portion of picrate of ammonia is washed through, and as cobaltous oxide is not easily soluble in ammoniacal solutions, it is not easily removed by washing with such. It is also probable that more or less cobaltous oxide is thrown down with the precipitate, as the latter at the first moment crystalline becomes quickly more and more curdy; and ammoniacal solution of cobalt by mere dilution with water throws down a bright green precipitate. In the preparation of the ammoniacal solution, it is better to drop a strong solution of cobaltous chloride into a large excess of ammonia,

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[1] *American Journal of Science and Art.*

Like the foregoing, this salt explodes by heat.

Ammonia-picrate of Zinc.

To a solution of pure sulphate of zinc, chlorhydric acid was added in sufficient quantity to prevent precipitation by the subsequent addition of ammonia in excess. To the clear solution, a hot, strong solution of picrate of ammonia was added. Immediately the whole became nearly solid with beautiful yellow needles and scales; the beaker was rapidly cooled by cold water, the contents thrown on a filter, and washed first with a solution of carbonate of ammonia, mixed with caustic ammonia, and, finally, with dilute caustic ammonia alone.

The splendid gold-yellow mass of interlaced needles and scales was more or less decomposed, even by the most careful washing, so that the brightest and purest parts only could be taken for analysis, nor were even these satisfactory.

$C_{12}H_2N_3O_{14}$	mean of	5 determinations	81·26
NH_3	,,	3 ,,	9·06
Zn			10·08
			100·30

The formula $3N\widehat{H_3\,2}\left[ZnO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O\right]$ requires :—

$2C_{12}H_2N_3O_{14}$	79·692
3NH	8·913
2Zn	11·395
	100·000

The salt examined may be either this compound containing admixed picrate of ammonia, or may be a mixture of

$$2N\widehat{H_3Z}nO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O,\text{ with}$$

$$N\widehat{H_3Z}nO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.$$

The fact that when a strongly acid solution of zinc-chloride is supersaturated with ammonia, the compounds $2NH_3$ ZnO HCl and NH_3 Zn O HCl successively crystallise out, speaks in favour of the latter hypothesis.

Ammonia-picrate of Cadmium.

The cadmium salt is obtained and purified in exactly the same manner as the zinc, which it nearly resembles; it is, however, not so bright in colour, but pure canary-yellow. Like the zinc salt, it re-dissolves in its mother liquid by heat, without decomposition, but, like almost all the compounds here described, when washed with pure water on a filter, the picric acid and ammonia wash through. A grayish-white residue of oxide of cadmium remains behind.

Heated on platinum foil, the salt deflagrates with a beautiful green light.

·7170	substance	gave	·5247	picric acid.
·6622	,,	,,	·4877	,, ,,
1·1459	,,	,,	·8452	,, ,,
·6496	,,	,,	·1653	sal ammoniac.
1·1459	,,	,,	·2945	,, ,,
·6045	,,	,,	·6289	chlorplatinate of ammonia.
·7321	,,	,,	·1501	cadmium oxide.

These results lead to the formula :—

$$3N\widehat{H_3 2}\left[CdO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.\right]$$

	Calculated.	Found. I.	II.	III.	Mean.
$2(C_{12}H_2N_3O_{14})$	73·67	73·44	72·86	73·31	73·20
$3NH_3$	8·24	8·10	8·17	7·93	8·07
2Cd	18·09	17·94	—	—	17·94
	100·00				99·24

It seems, however, not improbable that this substance may be a mixture in nearly equivalent quantities of the salts :—

$$N\widehat{H_3C}dO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O\text{ and }2N\widehat{H_3C}dO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.$$

for reasons similar to those mentioned in the case of the zinc salt.

Ammonia-picrate of Chromium.

This salt is easily obtained from an ammoniacal solution of chromoxide, which may be prepared in the following manner :—

Three grammes of potash chrome alum are dissolved in water by boiling, the solution removed from the fire, and 14 cubic centimètres of pure concentrated sulphuric acid and 90 cubic centimètres of liquid ammonia added. A strongly-alkaline, beautiful purple solution is thus obtained, which may be preserved in well-stopped vessels, quite filled, for a considerable time.[1]

To a solution prepared in this manner, a strong, hot solution of picrate of ammonia, not in excess, is to be added. Some precipitation follows at once, which increases very much as the solution cools. The precipitate is thrown on a filter and washed with dilute liquid ammonia. It consists of splendid green, metallic-looking scales, which, if heated with a considerable quantity of water, even strongly ammoniacal, are decomposed with precipitation of chromoxide. On cooling, picrate of ammonia crystallises out. Even with very careful washing, the salt is always partly decomposed, with separation of green oxide of chromium.

The portions that were least decomposed were selected, but were not sufficiently pure to give any satisfactory result, although a number of specimens were analysed.

I.	·6949	substance	gave	·5864	picric acid.
II.	·5537	,,	,,	·4735	,, ,,
III.	·4446	,,	,,	·3709	,, ,,
IV.	·4446	,,	,,	·0179	sesq. ox. chromium.
V.	·6949	,,	,,	·0271	,, ,,

These numbers correspond to

I.	$C_{12}H_2N_3O_{14}$	84·02	per cent.
II.	,,	85·14	,, ,,
III.	,,	83·06	,, ,,
IV.	Cr	2·78	,, ,,
V.	,,	2·63	,, ,,

results which do not lead to any satisfactory conclusion, but show that the quantity of ammonia salt necessary to keep the chromoxide in solution throws down picrate of ammonia simultaneously with the ammonia-picrate of chromium. This salt is very beautiful; its lustre is remarkable.

Ammonia-picrate of Manganese.

If ammonia be added to a solution of manganous sulphate, previously rendered strongly acid with chlorhydric or nitric acid, a considerable quantity of manganese escapes precipitation, an extremely unstable solution is obtained, which, in a few minutes, even before filtration is ended, becomes cloudy again. To give the solution more stability, the presence of a very large quantity of ammonia salt is necessary,—a circumstance very unfavourable for the production of a pure ammonia-picrate, as, by a solution so strongly saline, picrate of ammonia (as already observed) is at once precipitated. The following were the observations made :—

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[1] While studying the chromium salt, I met with Frémy's "Investigation of the Salts of Chromium." In it the author states that the salts of chromoxide, modified by boiling (green modification), are insoluble in ammonia, except after having been boiled in acid and precipitated by ammonia. It will be observed that the above process is not in conformity with the opinion of M. Frémy, as it is there shown that the green modification, produced by boiling, is rendered soluble in ammonia by the simple addition of sulphuric acid, and without any application of heat. The solution in ammonia is always complete, and the process never fails.

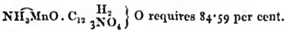
A. If a cold solution of picrate of ammonia be poured into an ammoniacal solution of manganese, prepared by adding to a concentrated solution of manganous sulphate an equal volume of dilute chlorhydric acid, and then ammonia in large excess, there falls a precipitate of brilliant, satiny scales, mixed, however, with precipitated manganous oxide, which is readily distinguishable on the filter.

B. If the ammoniacal solution is added to a hot, moderately-strong solution of picrate of ammonia, and the whole be rapidly filtered, the liquid passes through before complete precipitation takes place, and on cooling, granular crystals are obtained, which rapidly turn brown in the air. Heated on platinum foil, they turn brown, melt, and deflagrate sharply, with a brilliant white light.

Neither A nor B yield a salt sufficiently pure for analysis. A always contains admixed manganous oxide, and B picrate of ammonia. A bears considerable resemblance to the chrome salt just described.

1·0115 of B gave ·9115 picric acid.

So large a per-centage of picric acid corresponding to 89·72 per cent. of the molecule $C_{12}H_2N_3O_{14}$ probably indicates a large admixture of picrate of ammonia, which cannot be removed. The formula

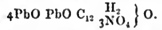


$$\widehat{NH_3}MnO\,.\,C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O \text{ requires } 84{\cdot}59 \text{ per cent.}$$

Ammonia-picrate of Iron.

An ammonia picrate of protoxide of iron appears to exist. If solution of protosulphate of iron be mixed with one of sal ammoniac, and then with liquid ammonia, be rapidly filtered, and the filtrate be treated with picrate of ammonia, a dark-green crystalline precipitate is obtained; thrown upon a filter, a red liquid passes through, containing a portion of the picrate of ammonia reduced by the ferrous oxide. The precipitate, when heated on platinum foil, deflagrates with scintillations. Heated with solution of sal ammoniac and caustic ammonia, it does not appear to dissolve, but ferric oxide is thrown down, and picrate and picramate of ammonia remain in solution. It was found impossible to obtain this salt in a condition to admit of even an approximate determination of its constitution.

If acetate of lead be treated with ammonia in excess, and if to the clear solution be added picrate of ammonia, a curdy precipitate, at first pale yellow, gradually deepening to orange colour, falls. Heated on platinum foil it detonates with violence. Boiled with caustic alkali, it disengages no ammonia. The result of an analysis showed it to be Marchand's penta-basic picrate of lead,



$$4PbO\ PbO\ C_{12}\left.\begin{matrix}H_2\\3NO_4\end{matrix}\right\}O.$$

The above described salts containing ammonia are all highly crystalline. The lead compound just mentioned is amorphous.

Other ammonia-picrates exist, which I propose to examine hereafter.

On Arsenical Paper-Hangings, by Dr. W. FRAZER, *Lecturer on Medicines to the Carmichael School of Medicine.*

HAVING recently been requested by some of my friends to examine paper-hangings for them, which were suspected to contain arsenical preparations, and having in every instance obtained not mere traces, but large quantities comparatively, of this substance from the paper thus tested, it occurred to me that a description of the simple means necessary to determine the fact would be interesting, and perhaps useful to many who might be desirous of having a similar question solved.

There is ample proof that the consideration of arsenical paints is at last engaging the attention of the public in a manner it has never heretofore done, and several papers have been published on the subject in the past year. The paper which was read by my friend Dr. Davy, at a recent meeting of the Royal Dublin Society, abounds in important and practical information. He detailed in it the results of his inquiries, and alluded to several striking instances in which deleterious effects have followed the use of these arsenicated papers, and also the employment of a green arsenical distemper, or water colour, applied as a paint to the walls of rooms in a public institution in this city. He further demonstrated, and I think sufficient notice has not yet been drawn to it, that these same obnoxious colours are lavishly employed in other manufactures, as wax tapers, and in the colouring of green tarlatan for ladies' dresses, to which the poison adheres so loosely that slight friction removes it; and an instance was incidentally mentioned by Dr. Barker, proving that French confectioners have not quite given up the employment of these dangerous colourings, and in which serious results might this winter have readily occurred, from children eating their attractive but deadly sweets. It is right to state, in my opinion, that every such colouring is avoided by our best Dublin houses, and that these sweets come from abroad. To these instances I might myself add the very free use of arsenical green in the painting of blinds and woodwork. I do not purpose to enter into the details of the symptoms which are now recognised as attending the continued inhalation or absorption of small quantities of arsenic. I can readily understand that some individuals are much more susceptible of their injurious influence than others; and this is what we would be led to expect from our previous knowledge of the action of other mineral substances, such as either lead or mercury. But I think that these cases which have been accumulated add another proof to the experience already acquired on the subject, of the effects of poisonous substances in small doses, and almost, I might say, proving the existence of a general law, that "minute, but far from imaginary, homœopathic, or infinitesimal quantities of mineral preparations can and do produce decided and well-marked symptoms in the human frame." Burton, Tanquerel, and others, have well worked out the details of the pernicious influence of lead preparations when absorbed in small and repeated quantities; and it is truly wonderful how small an amount of this metal may produce serious results. In connexion with this, let me detail the following case :—I was asked to see a woman some years since, labouring under obscure and unexplained *malaise;* she had wandering rheumatic pains, constipation, and colicy attacks, for which she had been under medical care for upwards of a fortnight, and as yet with little or no improvement. I chanced to look at her gums, which I always do in obscure attacks of abdominal pain, and the well-defined blue line at once told its tale of lead as the source of the attack. I had considerable difficulty in getting any history of her exposure to its action; at last it came out that she was employed as a care-taker in an empty house; that the family had been away, and that painters were working there; and, finally, that she used to sleep in the room in which the paint-pots were kept, and that for several nights some of these pots were close to her bed. She rapidly recovered by using suitable treatment.

Copper, too, is another of those metals whose insidious action upon the system, when absorbed in small quantities for a considerable period, is undoubted; and Dr. Corrigan's purple line around the gums is as distinctive and conclusive of its presence as the blue line of Burton is well known to be of lead. In a case which I saw in Dr. Corrigan's wards, there was no doubt either as to the existence of a purple copper-poison line upon the gums of the patient, or of the presence of those symptoms which copper causes; but how was the entrance of copper into the man's body to be accounted for? His trade was that of an upholsterer's workman; he had to all appearance nothing to do with copper, and still, on strict examination into his history, it was found that for some weeks he had been chiefly, if not altogether, engaged in polishing and cleaning up old brass furniture castors, the dust from which he stated, when closely questioned, had often affected his taste unpleasantly when working at them. Yet the symptoms caused by both copper and lead absorbed in this gradual manner are very different in character from such as attend their exhibition in full medical doses.

Shall I also instance mercury,—how well recognised as the result of exposure to it in continued, though minute doses, is the metallic trembling, the palsied state of a looking-glass maker, or a button gilder, and yet how distinct from salivation, griping, and other well-known symptoms of the influence of this mineral in full therapeutic doses. In fact, we are but beginning to recognise the truth of that law which I allude to, that the influence of medical agents, especially mineral substances, over the human body, differs not only in degree, but even in essential characters, according to the doses and the mode of exhibition. Palpably I would say that one special class of symptoms will be produced by exposure to the continued absorption of minute doses; and this is noticed alike, whether the substance be inhaled by a painter working in lead preparations, a person inhabiting arsenical-papered rooms, or one breathing the fumes of mercury, or from the repeated internal employment of the deleterious substance, as in drinking leaden waters. That a distinct class of symptoms will attend the use of these same substances when employed in their ordinary medical doses,—a class of symptoms best known to us from their being most frequently brought before our notice; and, lastly, that a third class of symptoms are to be traced when these preparations are exhibited in excessive, or, as they are popularly termed, poisonous quantities. I do not wish to be understood as asserting that the symptoms produced by any one of these substances, arsenic, copper, or lead, have nothing in common in these three modes of exhibition, but that they present broad and well-marked distinctions, valuable to us as practical guides, and interesting as abstract truths.

But, to return to the subject of testing arsenical paper-hangings, I wish to point out a very simple and satisfactory mode of examining these papers, which was first mentioned to me by Dr. Davy. The powder detached by scraping from the surface of the paper, or any other substance which we desire to examine for arsenic, is intimately mixed with about twice its bulk of pulverised yellow prussiate of potash, which has been previously dried to deprive it of its water of crystallisation, and the mixture is placed in an ordinary reduction-tube, and steadily heated. I always wrap a small portion of moistened bibulous paper around the tube, about half way up, to aid the condensation of the arsenic at this part, which sublimes in the metallic state. I find in practice that the yellow prussiate of potash is much to be preferred to other reducing agents, and I think it will be found that it is so simple in its action, and so satisfactory in its results, that anyone with a moderate amount of care can at once determine for themselves the presence or absence of arsenic in paper-hangings. Let me say, in conclusion, that it is not necessarily a bright-green paper that alone contains arsenic, many very pale greens and greenish-white colours also yield it; and at present I would be disposed to look with more than suspicion on almost all the green tints employed by the paper-stainer. There is no reason that this should continue, and in some of the compounds of chrome I have no doubt satisfactory substitutes might be found, even if they were more expensive than the gaudy but deadly arsenical tints.—*Dublin Hospital Gazette.*

Note on the Estimation of Uranium and Phosphoric Acid, by M. F. PISANI.

IF phosphate of soda is poured into an acetic solution of oxide of uranium, the metal is completely precipitated in the state of phosphate, having the formula,—

$$2\,(Ur_2\,O_2\,O)\,PO_5.$$

This precipitate, although gelatinous, deposits perfectly when hot, which allows of its being easily washed.

Estimation of Uranium.—After saturating with ammonia the liquid containing the uranium, acidify with acetic acid, then add a sufficient quantity of phosphate of soda. Allow the precipitate to deposit when hot, and wash it several times by decantation with boiling water. Care must always be taken when, after two or three decantations, it happens that the precipitate does not deposit itself readily, and the supernatant liquid becomes turbid, to add a little chloride of ammonium, which favours the settling. If this precaution is omitted, some phosphate of uranium passes through the filter, and considerably retards the filtration. Whilst the precipitate is on the filter wash it with hot water, with the addition of chloride of ammonium, and dry it artificially. Then separate it as completely as possible from the filter, calcine it in a platinum crucible, taking care to incinerate the filter separately. The calcined phosphate has generally a green tint, owing to a partial reduction, but that has very little influence on the results. The weight of phosphate of uranium, multiplied by 0,8023, gives the quantity of oxide Ur_2O_2, O.

Estimation of Phosphoric Acid in Combination with Alkalies, Alkaline Earths, and Magnesia.—Although the method of estimating phosphoric acid in alkaline phosphates by sulphate of magnesia yields excellent results, yet chemists well know that a long time is required for its complete precipation, and there is always a difficulty in detaching the portion adhering to the sides of the vessel. Even in this case the uranium method of estimation can be recommended. Moreover, it must be observed, that as the equivalent of phosphate of uranium is very high (it contains 80 per cent. of oxide of uranium), the loss ought to be less than with magnesia.

The following is the operation for estimating phosphoric acid by this means. After dissolving the phosphate, either in water or hydrochloric acid, add a certain quantity of nitrate of uranium, supersaturate with ammonia, and acidify with acetic acid: phosphate of uranium is thus precipitated. The precipitation is known to be complete by the supernatant fluid having a yellow tint, owing to excess of uranium. If incomplete, a fresh quantity of nitrate of uranium must be added, and afterwards ammonia or acetic acid. The precipitate must be treated with the precautions abovementioned. Its weight, multiplied by 0,1977, gives the relative quantity of phosphoric acid. This method of estimating is particularly applicable to the separation of phosphoric acid from magnesia, a separation usually attended with considerable difficulty.

I have also successfully utilised this method for analysing the uranite of Autun in the following manner:—

Dissolve the uranite in nitric acid; then, after separating the gangue, add to the solution some ammonia, and afterwards a slight excess of acetic acid. All the phosphoric acid is precipitated with the uranium, and the two bodies are thus estimated simultaneously. A little oxide of uranium usually remains in the filtered liquid, which may be separated from the lime in the ordinary way.

The estimation of phosphoric acid by uranium does not succeed well when phosphate of iron and of alumina are present, these being always precipitated in more or less amount with phosphate of uranium.—*Comptes-Rendus*, vol. lii.

On the Presence of Arsenic and Antimony in the Sources and Beds of Streams and Rivers,[1] *by* DUGALD CAMPBELL.

THE author has forwarded the following letter to the Editors of the *Philosophical Magazine*:—

"Since my communication upon the above subject,[2] published in the *Philosophical Magazine* of October last, I have repeated my experiments upon several of the sands I then reported upon, and with the like result which I then gave. I have also made experiments upon other specimens since obtained, and in all I have hitherto examined I have found arsenic, and generally, if not always, accompanied with antimony. The process followed was the same as I formerly described, only I invariably used hydrochloric acid without the slightest trace of arsenic in it, as some doubts had been cast upon my former results in a notice of my paper in the CHEMICAL NEWS of the 20th of last October, because in my anxiety to admit of anyone testing the accuracy of my results I had described how the process might be conducted with what is generally sold as pure acid, but which, if properly tested, is rarely free from arsenic.

"During these last experiments it occurred to me to distil the sands with a second and a third dose of acid; and in most cases I have found the yield of arsenic and antimony to be much greater—say from two to five times—in the second distillate than in the first; and in some I have found the third distillate to give more than the first, but in others less.

"These results induce me to say, that before a sand could be pronounced not to contain any arsenic or antimony it should be distilled to dryness with at least three distinct doses of acid, each distillate being tested carefully in the manner described in my former communication."

TECHNICAL CHEMISTRY.

The Society of Arts' Annual Exhibition of Inventions.

THE Thirteenth Annual Exhibition of Inventions was opened to the public on Monday at the Rooms of the Society of Arts, John Street, Adelphi. Many of the articles there exhibited are of a character to be of considerable interest to our readers, and we therefore purpose to give a short notice of those inventions which, upon careful inspection, we deem most deserving attention. The figures are the same which are attached to the articles themselves. The first in the Catalogue which attracts the notice of the chemist is No. 2. *A Specimen of Patent Fuel*, exhibited by Jabez Church. This fuel is manufactured from the breeze of gas-works, mixed with asphalte or coal-tar pitch, slaked lime, and a sufficient quantity of coal tar to render it adhesive. It is then placed in a wrought-iron scoop, which is to be put into a retort, such as is commonly used in gas-works, and then submitted to the action of heat during five or six hours, when it forms a fuel for locomotive engines, as well as for other purposes requiring the use of coke fuel. This would, in our opinion, be improved by leaving the lime out. It not only is useless in the fire, but it would remain behind as a caustic and irritating ash, which would be liable to cover everything near with fine lime-dust. Added to this, it would occupy space to no purpose,—an important consideration for steam-vessels. No. 8. *A Safety-pipe for Steam-boilers*, exhibited by Samuel Terrill. This pipe is placed in the boiler, and fastened at the bottom by rivets. Two inches above the furnace a lead ring is placed, forming part of the pipe. If the water should at any time be low the lead will melt and permit the steam to escape through the pipe at the top of the boiler. The lead being placed two inches above the furnace, will be prevented from corroding by any action upon it of sediment in the boiler. No. 17. *Sebill's Patent Water, Gas, and other Conduit Pipes*, exhibited by John Gedge and Son. These pipes are manufactured (by pressure in a cylinder) from a composition of waste slate, vegetable or mineral pitch or resin, and a small portion of animal or vegetable fibre or bristle. They offer the advantages of cheapness, comparative lightness, and facility of connection with each other, or with other pipes, jointing being effected by bringing the ends of the pipes together and passing a hot instrument over them, when the pipes will cement by running one into the other, or when softened at the joint a portion of the same composition may be used as a solder. By boring with a hot augur, any description of branch pipe may be at once introduced, or a tap. This plan of forming pipes is very ingenious, and the facility of connection or insertion of taps, &c., renders them superior to the bituminised-paper tubing, which we are sorry to find is not exhibited alongside. On the other hand, they would probably be inferior, in the matter of brittleness, to the paper tubes. No. 22. *Improved Safety-lamp for Coal-mines*, exhibited by Charles Edward Crawley. This lamp combines Crawley and Schneider's improvements with those of Howden and Thresh. Its advantages are stated to be,—1st. That it gives considerably more light than any other description of safety-lamp without the use of glass; this is effected by means of a tube, open at either end, fixed in the bottom of the lamp, passing through the wick, and protected at the lower end by a double gauze. 2nd. It can be immediately extinguished without trouble. 3rd. It has an insulated handle, which enables the miner to carry it for any length of time without burning his fingers, even were the rest of the lamp to become red-hot. 4th. It seldom, if ever, will become red-hot, whatever quantity of gas may be burning inside the gauze. 5th. It will, on account of its peculiar construction, consume, whilst burning with a good flame, from one to two feet of gas per minute, thereby tendering, under ordinary circumstances, to some extent to lessen the danger. 6th. The lock is rendered perfectly secure by means of a seal, consisting of a very small, thin metal disc, with any kind of device stamped upon it, fixed over the lock in such a manner as to render it absolutely impossible for the miner or any one else to open the lamp without breaking the seal, thereby forming a perfect detector. The seal would be varied from day to day, so that the miner would never be able to tell what seal would be used on any particular day. 7th. The great increase, however, in the light would of itself remove the temptation to open the lamp, added to which it gives, if anything, less light when opened. If light arising from combustion must continue to be used in coal-mines, this lamp is perhaps as safe a one as could be devised; but why is not electricity employed, in one of its many economical forms, as a means of lighting the miner at his task? No. 23. *Specimens of Machine-puddled Balls of Iron*, exhibited by William Yates. These specimens of iron were made by Tooth's patent self-acting puddling machine, which is a wrought-iron cylinder, lined with fire-bricks, and driven by steam power. The charge of pig iron is introduced at the chimney end of the barrel; in the door is a hole for inserting the rake, and which also serves as a peep-hole to watch progress. The barrel is kept stationary during the fixing of the pig, after which it is set slowly rotating and kept so moving till the charge has gone through all the usual stages of boiling, working dry, and dropping, which takes about an hour. The iron now begins to gather into a mass; the speed of the barrel is accelerated to solidify and shape the balls; the workman now, for the first time, has to put a tool to his charge, to cut off from the lump pieces suitable for his requirements; the fire-box is now shunted away and the orifice through which the flame had entered the barrel serves as the opening through which to admit the tongs, which are attached to a small crane affixed to the standing frame of the cylinder; thus the balls are extracted ready to be sent to the shingling hammer and rolls. The machine, the working of which these balls are illustrations, represents one of the most important improvements in the maunfacture of iron which has been made since the days of Cort. If it answers in general practice as well as it has on the small scale it will revolutionise this branch of manufacturing industry. No. 25. *Patent Metal for Bearings, Ships' Sheathing, &c.*, exhibited by John Gedge and Son. This metal is a composition of copper, zinc, and iron. From its toughness, it is stated to be a suitable metal for bearings of railway rolling stock or fixed machinery, and being cheaper than copper, and equally unaffected by sea water, can be used for sheathing ships' bottoms. No. 84. *Rostaing's Compounds of Gutta Percha with Mineral and Vegetable Colours and Substances for Dental and other Purposes*, exhibited by John Gedge and Son. For dental purposes the crude gutta percha is first purified from earthy and other matters soluble in boiling water, and then from oily and odorous parts soluble in alkalies; it is then heated to 110° or 120° Réaumur, to soften the substance; oxide of zinc, dense white or coloured, and a preparation of tannin are added, and the whole carefully mixed, and when it is desirable to give an agreeable odour, oil of peppermint or lavender, or an essential oil, mixed with a solution of gutta percha in chloroform, is added. Some of the preparations exhibited are composed of gutta percha and a mineral combination of blende, kaolin, and calamine, varied by the addition of a little catechu. The patent mineral colours used in combination with the gutta percha are unalterable and non-poisonous. We think that the employment of gutta percha which has been purified by Cattell's valuable process would be a great improvement upon the above. Its pure white colour would enable it to be tinted of any desired shade. No. 88. *Specimens of Aluminium and of Aluminium Bronze*, exhibited by Bell Brothers. The specific gravity of aluminum is 2·5, or about one-fourth that of silver, and thus, weight for weight, the bulk of aluminium is four times the bulk of silver. It does not tarnish by exposure to the air, has no perceptible odour or taste in the mouth, is malleable, can be forged, either hot or cold, equally well with gold and silver, and rolled into thin sheets or leaves; is ductile, so as to be capable of being drawn into fine wire. It further resembles silver in elasticity and tenacity, and, when cast, in hardness. When hammered, it takes the character of wrought iron, with elasticity and considerable rigidity, sounding like steel when let fall on a hard body. It is extremely sonorous, and M. Lissajous has made tuning-forks of it, which act extremely well. It melts at a temperature a little above that of zinc, and considerably below that of silver. Aluminium may be readily run into moulds, and when heated to a high temperature in the crucible loses none of its weight. From experiments made by Deville, he deduces its power of conducting electricity to be eight times that of iron; and as a conductor of heat it stands high amongst metals. According to this chemist, water, whether hot or cold, has no action upon the metal, even at a red heat near the point of fusion. It is, however, slowly oxidised when steam is passed over it at a white heat. Sulphuretted hydrogen has no effect upon it, nor has sulphur itself, so long as the metal is not heated higher than a red heat, though at a higher temperature they combine, forming sulphide of aluminium. Sulphuric acid, so diluted as to attack metals which ordinarily decompose water, has no action upon it whatever; and, according to De la Rive, the contact of a different metal, as in the case of pure zinc, does not help to dissolve the metal. Nitric acid, weak or strong, at the ordinary temperature, does not act on it, but when boiling it slowly dissolves it. Hydrochloric acid, whether weak or strong, is the true solvent for aluminium. Alkaline solutions have an energetic action on it, but caustic alkalies have no effect upon it, even when in a state of fusion. Ammonia exercises a feeble action. The organic acids, such as vinegar, tartaric acid, &c., have little or no action on it. The effect, however, of a mixture of vinegar and salt is different; for in this instance a small amount of hydrochloric acid is set free, which acts on the metal, but even this action is extremely slow, much slower than on tin. The salts of tin, too, have a strong flavour, whilst the salts of aluminium are less in quantity, and have little or no flavour. Deville considers that the action of sea water on aluminium is decidedly less than on copper. It can be gilt or plated by galvanic agency, but acid instead of alkaline solutions must be used. A coating of copper may in like manner be given. The effect produced on it by saliva is very slight, scarcely perceptible, even when the metal is kept for a long time in the mouth. Up to the present time no solder for joining it has been found which is satisfactory, though it is stated that M. Mourey has succeeded in this object, but his process is not known. M. Hulot has proposed to effect this object by covering the surface with a deposit of copper, and then employing the ordinary solders. The solution of this problem would tend much to bring the metal into general use. The alloys of aluminium and copper, forming what are termed "aluminium bronzes," are remarkable. That composed of 10 per cent. of aluminium and 90 per cent. of copper is probably the most remarkable. It is a perfect chemical combination, and has no tendency, as is the case with ordinary alloys, to separate under the influence of heat. These proportions represent an exact number of chemical equivalents of the two metals. Aluminium bronzes are of a yellow or orange colour, closely resembling gold, and take a fine polish, equal to that of steel. The chemical properties are the same as those of other copper alloys. In tenacity they fully equal steel. Drawn into wire, No. 16 guage, the breaking strain of copper, according to Mr. Gordon, was 190, of iron 280, of aluminium bronze 434; showing a strain of 84 kilogrammes to the square millimètre. Good French iron, in Deville's experiments, broke at a strain of 60 kilogrammes the square millimètre, and steel wire at a strain of from 90 to 100 kilogrammes. It thus appears that steel, and that of a fine quality, only can stand a comparison with aluminium bronze in respect of tenacity. As regards hardness, a comparison was made between a steel and a bronze groove for the guide-blocks of a locomotive engine, and, after six months' use, no trace of wear was perceptible; the bronze gave a result equally good with the steel. It was also tried for the journals of the front wheel of a locomotive, with excellent results, its great malleability, combined with hardness and tenacity, rendering it well adapted for this purpose, where ordinarily a very brittle alloy is used. The bronze containing 10 per cent. of aluminium can be rolled at all temperatures, from cold up to a bright cherry-red. It rolls well at a bright red heat, breaks less and elongates more than pure red copper. It is difficult to roll cold, and after a number of passes through the rolls, it elongates no further; it is then necessary frequently to re-heat it, as it hardens rapidly under the rolls. It is desirable to roll it at as high a temperature as possible, short of fusion. Re-heating and plunging in water to cool renders the alloy more tractable than simply re-heating without dipping. If re-heated to a bright red heat, and not dipped in water until it has been left to cool in the air down to a low red heat, it is sufficiently malleable and ductile, when cold, to bear without breaking the ordinary manipulations in working it, except some descriptions of stamping.

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[1] *Philosophical Magazine*, Fourth Series, Vol. xxi. p. 318.

[2] CHEMICAL NEWS, Vol. ii. 218.

(To be continued.)

Natural Oxide of Silver.

The following letter appeared in the *Times* last week:—

"Sir,—Up to within a very recent period the existence of such a thing in nature as oxide of silver has been denied by the most eminent chemists.

"Science, however, has now demonstrated not only that it does exist, but also in enormous quantities in the neighbourhood of the ordinary metallic silver deposits.

"The result is simply this, as proved by recent experiments:—

"First, That in a ton of ore in its natural state, where the normal average of yield of the metal would be 13 ounces, now, by the new process, an average of 113 ounces of silver would be obtained, the overplus being derived from that which has hitherto been rejected as useless; and, secondly, That the cost of this new method of reduction is relatively so small as to render the expense but little more than nominal.

"The importance of this discovery to our own mines where silver is often found in close approximation to copper and other metals will be very great, but to some of the foreign ones, where the deposit is large and unmixed with other metals, the result in the shape of returns will be almost fabulous.

"Yours obediently, "Chemicus."

In corroboration of this, "R. G." writes to the same paper as follows:—

"Sir,—In your impression of the 29th of March you publish a letter from 'Chemicus,' on the important subject of 'Natural Oxide of Silver.' The period up to within which the existence of silver in the form of a natural oxide has been denied by scientific men of the highest chemical attainment has hardly yet passed away, there being even at this moment some whose opinions or prejudices are so strong that they have not given themselves the trouble to investigate the subject, and either still wholly deny its existence, or assert that, if it does exist, it can only be extracted in such small laboratory experiments as would render the discovery commercially valueless. The existence of silver in gozzans in some form hitherto unsuspected and unknown was first discovered two years since by Mr. F. Squire, who conducted a series of successful experiments in public with a view to the utilisation of the discovery, the result of all which goes to prove the accuracy of the figures stated by 'Chemicus' as the proportion of yield in the ores before and after treatment,—ores which in their natural state gave, upon assay, from 5 to 13 ounces of silver per ton, yielding from 54 to 216 ounces. These experiments were mainly conducted on a small scale, but the discoverer of the presence of silver in this form, and of the method of concentrating, or, as it may be perhaps more properly called, globulating the same, asserted that operations on a large scale would be even more successful, and it may be interesting to 'Chemicus' and to the public to know that this position he has been enabled to prove as incontestably as the former.

"The importance to this country generally, and to the county of Cornwall in particular, of this most interesting discovery it is impossible to over-estimate. There are millions of tons of gozzan in Cornwall, all known to contain silver, but believed to contain it in such small quantities as to be worthless, which may be worked under this discovery to an enormous profit. In Russia silver ores containing four ounces of silver per ton are worked profitably. In Mexico, where labour is dearer, the lowest yield is five ounces. Taking a comparative statement for the higher wages of England, it would not pay to work ores containing silver only in less quantity than 10 ounces to 12 ounces. The Cornish gozzans are known to contain from 5 ounces to 13 ounces on the average in their natural state, but it is proved that in the form of oxide they contain from 54 ounces to 216 ounces, according to the average of the trials yet made, while some have been found to be fabulously richer. It follows that if an average yield of 12 ounces could be worked without loss,—an average the lowest yet obtained by the process of reduction now adopted, 54 ounces would yield an immense profit,—the expense of raising, reducing, and crushing these neglected ores could not exceed 3*l.* per ton, while the value of 54 ounces of silver only would be over 13*l.*, and that of the average yet obtained over 35*l.* per ton.

"In every point of view, then, this fresh discovery of science may be looked upon as likely to confer great and lasting benefit upon the country. It will open up a vast field for the employment of labour as well as capital, and will turn into wealth thousands and tens of thousands of tons of mineral hitherto looked upon as waste and worthless."

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

Lecture VI.

Normal Constituents of Water as it occurs in Nature—Sources of Impurity in Water used for Domestic Purposes.

At the close of the last Lecture I directed your attention for a moment to a particular class of the constituents present in water as it occurs in nature, these constituents are generally regarded as insoluble, and yet we find them in water in very considerable quantities. I allude to the carbonates of lime and magnesia which are present, for instance, in the New River Company's water, to the extent of nearly 8 grains per gallon, in the East London, to the amount of 10·16 grains. These numbers refer to the carbonate of lime only. Then we have also carbonate of magnesia present—in small quantity, it is true, but still in an appreciable degree. The Thames Ditton water contains 1·27 grains in the gallon. Now comes the question, how are these substances, which are generally regarded as insoluble, dissolved in the water? Chalk or carbonate of lime is so nearly insoluble in water that one gallon of pure water will only take up 2·5 grains of this compound. Carbonate of magnesia is perhaps scarcely so soluble. Now, the cause of the solution of these two comparatively insoluble salts is the presence of carbonic acid among the gaseous impurities of water. I have already alluded to it as one of the gases dissolved in normal water, and I think I shall be able to show this power of carbonic acid in dissolving carbonate of lime. If we take some lime-water, which is quite clear and transparent, to begin with, and pass through it a stream of carbonic acid, which I shall get from this apparatus, we shall, I think, almost immediately begin to see a turbidity produced. [A stream of carbonic acid gas was passed through a vessel of lime-water.] There you see we have carbonate of lime thrown down. We have a "precipitate," as we term these insoluble matters that are thus separated from solution, and that precipitate is carbonate of lime; so that you see carbonate of lime is obviously insoluble in the water in which the lime itself had previously been dissolved. Now, I think we have perhaps thrown down as much as is necessary for our purpose, and I want to show you now that on dividing this turbid liquid into two portions we shall be able to dissolve one of them by means of water containing carbonic acid in solution, while distilled water will entirely fail to dissolve the other. We will try distilled water first. You see, of course, the solution is less turbid, but we certainly have no effectual solution of the carbonate of lime, or very little indeed. Distilled water generally contains a little carbonic acid, but not enough to dissolve the carbonate of lime. Now let us try the effect of the carbonic acid water. I think we shall not require to add so much of this, as of the other, and yet we shall in the course of a few seconds see a very marked difference between the two waters. Here you see we have perfect transparency produced. The carbonate of lime which was at first thrown down is dissolved by this carbonic acid water, while the distilled water had not the power of dissolving it. The precipitate of carbonate of lime was quite readily soluble in water containing carbonic acid, and hence the solution of this substance as it occurs in nature. The same remark applies to the carbonate of magnesia.

We alluded in the last Lecture to the property, possessed by most of these natural waters, of curdling soap by producing a precipitate in a solution of soap, and of possessing, consequently, the property which we generally term *hardness.* To which of the constituents of water is this hardness due? Is it due to the whole of the constituents? If not, to which? There is a very convenient mode of testing this. What is termed the "soap test" is used for this purpose. Here I have some of it which consists of a solution of pure white soap in dilute alcohol. The alcohol is made use of because the mixture of alcohol and water dissolves more soap than water alone. Now, let me add some of this soap-test, as it is termed, to the distilled water, and I will add it from a graduated vessel, in order that we may notice the quantity we take. Here is about a pint of distilled water, and I will add a very few drops of this solution of soap in the dilute alcohol. On shaking the vessel, you see we at once get a lather which is quite permanent, or very nearly so. It will break over the surface presently, but if I add a few more drops of the solution the lather, you see, is now practically quite permanent: we have, in fact, an excess of soap there over what is decomposed by the water itself. You see we have produced very readily, by this very minute quantity of our soap-test, this lather in the distilled water. Now, let us try the effect of the same test upon a solution containing salts of sodium—containing the sulphate of soda and the chloride of sodium, which are two of the principal alkaline salts contained in those natural waters. Here is some of this water; we have a larger quantity than in the former experiment, but I am not very particular about quantity. I will add about the same quantity of soap-test. You see we get a lather there, but not a permanent one yet. We must add a little more. We have now a very evident lather but still it is not quite permanent. We will make a third addition of this test. We have still added a very small amount. I have about twice as much water as in the former vessel of distilled water; and we have added about double the quantity of soap-test. Now, we will add in the same way some of this soap-test to the water supplied to this Institution. I will, in the first place, add a little more than was added to the distilled water in the first instance. You see we get not the slightest lathering effect there. We will add a much larger quantity. [A larger quantity of the soap-test was poured in.] But we have still no approach to a lather. We will add a very large quantity. Well, we are just approaching now the point at which a permanent lather would be produced. By the addition of a little more soap-test we shall get a permanent lather: but you see what a quantity of soap is lost in this hard water—what a large quantity of soap is consumed in the formation of this curd which I showed you in the microscope in the last Lecture, and what a very small quantity over and above this is necessary to produce this effect of lather in the water, and to give the water a detergent property. There, you see a very little more of this soap-test will suffice; at last, we have a lather with some pretensions to permanency. In the first place we had distilled water which produces a lather with a very small quantity of soap; then we had this solution containing alkaline salts which did not take more, bulk for bulk, than the distilled water; therefore we see that that water which contained ten grains of sulphate of soda, and ten grains of chloride of sodium is, notwithstanding practically as soft as distilled water. But where we have these salts of the alkaline earths, lime and magnesia, as in the waters supplied by the London companies, we require to add large quantities of the soap-test in order to produce a lather.

In order that you may see the hardening effects produced by the lime and magnesia salts we will add a little more of our soap test—an excess of soap to this water. We have, you see, a lather there. Now I will add to this water, with the lather upon it, one of the constituents which is generally present in hard water—namely, sulphate of lime which is present in the New River Company's water in the proportion of 3·23 grains to the gallon. This sulphate of lime is not very soluble in water; it requires about 500 times its weight of water to dissolve it, still I need add but a very small quantity of its solution to the water in which we have the lather. [A small quantity of sulphate of lime was then added to the water.] You see our lather has disappeared; we have no longer a permanent lather, we have no longer an excess of soap there. That very small amount of sulphate of lime added to that large quantity of water killed the excess of soap we had at first. The hardening constituents of water are the carbonates of lime and magnesia, sulphate of lime, nitrate of lime, and sulphate of magnesia. All the other constituents present in that analytical table do not practically harden the water at all.

Now, having this hard water to deal with very frequently, especially in such chalky localities as this where the water charged with carbonic acid percolates through the soil and readily takes up some of these salts which are otherwise nearly insoluble, it is a very important point to ascertain how this hardness can be removed—whether it can be removed by any process at all, and in the next place what is the best method of doing it?

The hardness resulting from each of the different hardening constituents is not equally capable of removal. The hardness due to the carbonates of lime and magnesia is comparatively very easy of removal. The hardness caused by the nitrates, sulphates, and other salts is very difficult to remove. The best means to remove, upon a small scale, the hardness, when it is due to the presence of the carbonates, is to boil the water. You see the condition upon which the solution of these carbonates depends is the presence of carbonic acid in the water; but water at the boiling temperature cannot dissolve carbonic acid, or retain it dissolved if it has already dissolved it beforehand. Consequently, in heating such water for some time every trace of carbonic acid is expelled, and the carbonates of lime and magnesia are precipitated. Here, for instance, is some of the water supplied to this Institution, and which has thus been boiled. At the bottom of the vessel is a white sediment of the carbonates of lime and magnesia, which has been deposited on boiling. The hardness of such water as this may be reduced to three and a-half or four degrees by boiling. What are these degrees of hardness? By this term the chemist understands an amount which is equal to one grain of carbonate of lime, so that you can see at a glance from the analysis of a water what will be the hardening effect of the carbonate of lime in it. For instance, this New River Company's water will have 7·82 degrees of hardness due to the carbonate of lime. But then the other hardening constituents—namely, sulphate of lime, carbonate of magnesia, and sulphate of magnesia—have hardening powers somewhat different from those of the carbonate of lime. We require somewhat more than an equal quantity of the sulphates of lime and magnesia to produce the same degree of hardness. We require rather less of the carbonate of magnesia to produce the same effect. But, practically, we do not mind to which of these ingredients the hardness may be due, as our soap-test affords a ready means of determining its exact degree. The soap-test is so made that, if one of these measures is required to produce a lather with a thousand grains of the water you are investigating, that water possesses a hardness of one degree, and contains either one grain of carbonate of lime to the gallon, or a quantity of the other constituents equivalent in hardening effect to one grain of carbonate of lime. Thus the quantity of the soap-test required will enable you at once to ascertain what is the comparative hardness of any particular water, and consequently the comparative amount of soap which will be expended in producing a lather, and the comparative amount of the greasy matter which will be produced before the detergent process can take place.

Another process, and a very beautiful one, for softening water, consists in the removing of the carbonic acid by the addition of caustic-lime, to the water. Caustic-lime has a strong affinity for carbonic acid. We passed a stream of carbonic acid gas into a solution of caustic lime, and we at once had the lime thrown down in the form of carbonate of lime, so that the lime had fixed the carbonic acid, which was in a gaseous state, and produced this white precipitate. If we add a further quantity of lime to take away the carbonic acid which has already dissolved the carbonate of lime, we shall effect a precipitation of the carbonate of lime, and we shall at the same time remove in the same form the lime we add to the water. Let me see if I can illustrate this by an experiment. Here is a vessel more than half filled with the water supplied to the Institution. This water [pointing to a vessel of lime-water] contains lime in solution. This water [referring to the vessel containing the water supplied to the Institution] contains carbonate of lime. The lime in the first vessel wants to get at carbonic acid and combine with it; and I dare say we shall find when we add this lime-water to the water in the other vessel, that we shall get a very copious precipitate of the carbonate of lime and also of the carbonate of magnesia; for I scarcely need to explain that the same thing which holds the carbonate of lime in solution holds also the carbonate of magnesia, and they are thrown down as a precipitate by the same means. Both are thrown down together. Let me try to illustrate this a little more clearly to you by our atomic cubes. Here we have the carbonate of lime contained in the water from the pipe. It is composed of one equivalent of lime and two equivalents of carbonic acid. To this we added in the larger vessel pure lime or caustic lime, which will be represented by this cube which we will add to the others. Now, the effect of the addition of lime was to cause an equal distribution of the carbonic acid between the two equivalents of lime, so that this atom we added ran away with half the carbonic acid of the bicarbonate of lime originally contained in the water, and left it with only one equivalent of lime; but this carbonate of lime, which contains only one equivalent of lime to one of the carbonic acid, is practically insoluble, and goes to the bottom. The carbonate formed by the union of the lime added in solution with the carbonic acid which was taken from the bicarbonate of lime was also insoluble, and was, therefore, precipitated. The carbonated hardening constituents of water may be thus removed, the subsidence being complete in the course of a few hours. Here is some water which has been thus operated upon, with the carbonates below, settled to the bottom of the vessel. Both these vessels contain water which has been operated upon in the same way. This [pointing to the water contained in one of the vessels] is the clearer. It was operated upon some six or eight hours ago. The other one was operated upon only two hours since. Now, this is really practicable upon a large scale. At the present moment water at Woolwich and Plumstead—which is very hard—is softened in this way. It is pumped out of the chalk strata and mixed with some milk of lime, and allowed some twelve or eighteen hours to subside before being used. It is thus softened from sixteen or eighteen to about six or eight degrees of hardness, which makes a considerable difference when you come to operate upon water with soap. This process, however, is one which is not adapted for being carried out by individuals. It is a process which can only be carried out upon a large scale, and involves a certain amount of trouble. I know a gentleman in London who softens the water supplied to him in this way; but it is not a convenient process for general adoption. There is, however, another way in which this softening can be performed so as to avoid the curdling of the soap, which I showed you in the last lecture, and which can be applied in a moment to a basin of water when it is about to be employed for washing. In order to show you the effect, I am about to employ hot water, because we get the effect far more rapidly and visibly with hot than with cold water, and it is a very visible effect which I must produce in order to render it evident to you that there is any effect at all. The method I am now going to explain to you removes the whole of the hardening matters, therefore it has an advantage over the lime, because the lime only removes the carbonates; but as these are generally the most abundant it reduces the hardness down to five, or six, or eight degrees. What I am going to do now is to add to this water a few grains of carbonate of soda—of the soda, in fact, which is used by laundresses for softening water. Although we do not mind rubbing the greasy matter into the skin, it is found that when such a substance is produced the water will not do for washing linen. Two or three grains of carbonate of soda to a pint of water are sufficient. You see in this case an effect is at once produced. Instead of having clear water as before we have turbid water, which is due to the precipitation of the hardening substances. It is quite easy to soften two or three gallons of water in this way when required. It is, however, advisable not to add a further amount than this, otherwise the alkaline carbonate of soda will produce an unpleasant effect upon the skin, which would not make its use popular. If, however, you guard against using too much you will find this use of carbonate of soda very agreeable and advantageous.

This method, then, as I have said, removes the whole of the hardness, and we have another process by which the hardening constituents; and, in fact, nearly all the impurities of water may be effectually removed, and that is distillation. But here again we have a process which is not practicable under ordinary circumstances. Water thus prepared would necessarily be a comparatively expensive article. The process is, however, carried out in the Navy, and the water is subsequently aërated by an ingenious process invented by Dr. Normandy. But this is an exceptional case. We could not expect this process to be adopted on land, where there are far more available processes capable of being applied.

I have now to notice very shortly the organic impurities contained in water. These are partly of animal and partly of vegetable origin. They are both of them decidedly objectionable in water, but the animal impurities are by far the most objectionable. I allude now to both classes of impurities when in solution, not in mechanical suspension, because we have seen the latter are capable of being removed by the simple process of filtration. These organic impurities consist of materials generally formed of the elements carbon, hydrogen, nitrogen, and sulphur, and the peculiarity of them is that, as they become dissolved, they are constantly undergoing chemical change, and it is during that process of chemical change that danger is to be apprehended. Substances of this kind containing nitrogen—and this is peculiarly the character of the animal impurities contained in water—constitute a class of substances which chemists call "ferments," of which yeast is a well known example. Now, these ferments have the property of causing chemical change in substances with which they are brought into contact, without actually entering into combination with the new compounds. Thus, in the case of yeast, sugar is resolved into alcohol and carbonic acid. Some of these ferments, when they gain access to the system, produce disease. We have, for instance, small-pox virus, and there are others of these ferments which produce a very definite action on the human system. Now, the mode in which ferments act cannot be said to be very clearly made out, but still it has been generally ascertained that the use of water containing any of this class of organic impurities in a state of chemical change—and they must be, if dissolved, in a state of chemical change at the ordinary temperature,—I say it has been ascertained that these are highly dangerous to health, and therefore the use of water containing these organic impurities, of animal origin, ought to be very stringently avoided. It is not possible, however, to give you any test by which these can be infallibly detected. The distinction between *animal* and *vegetable* impurities belongs to one of the nicest points of chemistry, but still there is a test which will assist us in the matter, and that is the permanganate of potash. This salt produces a solution of a very beautiful pink tinge. If this liquid be added to water containing no animal matter whatever, the colour will simply be diminished in intensity by being diluted with the water. In twenty or thirty minutes, if the water contains organic impurities, the colour will disappear. It disappears from vegetable impurities in time, and from animal impurities more rapidly. Let us now apply our test to see which of these two waters would be the best to be used as a beverage. We will add a drop of this test first to this water. I do not want to add too much. You may perhaps already be able to see a difference in the tinge of these two waters, even from that distance; the difference to me is very perceptible indeed here, and I think at least it ought to be seen at that distance. I do not want to add too much of this solution to it, because I want to show you that this very small quantity which I have added is sufficient to produce this very decided change in the tinge. Let us add about the same quantity of this solution to the other water. Now I think, if you compare them, you will find that there is a very decided difference, and that this last water is not altered in colour. You see in one case we have a water which is free from organic matter; in the other we have a water which not only contains organic matter, but contains organic matter of a dangerous kind; it is, in fact, water that has been intentionally mixed with sewage. It resembles some of the water which is sometimes pumped up from the wells in the streets. We will add a further quantity of the test, and you see with this large addition of pink solution we have not the slightest tinge of colour. Now we will add a still further quantity. You see with this large addition it is still practically colourless,—the tinge of colour is very imperceptible. I could go on adding until I had completely oxidised all the organic matter in this water, and as soon as that was accomplished, we should get a tinge produced by the first few drops of the solution in excess added to the water. You must not expect, if you apply this test, that you will find absolute freedom from organic matter; but still the colour ought to be only very slightly discharged if the water be fitted for use as a beverage.

The next form of contamination which is met with in water is that of lead, which results from the solution of the pipes and vessels through which we transmit, and in which we store up, the water that we make use of. Although we have the misfortune, here in London, to be supplied with very hard water, there is a little alleviation of this distress in the fact that this hard water is almost incapable of dissolving lead, and becoming contaminated with this form of poison. It is only when it is kept for a long time in lead vessels, and under circumstances favourable for the solution of the lead, that such contamination need be feared. Seeing that this is the case it is, perhaps, very unwise to adopt any method for preventing contamination which is not very certain in its action. Such a method is in use here and elsewhere. It consists in coating with a thin layer of tin the inside of the pipe; and no doubt, so long as the coating remains intact, you cannot have a trace of lead dissolved. But, in the first place, this tinning process may never be so perfect as to prevent some small portion of the lead from being uncovered; and, in any case, where joints are made, you will there certainly have some of the bare lead exposed. Then comes the question, Have we no voltaic action set up? There must be. Now if that action be in one direction, our pipe will be perfectly free from any contaminating influence; but if the action be in the other direction, the use of the tin will greatly increase the contamination instead of preventing it. The difference consists simply in the direction of the electric current which is always set up when two metals of different kinds are put in contact with water, if one of the metals is at all oxidisable. If the electric current pass in this particular case from the tin to the lead then the water will be effectually protected from being contaminated with lead; if, however, the current proceed from the lead to the tin, then if any trace of lead be exposed in the pipe, a voltaic action will be set up, which will cause rapid corrosion of the lead and its rapid solution in the water, and then the contamination from the lead will be produced perhaps a hundred-fold.

Let us try whether this tinning does effect the protection of lead or not. It is a question very easily settled. We have here some of the water supplied to the Institution, and we have here a galvanometer which will indicate the direction of the current. Here I have a sheet of tin and here I have some lead. If the red end of the needle moves towards you when I bring the two ends of the metals into the water, the question as to whether the tin protects the lead will be answered in the negative. In that case the current passes from the tin to the lead, and the tin will have the effect of causing a more rapid solution of the lead, if the latter be in any part uncovered. Now we will immerse the two plates without letting them come in contact. [No effect was produced upon the galvanometer.] There is some want of contact here. The wire is broken off here from one of the metals, so that there is not the slightest possibility of a current being communicated to the galvanometer. [The detached end of the wire was refixed to the plate of metal, and the experiment was repeated with success.] There you see the red end of the needle moves towards you, and the question is answered in the negative—the tin does *not* protect the lead, but on the contrary, it facilitates the solution of the lead in the water.

We all know that spring-water is a very refreshing and delicious beverage—very different from the kind of water we usually meet with in large towns. What is the reason of this? The reason is that in spring-water we have a well aërated water—a water which has dissolved carbonic acid and oxygen from the atmosphere in descending through it, and in percolating through porous strata where, it is true, it comes in contact with organic matters, but they are present in a very small amount. It goes down below the surface and acquires a mean temperature of somewhere about 50°, while the water we drink in London during the summer varies from 65° to 75°. Further, spring-water (if it be carefully selected at least) is free from any of the organic impurities of which I have spoken, and being used immediately it is brought out of the stratum where it has accumulated, it possesses those refreshing qualities to which I have just alluded. Now these conditions are quite within the reach of most, if not all, of the Companies supplying water to this metropolis. If you look at this list [referring to the table printed in the last lecture] you will find that we have not a very large quantity of organic matter, except in the case of the East London Company where the quantity of organic matter is more than four grains to the gallon. Many of these organic substances are, probably, entirely vegetable. The quantity you see in no other case is by any means excessive. We may say, in fact, that the London Companies have it in their power to supply us with water possessing very nearly an equal degree of purity and equally refreshing qualities with that which we obtain directly from the spring. But it cannot be done on the present system of supply. It is quite impossible, in the way in which it is supplied at present to have it in any other way than that in which it now reaches our lips, and these conditions are superinduced entirely through the mode in which it is supplied. Let us just glance at this mode. We collect the water in a vessel of this construction [pointing to a diagram of a square water-cistern, and supplied with water by means of a ball-cock]: the water is turned on for some twenty minutes during the twenty-four hours. Here is a ball-cock which rises with the water, and when the water rises to a certain height this cock prevents a further quantity of water from flowing in. There is also a waste-pipe which prevents the cistern from overflowing. The waste-pipe is fixed to carry off the water in case the ball-cock should get out of order, and the pipe communicates with the street sewer. It is, however, sometimes, perhaps generally, trapped. The lower end of the pipe is re-curved, and the water collects there, and prevents gases going back again into the cistern. So long as the water continues to fill the bend of the pipe, such an escape of gas from the sewer into the cistern is impossible, but then, if water be first poured down into this pipe, it will evaporate in the course of three or four weeks, and leave the space quite free for the passage of gas, and this ball-cock is so arranged that it does not let the overflow run down the waste-pipe, unless it gets out of order, which is, perhaps, once in six or seven years. When the water has evaporated from the bend in the waste-pipe, the sewer gases are free to pass into the cistern. You all know, when you open a window, the air rushes into the room; and in the same way the air from the sewer rushes up the waste-pipe into the cistern, and we have seen what a powerful absorbent of gaseous matter water is. The water becomes contaminated to a very considerable extent, and this contamination goes on until the water is almost saturated, because probably not one-third of this water is used in the day. The cistern is again filled up from the ball-cock, and the fresh stratum of water is again saturated in the course of a few hours with these noxious gases, and thus the water reaches our lips in this infected, unwholesome condition. The supply of pure and refreshing water is utterly impossible upon this intermittent system. You see, if the water were laid on so that, by opening a cock, we could at once obtain it without the necessity of storing it in a cistern, we should have at any moment water free from contamination, and perfectly free from these sewer emanations. Almost all the large towns except London are now supplied upon this constant system. Manchester is thus supplied, and the benefit which results is incalculable. We have now only one source of pure water in London, and that is our drinking-fountains. We cannot, however, have the water supplied to us in this state in our own houses, and the only plan to obtain it free from contamination is to send for it to the drinking-fountains.

I see, however, that our time is very nearly gone, and I have one or two other matters still to mention. The first is ozone. I have already stated that hydrogen is capable of uniting with one equivalent of oxygen, forming water; with two equivalents of oxygen to form binoxide of hydrogen; and with three equivalents to form "ozone," or, at all events, to form teroxide of hydrogen, the existence of which is theoretically indicated, if ozone be not this very compound. What is this ozone? Some chemists suppose it not to contain hydrogen; others think it does contain hydrogen; and a series of analyses certainly did seem to prove that hydrogen in the proportion I have stated was a constituent of ozone. Now, ozone is produced in two or three different ways. One of the most peculiar is by means of the electrical machine. There is a peculiar odour in the neighbourhood of an electrical machine when worked, and that odour is usually supposed to be due to ozone. It is caused by electric sparks passing through the air. We can collect this ozone in a variety of ways; but before doing so I must refer you to the liquid contained in this glass vessel, which furnishes a very delicate test for the presence of ozone, and of other matters too, as we shall presently see. It consists of a solution of iodide of potassium and starch. Ozone, containing as it does three equivalents of oxygen, readily gives up oxygen, and it is capable in this way of oxidizing the iodide of potassium and converting the potassium into potash. The iodine thus liberated forms an intense blue solution with the starch. Here I have a Rhumkorff's coil, which will give us a series of sparks. You see by the working of this air-pump the so-called ozonised air passes through the liquid, which becomes blue. You see we have now got a very decided blue colouration here. That is one mode by which ozone is produced; but we shall see that the blue colour is not entirely due to the presence of ozone. Now, another mode for the production of ozone is by placing moist phosphorus in a close air-jar. Here is some filter paper which has been imbued with the solution of iodide of potassium and starch, and which will become blue when it is plunged into this vessel containing ozone produced by this process. This constitutes what is generally known as the usual test for ozone in the atmosphere. Then we have another mode of producing ozone, and that is by electrolysis. When water is mixed with some highly oxidizing substance, such as chromic acid, we have this ozone produced when the water is decomposed by a current of electrity. It was from this source that ozone was produced for analysis, and it was from this that the formula I have here used was formed—namely, HO_3, three equivalents of oxygen and one of hydrogen.

Ozone, as I have said, is supposed to be present in the air, and many very careful observers are at the present time making observations upon the relative quantities of ozone present in the air. Papers of this kind [exhibiting ozone papers] are exposed to the atmosphere for certain fixed and definite lengths of time, the amount of blueness which they manifest in that time is carefully noted, and the intensity of this blue colour is supposed to represent the comparative quantity of ozone present in the air. Now, it is greatly to be regretted that such an amount of labour should be expended upon a matter which is utterly and entirely valueless, because this so-called "test" for ozone is really only a test for a great number of things which may—nay, do—exist in the air, and from which ozone may be really absent. We may say that there is not up to the present time a single experiment which demonstrates that ozone is present in the atmosphere; and certainly these tests, so far from proving its existence, do not even infallibly demonstrate the presence of an oxidising influence in the air. It is quite possible that this blueing of the paper may be produced in a condition of air very different from that in which ozone is present. Ozone being a highly oxidising substance, is supposed to decompose organic impurities in the air, and therefore the air which contains the largest quantity of this ozone is supposed to be the most wholesome. You have only to have present, in the air, some acid gas,—you have only to go into the neighbourhood of some chemical works, for instance, where hydrochloric acid gas is evolved,—and you will there have plenty of these manifestations of the presence of ozone. Here we have the test I have just applied and described. Here we have some solution of iodide of potassium and starch, and I acidify it with acetic acid. You see we have abundance of ozone indicated, but it is said that ozone is never present in this part of London. The liquid has become of a dark blue colour. The reason is, that the iodide of potassium which is contained in this liquid, and in all these test-papers, is decomposed by acids, and hydriodic acid, a compound of iodine and hydrogen, is immediately formed. It is only necessary to bring this hydriodic acid in contact with free oxygen; when the hydrogen is oxidized, and the iodine is liberated; so you see this so-called "test" for ozone is perfectly unreliable. But to what are the effects due which are produced upon the test solution by the air through which the electric sparks passed? They are due, as I will show you in the next Lecture, to the combustion of nitrogen, which forms nitrous acid when electric sparks pass through the atmosphere; and I think I shall be able to show you that this gas produces a very intense colouring effect upon the ozone test-papers.

SOCIETY OF ARTS.

Wednesday, March 20, 1861.

Professor H. W. Williamson *in the Chair.*

At this meeting Mr. Charles Tomlinson read a paper on *The Economic History of Paraffine*, a portion of which we will extract, as our readers can hardly fail to regard the subject as an interesting one. After the usual introductory remarks, Mr. Tomlinson said:—

"There is scarcely a better known natural product than common coal, and yet how marvellous are the properties of that familiar substance. Its history, if fairly written, would be of the bulkiest; it would take us into many sciences, it would draw upon many arts, and when written—when all that is known respecting it had been set down—the last words of the last page must still be, 'To be continued.'

"Such must ever be the case when the finite attempts to take measure of the Infinite—when the human seeks to scan the purposes of the Divine mind in the varied relations of an individual specimen of any one of the great kingdoms of nature.

"I trust the Society will not deem these reflections impertinent to my subject, which, though including only one of the applications of coal, is indeed so large, that in attempting to give anything like a complete and logical account of it, I am bewildered with its richness, and the multiplicity of details, all so important, and so suggestive, that my task of selection would be one of great difficulty, were I not guided by a principle which this Society I know will appreciate, namely, that of insisting on its technological, rather than on its scientific bearings. The botanist, the chemist, the geologist, the palæontologist, have abundant information to give us respecting the various kinds of coal, bitumen, petroleum, &c. My business is to gather together a few details chiefly respecting one substance that has been associated with coal and other carbonaceous bodies, and to point out some of its applications to the arts of life.

"Paraffine, however, is so mixed up with other compounds of carbon and hydrogen that it will not be easy to disentangle it without some reference to them. I must, however, be permitted once more to remind the Society that my object is industrial, not chemical. Some of the most illustrious chemists of this and of other countries have been, and are devoting their high powers to the solution of questions of great importance and still unsettled, respecting the products of the different varieties of coals and bitumens, so that it would be presumptuous in me, a simple technologist, to pretend to do more than invite the attention of the Society to a few practical applications of the labours of those distinguished men.

"In common with all other branches of industry, the distillation of coal has been of slow growth. Sources of inflammable gas had long been known to exist in the coal districts of this country before the year 1659, when Mr. Shirley communicated to the Royal Society of London a notice of some experiments on an inflammable gas issuing from a well near Wigan, in Lancashire. Some years later Dr. Clayton made some direct experiments on the distillates of coal, but they were not communicated to the Royal Society, or indeed made public, until the year 1739. He says:—'First there came over a flegm, then a black oil, and then likewise a spirit arose, which I could in no wise condense.' This spirit (as the inflammable non-condensible gas was called) excited so much curiosity that the black oil was disregarded. Yet Dr. Hales, in 1726, had published a notice of some experiments on the distillation of coal, and had mentioned the condensation of a volatile oil in the vessel attached to his still. Dr. Watson also noticed the production of oils, when coal is heated to redness in close vessels.

"This scanty information expressed all that was known for many years on the subject of the liquid products of the distillation of coal.

"In 1781, Lord Dundonald obtained a patent for 'a method of extracting or making tar, pitch, essential oils, volatile alkali, mineral acids, salts, and cinders from pit-coal'—'which coals, being kindled, are enabled by their own heat, and without the assistance of any other fire, to throw off in distillation or vapour, the tar, oil, &c., they contain into receivers or condensing vessels,'—'the art depending on the management of the air admitted into the kilns, which can only be acquired by experience.'

"In 1797, a writer in the *Encyclopædia Britannica* refers to Lord Dundonald's process with applause, 'He turns to a very considerable profit the mines of coals in his and other estates, building ovens of a proper construction for burning pit-coal into coke, and at the same time collecting in separate receptacles the volatile alkali, oil, tar, and pitch, which are generally lost by the usual method.' The writer goes on to say, that this method, compared with the usual method of coking coal, 'affords a very remarkable instance of the great losses to mankind, for want of carefully attending to every result from great processes of art when made on a large scale. These ovens are so contrived as to admit an under supply of air, and the coals, after being kindled, decompose themselves by a slow but incomplete combustion, which does not destroy the ingredients. The residuum left in the oven proves to be most excellent cinders or coaks.'

"It appears that about this time, M. Faujas de St. Fond introduced into France a similar process, from information which he had obtained at Lord Dundonald's works, forgetting, however, as Frenchmen sometimes do, to acknowledge the source of his information.

"The article above referred to states that, 'on subjecting pit-coal of any kind to distillation in close vessels, it first yields phlegm or watery liquor, then an ethereal or volatile oil, afterwards a volatile alkali, and lastly a thick and greasy oil; but it is remarkable, that by rectifying this last oil, a transparent, thin, and light oil of a straw colour is produced, which being exposed to the air, becomes black like animal oils.'

"Mr. Northern, of Leeds, in the *Monthly Magazine* for April, 1805, directed attention to the use of coal gas. He says, 'I distilled in a retort 50 oz. of picked coal in a red heat, which gave 6 oz. of a liquid matter covered with oil, more or less fluid, as the heat was increased or diminished; about 26 oz. of cinder remained in the retort; the rest came over in the form of air. . . . In the receiver I found a fluid of an acid taste, with a great quantity of oil, and at the bottom a substance resembling tar.' In this, and in Lord Dundonald's previous process, we have the germ or basis of the manufacture of volatile oils from coal. The germ, however, was destined to remain nearly half a century without expanding, and what at last gave it vitality was the demand for rapid solvents of caoutchouc, which led to the distillation of tar, as a source of benzole, when other valuable luminiferous agents were found to be present, or separable from the tar.

"The subject, however, had not altogether remained neglected. Substances were obtained from different sources, and by different inquirers, in various parts of the world, and at length becoming assembled and made known to each other, certain relationships were recognised where least expected. In 1812, Mr. Le Witte patented an apparatus for extracting tar from coal, for the manufacture of varnishes, and for protecting surfaces. It is stated that 10 per cent. of tar was thus obtained, while the residual coke was a valuable marketable product. In 1819, Theodore de Saussure obtained from the asphaltic limestone, near Neufchatel, in Switzerland, an oil which he found to be identical with that from the petroleum of Amiano. In 1824, Chervau took out a patent for a method of distilling bitumen from the rocks in the Department of Sâone-et-Loire. The manufacturers stated that they extracted 40 parts of volatile oils from 100 parts of rock, and they recommended their bitumen or naphtha as a fuel for alcohol lamps. The year 1829 introduces us to Reichenbach, the proprietor of chemical works in Moravia, whose name occupies a high place in connection with our subject. In that year, while examining the tar obtained from the dry distillation of beech-wood, he discovered *paraffine*, a wax-like substance, which earned its name from its want of affinity for other bodies (*parum affinis*, or 'little affinity') for it would neither unite with acids nor alkalies; it was insoluble in water and alcohol, and had no action on metals. An oil, which possesses some of these properties, has received the name of paraffine oil, and has been extensively used as a lubricator. Reichenbach, also, in pursuing this inquiry recognised paraffine in the tarry matters distilled from other species of wood, and also in the tars arising from the distillations of the bitumens, and, at a later period, in the tar obtained from coal. He also obtained from the tar of wood naphthalin and creosote, and also bodies named piccamar and pittacal, which have been but imperfectly examined. In 1833 and 1834, he distilled coal in close vessels in contact with water, but from 220 lbs. of coal he obtained only 9 ounces of volatile oil. In 1833, Dr. Bley obtained a small quantity of volatile oil, and some ammoniacal products, by distilling brown coal. These failures, as they must be called, so far as respects coal, arose from misapprehension respecting the required temperature, the manipulation of the gas-house naturally suggesting a high degree of heat, and inquirers did not recur to the earlier methods of trying the effects of a comparatively low one. As early, however, as 1841, improved forms of apparatus for distilling coal were introduced by the Count de Hompesch and others, and the problem how to obtain liquid products instead of permanent gases began to assume a clearer form.

"Hompesch's patent is 'for improvements in obtaining oils and other products from bituminous matters, and in purifying and rectifying oils obtained from such matters.' These are 'bituminous schists, shales, or slates, or other rocks or minerals containing bitumen or bituminous substances.' He describes the oil of schist or clay slate as 'essential oil, intermediary fat oil, and thick oil.' His method of distillation is in a long iron close retort, arched on the top, and flat at the bottom, and part of his apparatus is an Archimedean screw, for filling the retort and turning out the coke at the other end.

"There were three tubes leading from the retort, one at the part farthest from the furnace, another in the middle, and another at the end nearest the furnace, and Hompesch says that he obtains the three separate oils by the gradual increase of the heat, thus effecting distillation without decomposition of the substance. His mode of rectifying the fat oil, is by distilling it with sulphuric acid, and treating it with caustic potash lees.

"In 1845, M. Du Buisson patented a process for 'new and improved methods for the distillation of bituminous schistus, and other bituminous substances, as well as for the purification, rectification, and preparation necessary for the employment of the productions obtained by such distillation for various useful purposes.' His works were at Autun, in France, and one feature in his process is the employment of steam. He applies a low red-heat, and 'when the oil begins to be disengaged, steam is introduced through the cock into the cast-iron pipes which are now heated to redness.' His object in introducing the steam, is to abridge the process of distillation. 'The effect,' he says, 'of introducing steam at a high temperature, is to shorten the time necessary for distillation, six hours for every sixteen, which is a very great advantage, and nearly double the quantity of oil is produced in a given time.' By the introduction of steam also, 'the retort does not require to be heated to so high a temperature, which prevents decomposition of the oils, and saves wear and tear of the apparatus.' The practical effect of the steam was thus to carry off the vapours of the schist as fast as they were formed, and to prevent the retort from being raised above red-heat by the external application of the fire. The products of the distillation were a liquid bitumen, named raw oil of schistus, ammoniacal water, and a carbonaceous residuum. From this raw oil of schistus are obtained two volatile oils, the first of which may be used in lamps with the greatest advantage with a reservoir below, and with a double current of air in which the oil rises a distance of about five inches by the capillary attraction of the wick. The light is described as being 'superior to that of gas, without any unpleasant smell or smoke; the said lamps have a glass or chimney, with a diaphragm of the same diameter as the wick, and placed a little above the top of the burner.' Another oil is also described, capable of being 'burnt in the said lamps without any alteration, by mixing with it a corresponding oil obtained from petroleum and other bituminous mineral substances.' In the third place, he gets a fatty mineral oil, which he says, 'contains paraffine in large quantity. It is particularly applicable for lubricating, for machinery;' and, fourthly, he obtains paraffine 'by crystallisation from fat and thick oils; it is thus obtained very pure, and requires but little treatment to make excellent candles.' He also gets a black and siccative tar. His method of purifying the raw oil is by distillation with sulphuric acid, drawing off the supernatant oil, neutralising the sulphuric acid by means of a caustic solution of soda, and by alcohol, the latter carrying off a pure red colouring matter. He also distills again, and collects the different oils of different densities into separate receivers. He has also a method of depriving the first refined product of its bad odour by means of sulphuric acid, caustic soda, hydrate of protoxide of iron, sulphate of iron, and caustic solution of soda, and he cautions against putting water upon the oil before the caustic solution of soda, since the effect would be to clog the wick and to make the paraffine oil give out a bad smell while burning. With respect to the paraffine, he states that 'it may be separated from the oil merely by pressure, in the same manner that stearine is separated from oleine.' The paraffine is then melted and filtered on a small quantity of animal black in a double funnel, heated by steam or boiling water, after which it is fit for making candles.

(To be continued.)

NOTICES OF PATENTS.

Fixing Pigments and other Colouring Matters on Textile Fabrics and Yarns. J. Lightfoot, Accrington.

The patentee prints on with glue, and, after steaming, passes the goods through a salt of mercury or lead. He also claims glue as a thickener, and fixing agent for murexide colours. According to this patent, colours may be fixed by printing with glue and nitrate of lead, and, subsequently, passing through ammonia and a salt of mercury. Printing on with glue and salts of alkaline metals is also claimed. The colours derived from aniline, &c., may be fixed, according to the patentee, by means of tannin or tanno-gelatine.

Artificial Fuel. J. H. Johnson, Lincoln's Inn Fields, London.

The patentee takes coal-dust, peat, &c., and causes it to cohere by mixing it with glutinous paste or size. The mixture is then subjected to great pressure in moulds, and afterwards dried. It is evident that this method is, in every way, vastly inferior to the plan adopted on such an immense scale near Swansea, and in other places. We allude to mixing the coal-dust with gas-tar previous to forming it into blocks. The tar which remains in the blocks, when subjected to the high temperature of the drying ovens, yields a highly combustible fuel.

A Liquid, or Novel Preparation, to be Applied to Wools. W. E. Gedge, Wellington Street South, Strand.

This patent is for a new fluid for oiling woollen yarns. It is, in fact, a fluid soap made by treating colza, olive, or other suitable oils with ammonia. As soon as the combination between the oil and the volatile alkali is complete, water is added, to render the whole sufficiently fluid to enable the fibres of the wool to separate properly.

Manufacture of Hydrated Oxides and Salts of Iron and Salts of the Fixed Alkalies. T. Richardson, Newcastle-on-Tyne, and M. Prentice, Stowmarket.

The patentees treat the oxides of iron with sulphurous acid; they then expose the solution to the atmosphere, by which means they obtain hydrated oxides precipitated, and sulphate of iron in solution. By treatment of a solution of iron obtained as above, with a solution of chloride of sodium or potassium they obtain, by double decomposition, sulphate of the alkali and chloride of iron. The latter they convert into hydrated oxides by treatment with lime, magnesia, baryta, or the carbonates of those earths. They thus get a precipitate of the desired hydrate and chloride of calcium, &c. in solution.

Treating Organic and Other Substances Containing Phosphate of Lime. T. Richardson, Newcastle-on-Tyne.

The patentee obtains soluble phosphates by treating phosphate of lime with sulphurous acid. The patent includes mixing porous substances with guano previous to decomposing by means of acids.

Substitute for Butter. B. Harben and R. J. Hathaway, Leadenhall Street, London.

The patentees prepare a delicious compound as a substitute for butter, by mixing eight parts of fat, four of butter, and one of oil! The fat being first divested of strings or ligatures, is mixed with the butter and oil in a cold state, and, if desirable, salt may be added. Provisional protection only. We submit that this is an attempt at patenting a process for a disgusting adulteration of butter. The reason it was not proceeded with was, probably, that the patentees became aware of the extreme want of originality in the very nasty idea for which they obtained protection. Most cheesemongers of middling honesty have been perfectly and practically familiar for many years with a mode of manufacturing "best fresh" from "stale salt" by a process not very different from the above.

Tanning. W. Clark, Chancery Lane, London.
The raw hides, prepared as usual, are deprived of hair by lime, and then cleaned. The skins are then steeped in a solution of an alkaline chromate for a period varying from six to twelve hours according to their thickness. They are afterwards washed in running water until quite clean. The patentee states that, by undergoing this process, the greasy matters of the skins undergo a transformation which enables the hides to absorb the tanning ingredients with greater readiness.

CORRESPONDENCE.

Silvering Glass.

To the Editor of the Chemical News.

Sir,—Having been for a considerable length of time engaged in a series of experiments for the purpose of depositing a thin layer of silver on the polished surface of a glass speculum, which I have prepared, without success, I should feel extremely obliged if any of your correspondents would direct me how to accomplish that object, or inform me where I could obtain the requisite information.

I have tried Drayton's method of silvering mirrors, viz.:—

$$\left.\begin{array}{lll} NO_5\,AgO & \ldots & \text{Two parts} \\ HO & \ldots & \text{Three parts} \\ NH_4\,O & \ldots & \text{One part} \\ C_4H_6O_2 & \ldots & \text{Three parts} \end{array}\right\}\text{By weight,}$$

reduced by grape-sugar, or oil of cloves; but the result was always a mottled surface of black matter and silver.

Another method was to dissolve gun-cotton in caustic potass, and drop into the solution crystallised nitrate of silver; redissolve by means of ammonia, and heat the mixture while on the surface of the glass. This produced a whiter metal, but the black residue spoiled the surface as before.

I have also tried a number of experiments with the silver salt dissolved in saccharic acid, glycerine, &c., but always without a satisfactory result.—I am, &c.

J. J. W.

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—I will once more trouble you with a few lines in reply to the more prominent points of "Verax's" letter on the above subject, published in No. 68 of the Chemical News, promising, however, that it is the last time I can accord any notice to critical communications where a mere *nom de plume* appears in place of a genuine signature; for, as another of your correspondents very truly wrote,—"truthful statements need no fictitious names."

I differ with "Verax" entirely as to the value of anonymous communications, for the general reader, at all events, will always look to the signature appended to any assertions, before he decides upon what amount of credence may be attached to the same. I cannot, either, permit another dictional point to pass without comment. "Verax" constantly associates himself and his opinions with those of the public at large, instead of bringing them forward in a simple, straightforward manner for whatever they may be worth as his own well-founded convictions. It is not a little remarkable that of the large number of communications upon the adulteration question I have received within the last two months, not one has been in accordance with the notions of your correspondent.

If "Verax" chooses to pervert very obvious and common-sense statements to his own mysterious purposes, I will not be answerable for any deductions from my writings thus peculiarly contorted; as the fairest way of replying to his remarks on colouring matters, I will quote from my own paper (Chemical News, No. 62, page 94):—

"In the second table accompanying this paper, I have condensed some general information relating to the chief colouring matters used for purposes of (what might be called) ornamental adulteration, amounting to upwards of sixty in number, of which twelve or thirteen are active and powerful poisons—a like number are harmless, except in special cases of disease or morbid debility,—while the remainder are either more or less injurious to health, or we know too little about their effects upon the animal economy to venture a decided opinion either way."

I think it hardly possible to imagine a more definite or explicit statement than the preceding; and I cannot conceive what observation of mine can be considered to imply that I "regard all colours and essences employed to please the eye or taste, as poisonous."

With regard to the adulteration of bread, I may inform "Verax" that, as might have been supposed, my results are not founded upon the examination of samples from any one district, or class of vendor, or consisting of any particular quality; had it been otherwise, those results would never have been published by myself.

I have seldom heard a more useless query than that of "Verax," who requires to know the "average amount" of alum "found in a quartern?" and I will candidly own my utter inability to reply to the interrogation. As well might one inquire what is the average force or direction of the wind on the entire surface of the globe all the year round! Where the extremes vary so greatly, an arithmetical mean might of course be arrived at; but would such be of the smallest practical account?

I have found as little as nineteen grains of alum to the "quartern loaf"—*i.e.*, *real* "quartern" loaf, weighing 4lbs., not the *theoretical* loaf, which is constantly several ounces short—and as much as 182 grains; and, of course, between these terminal points, every imaginable proportion. Perhaps, as a *very rough average* for the district of Bayswater, in the winter of 1859-60, I may give 72 grains of alum to 4lbs. of bread as a general proportion, and for St. Giles's 108 grains to the quartern.

If "Verax" ever possesses any samples of "bread made from pure wheat," characterized either by "brittleness of structure," or "crumbly texture," or any other bread samples without either of these properties, but containing respectively rice and potatoes in any quantity, perhaps he will be good enough to send me portions of the same; for, as a humble worker in the cause of science and truth, I am pleased to have the opportunity of investigating anything unusual.

I have throughout taken the word "adulteration" in its popular sense rather than in its more restricted meaning, as, for general purposes, I consider the former plan the most convenient, and one appealing best to the public mind.

At a future day the objections of "Verax," and others of a more important bearing, will probably be answered at length; now, however, I am content to reserve the discussion for a time, as I think a lengthened discourse on the special points brought forward would be wholly uninteresting to your readers. In reply to various other questions of "Verax," I must simply refer him to my paper and the tables relating thereto, in the hope that he will peruse the same a little more carefully than he seems to have done hitherto.

Analytical points I must altogether decline to discuss with unknown critics, in accordance with my usual custom. In the present instance I retain the views formerly expressed by me.

With regard to the last point I deem it necessary to notice; I may inform "Verax" that I referred to various manufacturers, importers, and others, including among them Messrs. Huntley and Palmer, Messrs. Phillips, Messrs. Crosse and Blackwell, &c., in my paper, for the reasons given therein, as I have examined the food products vended by these and various other firms in many ways for a long time past, and entirely without their knowledge. Only within the last few months have I had any direct communication with the parties in question for the purpose of acquiring certain information of a technical or statistical kind. Still, whatever assertion relating to his own works I may accept from a manufacturer—if borne out by my own researches—I should never dream of believing him if he told me that certain adulterations I had frequently detected were never used in his trade and city. Why, it is not so long ago that "one of the largest bakers" in London assured me that alum was "seldom, if ever, used now," while he was using it himself in the proportion of about 35 grains to the "quartern"!

The quotations of Dr. Odling's 64 samples from one district, which were said to contain no alum, must be taken for whatever they may happen to be worth. As something *per contra*, I may refer to Dr. Normandy's speech at the Society of Arts,[1] when he observed that he had found alum in every one of a series of 500 samples of bread he had examined not very long ago.

Thus much in support of the views "Verax" "conceives to be mistaken" ones, but which I hold are those of the great bulk of the public at large, however crudely and imperfectly they have been enunciated by me.

I am, &c. WENTWORTH E. SCOTT.
Bayswater, London, W.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Some Compounds of Molybdic Acid.—Gentele describes several of these (*Journal für pract. Chemie*, Bd. lxxxi. s. 411). *Molybdate of Soda and Alumina*, $Al_2O_3 + 3NaO + 12MoO_3 + 22HO$, is formed when a solution of potash-alum is precipitated by an excess of molybdate of soda, and the precipitate is dissolved in hydrochloric acid. This double salt crystallises in needles, which, when dried over sulphuric acid, lose 7·9 per cent. of water, and when heated to redness, lose 16·5. It contains alumina 4·25, molybdic acid 71·63, and soda 7·51 in a hundred.

Molybdate of Alumina $5Al_2O_3 + 2MoO_3 + 33HO$ is precipitated when molybdate of soda is dropped into a solution of alum. When dried in the air it forms a white horny mass, which contains 3·42 of water. The ignited precipitate is composed of Al_2O_3 65·24 and MoO_3 34·76. After ignition the precipitate is only partially soluble in hydrochloric acid, and from a solution of the unignited ammonia only precipitates one-third of the alumina. When the unignited precipitate is dissolved in hydrochloric acid, and an excess of ammonia is added, the precipitate which is produced slowly dissolves again, which makes it probable that a soluble basic molybdate of alumina and ammonia exists.

Molybdate of Iron.—An excess of neutral molybdate of ammonia dropped into a solution of sulphate of iron, or potash, or ammonia-iron-alum, produces at first a cloudiness; but a precipitate falls on standing, and continues to increase for some time. It is of a pale lemon yellow colour, which on ignition turns green, and again yellow. It fuses at a great heat, molybdic acid escaping, and a greenish glass remaining behind. The precipitates formed with the above iron compounds contained from 14·7 to 21·01 of iron oxide, and gave approximately the formulæ $Fe_2O_3\,4MoO_3$; $Fe_2O_3\,5MoO_3$; $Fe_2O_3\,7MoO_3$. When a solution of either of the above-mentioned iron compounds is dropped into the solution of molybdate of ammonia, the latter remaining in excess, a thick gelatinous precipitate is formed. The solutions must be very dilute, or a stiff jelly will be formed, which it will be difficult to wash free from the molybdate of ammonia. The moist jelly shrinks so much in drying, that a quart when dried weighs scarcely 500 milligrammes, and then forms a yellow, horny, half-transparent mass. The analysis of one specimen gave 26·32, and another 25·37 per cent. of oxide, and 73·60, and 74·61 of molybdic acid.

Molybdate of Chrome Oxide.—A solution of molybdate of soda added by degrees to potash-chrome-alum produces at first no change; but, after a time, a cloudiness appears, which goes again on shaking. It is only after adding a quantity of the molybdate that the cloudiness is permanent and a precipitate falls. If all the chrome-oxide is not thrown down from the solution, the new compound, when dried, is of a clear malachite green colour. It has the composition $Cr_2O_3 + 3MoO_3 + 7HO$. When an excess of the molybdate of soda is added, the precipitate is of a darker colour, and has the composition $Cr_2O_3 + 2MoO_3 + 8HO$. Another compound $3Cr_2O_3MoO_3 + 4HO$ is produced when a large excess of molybdate of soda and ammonia are added to the chrome alum at the same time.

Molybdate of Copper is precipitated when neutral molybdate of soda is added in excess to sulphate of copper. The precipitate is of a very pale green colour, and at first appears flocculent, but after washing it has a crystalline appearance. It has the composition $4CuO, 3MoO_3 + 4HO$. The author also describes some ammoniacal molybdates of copper.

On Benzile.—Zinin has before shown (see *Gmelin's Hand-book*, Vol. xii., pp. 174—184), that when benzoïn is heated with chlorine or nitric acid, it loses hydrogen, and is converted into benzile. He now shows (*Bulletin de St. Petersb.* Tome iii. p. 68) how the benzile may be reconverted into benzoïn without producing any by-products. Benzile is tolerably soluble in strong acetic acid, and the change is effected in this solution by means of metallic iron. The author dissolves one part of benzile in six parts acetic acid, sp. gr. 1·065, adds from one to two parts of iron, and boils until the fluid becomes a mass of slender white needles of benzoïn, he then pours the pappy mass from the deposit of iron, after cooling, washes with water, and then re-crystallises from alcohol. In this way he obtains perfectly pure benzoïn. It can also be obtained by acting on an alcoholic solution of benzile with zinc, and hydrochloric acid. On treating benzile with pentachloride of phosphorus, the author found that no hydrogen was displaced; but a new body which he calls chloro-benzyl, was formed $C_{28}H_{10}O_4 + P\,Cl_5 = C_{28}H_{10}O_2Cl_2 + P\,Cl_3O_2$.

Chloro-Benzyl (we adopt in this instance the author's orthography of the final syllable, to distinguish this body from Cahour's *chloro-benzile*) is very soluble in ether, and crystallises from the etherial solution, when evaporated at the ordinary temperature in large, colourless, transparent crystals two or three lines in thickness, but usually is obtained in short four or six-sided prisms. It is also soluble in alcohol. It melts under water or alcohol at 71°, and remains fluid after it has sunk to the ordinary temperature. It is decomposed by distillation, yielding a fluid, which among other things, contains some unaltered chloro-benzyl. The elementary analysis of chloro-benzyl led to the formula $C_{28}H_{10}O_2Cl_2$. Chloro-benzyl dissolves in strong nitric acid with the evolution of red vapours, and when the solution is mixed with water, pure benzile is precipitated. The reaction which takes place is explained by the following equation:—$C_{28}H_{10}O_2Cl_2 + HNO_6 = C_{28}H_{10}O_4 + NHOO_4\,Cl_2$. The action of caustic alkali on chloro-benzyl is remarkable. If hot alcoholic solutions of both bodies are mixed together and allowed to cool, benzoate of potash crystallises out; but if the solutions are mixed with water, an oily body separates, which has all the properties of bitter almond oil. Aqueous ammonia has no action on chloro-benzyl; the reactions of alcoholic ammonia are very complex, and offer considerable difficulties.

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[1] Not reported quite correctly in the Journal of the Society and in these pages.

III. ANALYTICAL CHEMISTRY.

Test for Grape Sugar.—Mulder gives the following (*Scheik. Onderz.* Deel iii. p. 25) as the best way of using his test for the discovery of grape sugar:—A little of the sulphate of indigo solution is placed in a test tube, the fluid to be tested is added, and the mixture is boiled. A solution of carbonate of potash or soda is then added until the liquid is alkaline. If grape sugar is present the mixture is decolorised, if not it remains blue.

MISCELLANEOUS.

Oxide of Chromium, when reduced to fine powder is one of the best reducing and polishing substances for metals known, and which we think is even superior to the finest emery for polishing steel. The best iridium-pointed gold pens become useless when used for writing signatures for a few hours over the green chrome ink that is printed on bank bills.—*Mining Chronicle.*

International Exhibition of 1862.—The following is the classification intended to be adopted by her Majesty's Commissioners for the Great Exhibition of 1862:—*Section* I. *Raw Materials.*—Mining, Quarrying, Metallurgy, and Mineral Products; Chemical Substances and Products, and Pharmaceutical Processes; Substances used for Food, including Wines; Animal and Vegetable Substances used in Manufactures. *Section* II. *Machinery and Engineering.*—Railway plant, including Locomotive Engines and Carriages; Carriages not connected with rail or tram-roads, Manufacturing Machines and Tools; Machinery in general, as applied to industry; Agricultural and Horticultural Machines and Implements, Civil Engineering, Architectural and Building Contrivances; Military Engineering, Armour, and Accoutrements, Ordnance and Small Arms; Naval Architecture and Ships' Tackle; Philosophical Instruments, and Processes depending on their use; Photography, and Photographic Apparatus; Horological Instruments; Musical Instruments; Surgical Instruments and Appliances. *Section* III. *Manufactures.*—Cotton; Flax and Hemp; Silk and Velvet; Woollen and Worsted, including Mixed Fabrics generally, Carpets; Woven, Spun, Felted, and Lace Fabrics, when shown as specimens of Printing or Dyeing; Tapestry, Lace, and Embroidery; Skins, Furs, Feathers, and Hair; Leather, including Saddlery and Harness; Articles of Clothing; Paper, Stationery, Printing, and Bookbinding; Educational Works and Appliances; Furniture and Upholstery, including Paper-hangings and Papier Maché; Iron and General Hardware, Steel, and Cutlery; Works in Precious Metals and their imitations; Jewellery; Glass; Pottery; Manufactures not included in previous Classes. *Section* IV. *Fine Arts (Modern).*—Architecture; Paintings in Oil and Water Colours and Drawings; Sculpture, Models, Die-sinking, and Intaglios; Etchings and Engravings. In the Exhibition of 1851 there were only 35 Classes. In the proposed one, therefore, there are five additional.

New Colour—Indian Red.—Painters use very few colours of an organic nature. Those which they employ are usully in the state of lake, which are a combination of the colouring principle, with a metallic oxide. For some time past I have been occupied with chemical researches on the colouring principles of organic origin, and have obtained one from sandal wood, which, by its beauty and its brightness, is nearly equal to carmine, and is of great interest to painters. This principle is a pure red solid, melts a little below 212°, and is afterward decomposed. It is insoluble in water and fixed oils, but very soluble in alcohol, ether, acetic acid and volatile oils. Dry chlorine has no action on it; wet chlorine destroys it. All acids—except nitric, chromic, and others which are rich in oxygen—have no action on it. Hydrosulphuric acid, which of all the gases is the most redoubtable test for such colours, has no action upon it; neither have light and air. Painters have found it to be a very solid colour. It was used to paint the carriages of the Emperor Napoleon about nine years ago, and the color is still as bright as when it was put on. Its preparation is easy, as follows:—Take the sandal wood in powder, and exhaust it completely by alcohol. In the alcoholic solution thus obtained, pour hydrated oxide of lead in excess. Collect the precipitate on a filter. Now, wash the precipitate with alcohol and dry it; dissolve it in acetic acid, and to this acetic solution add an excess of water. The colouring matter which is insoluble in water is precipitated, and the acetate of lead stays in the solution; and it may be used to make new oxide of lead. Now, wash the precipitate well and dry it at a low temperature. Researches on this colour—too long to relate here—have shown me that this colour is a peculiar principle, and is pure santalin. Its cost will be about 4s. a pound. I intend to make a new compound for dyers and calico-printers, extracted from santalin, capable of dissolving in water—a thing which has never been done before.—*Professor H. Dussauce.*

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the Chemical News, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

*** All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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J. Lyn.—We regret that we cannot assist you.

F.G.S.—1. We believe not. 2. The report was published and may be obtained on application to a bookseller. 3. Not to our knowledge.

A.B.C.—The value of commercial may be estimated near enough for practical purposes by distilling a known quantity, and collecting apart all that comes over below 212° F. To estimate it with accuracy it would have to be repeatedly submitted to fractional distillation, which is a most tedious operation.

J.M.W.—Snowdon diamonds are transparent crystals of quartz. The cubical crystals you speak of as occurring in the trap rocks are most likely iron pyrites; if so, they will be of a golden colour, and very brittle.

Manufacture of Varnish.—A correspondent asks if Gum Copal or Gum Anime is first melted and run into water according to Mr. P. F. Tingrey's method, and then again melted and mixed with boiling Linseed Oil to make a copal varnish? Or can the Gum Copal and Gum Anime be melted in a gum-pot, and mixed direct with boiling Linseed Oil, according to Mr. J. W. Neil, who obtained the Gold Isis Medal of the Society of Arts in 1832? And is there a flux that can be used with the gum resins so as to render those gums more easy to mix with Linseed?

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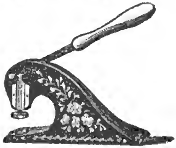

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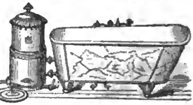

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AND

Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

No. 71.] Saturday, April 13, 1861. {PRICE 4*d*. POST FREE 5*d*.

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THE CHEMICAL NEWS.

Vol. III. No. 71.—April 13, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Substitution of Electro-Negative Bodies (Chlorine, Bromine, Iodine, Cyanogen, Sulphur, &c.) for the Metals in Oxygenised Salts: Production of a New Class of Salts in which the Electro-Negative Bodies replace the Basic Hydrogen, by *M.* P. Schutzenberger.

The idea struck me that by making certain compounds, such as the protochlorides of iodine, bromine, or sulphur, iodide of cyanogen, &c., react on oxygenised salts, I should succeed in substituting the radical bromine, iodine, sulphur, or cyanogen (which, in these compounds, is electro-positive) for the metal of the salt by simple double decomposition, which may be represented by easily generalised special equations:—

$$\underbrace{C_4H_3NaO_4}_{\text{Acetate of soda.}} + ClI = ClNa + \underbrace{C_4H_3IO_4}_{\text{Acetate of iodine.}}$$

$$\underbrace{C_4H_3AgO_4}_{\text{Acetate of silver.}} + ICy = IAg + \underbrace{C_4H_3CyO_4}_{\text{Acetate of cyanogen.}}$$

In these analogous products, the first of which is isomeric with iodacetic acid, the iodine, instead of being substituted for the equivalent of hydrogen of the radical acetyle takes the place of the basic hydrogen.

It is easy to foresee that bodies of this class, if they can exist, should be characterised by quite special properties, and, above all, great instability. Experiment has not failed to verify my surmises, and if I have not yet had time to give to my reactions the desirable degree of generality, I have carefully studied them on a number of compounds sufficient to enable me to predict with certainty that it is possible to prepare the oxygenised salts of chlorine, bromine, iodine, cyanogen, &c., in the same manner as salts of potassium, lead, mercury, &c. are obtained.

Here is a condensation of the facts on which I support these general conclusions,—facts described in detail in the account sent to the Academy:—

Anhydrous acetic and hypochlorous acids united at a very low temperature, in the proportions of $C_4H_3O_3$ to ClO, mix together and turn red. In a quarter of an hour the mixture decolorises spontaneously without any other apparent reaction, and without altering in weight. An excess of hypochlorous acid imparts a fixed red tint to it, which disappears when this excess is removed by gentle heat (30°).

The analysis of this liquid gives numbers corresponding to the formula,

$$C_4H_3ClO_4, \text{ or } C_4H_3O_3ClO,$$

which is that of monochloracetic acid; but the characteristics of these products do not allow of its being confounded with the latter. By its mode of formation and properties, I consider it an acetate of chlorine.

Acetate of chlorine is a liquid, colourless, or of a faint, pale yellow. It dissolves immediately, and in any proportion, in water, and turns into a mixture of hydrated acetic and hypochlorous acids,

$$C_4H_3ClO_4 + 2HO = ClO.HO + C_4H_3O_3\,HO.$$

Towards 100° C. it detonates violently, and yields chlorine, oxygen, and anhydrous acetic acid,

$$C_4H_3ClO_4 = O + Cl + C_4H_3O_3.$$

It may be preserved in ice and darkness without undergoing any alteration; but at the ordinary temperature and in the sun it decomposes, little by little, and the bottle-stoppers are forced out violently. It can be distilled *in vacuo* at a low temperature.

Mercury attacks it when cold with a rustling noise. It disengages much chlorine, and forms acetate of mercury and a little calomel,—

$$C_4H_3ClO_4 + Hg = Cl + C_4H_3HgO_4.$$

Here we have the singular phenomenon of the displacement of chlorine by a metal.

Zinc attacks it very slowly, and produces a mixture of acetate and chloride of zinc,—

$$C_4H_3ClO_4 + 2Zn = C_4H_3ZnO_4 + ClZn.$$

Iodine instantly dissolves in it, decolorising itself and disengaging chlorine, without producing chloride of iodine. It forms solid, white, crystalline acetate of iodine, isomeric with iodacetic acid, but the characteristics of which are entirely different to those of the latter acid,—

$$C_4H_3ClO_4 + I = C_4H_3IO_4 + Cl.$$

Acetate of iodine decomposes by heat above 100° C., almost always with an explosion, according to the equation,—

$$2(C_4H_3IO_4) = I_2 + C_2O_4 + \underbrace{C_4H_3(C_2H_3)O_4}_{\text{Acetate of methyle.}}$$

Water destroys it immediately, according to the equation,—

$$10(C_4H_3IO_4) + 10HO = I_8 + 2IO_5 + 10C_4H_4O_4.$$

Absolute alcohol also decomposes it when cold, according to the equation,—

$$10(C_4H_3IO_4) + 5\left\{\begin{matrix} C_4H_5O \\ HO \end{matrix}\right\} = I_8 + 2IO_5 + 5C_4H_4O_4 + \underbrace{5[C_4H_3(C_4H_5)O_4]}_{\text{Acetate of ethyle.}}$$

which corresponds to the preceding one.

In mixing equal equivalents of protochloride of iodine and dry acetate of soda, the mass becomes slightly heated, the strong odour of the chloride of iodine almost entirely disappears, and a mixture of marine salt and acetate of iodine is obtained. This mixture, from which it is impossible to separate the acetate of iodine, presents, in fact, under the influence of heat, water, and absolute alcohol, all the reactions formulated above. If there is an excess of chloride of iodine, a secondary reaction takes place, represented by

$$C_4H_3NaO_4 + 2ClI = I_2 + ClNa + C_2O_4 + \underbrace{C_2H_3Cl.}_{\text{Chloride of methyle.}}$$

The action of chloride of iodine on butyrate of soda is exactly parallel, and can be explained by a similar equation,—

$$C_8H_7NaO_4 + ClI = C_8H_7IO_4 + ClNa,$$

$$C_8H_7NaO_4 + 2ClI = I_2 + C_2O_4 + ClNa + \underbrace{C_6H_7Cl,}_{\text{Chloride of trytile.}}$$

$$2(C_8H_7IO_4)\text{ heated} = C_2O_4 + I_2 + \underbrace{C_8H_7(C_6H_7)O_4,}_{\text{Butyrate of trytile.}}$$

$$10(C_8H_7IO_4) + 5\left\{\begin{matrix}C\,H_5O\\HO\end{matrix}\right\} = I_8 + 2IO_5 + 5C_8H_8O_4 + \underbrace{5[C_8H_7(C_4H_5)O_4]}_{\text{Butyrate of ethyle.}}$$

When cold, bromine dissolves in acetate of chlorine, the chlorine being disengaged little by little; the mixture, at first red, becomes at last perfectly colourless,—

$$C_4H_3ClO_4 + Br = Cl + C_4H_3BrO_4.$$

The product thus obtained is liquid, and, after some time (one or two hours), decomposes spontaneously, with an explosion. Iodine dissolves in it, freeing the bromine and forming solid acetate of iodine.

Sulphur dissolves with a rustling noise in acetate of chlorine, disengaging chlorine, but it is separated from sulphurous acid, and anhydrous acetic acid and sulphur remain,—

$$2C_4H_3Cl_2O_4 + S_2 = 2C_4H_3O_3 + SO_2 + S + Cl_2.$$

According to this, acetate of sulphur appears less stable than the other negative acetates; in fact, by the action of chloride of sulphur (ClS) on dry acetate of soda, we obtain immediately, by cooling, the following reaction :—

$$2(C_4H_3NaO_4) + 2ClS = 2C_4H_3O_3 + SO_2 + S + 2ClNa,$$

which, being so pure, could be utilised with advantage for the preparation of anhydrous acetic acid.

Benzoate of soda and chloride of iodine heat slightly when mixed; the odour of the chloride of iodine disappears. If heated further it disengages much carbonic acid; iodine distills, and a liquid insoluble in water and potash, which can be separated by fractional distillation.

1. Into a colourless iodised liquid, boiling at 200° C., the composition of which, according to the analysis already made, appears to be that of iodide of phenyle ($C_{12}H_5I$).

2. Into a solid body very like naphthaline. According to this the first production of benzoate of iodine takes place probably according to the equation,—

$$C_{14}H_5NaO_4 + ClI = ClNa + C_{14}H_5IO_4,$$

and it remains for me to study more closely its products of decomposition. I shall endeavour equally to produce benzoate of chlorine, and with this pure benzoate of iodine.

By slightly heating a mixture of equal equivalents of acetate of silver and iodide of cyanogen, the mass melts, producing iodide of silver, disengaging nothing; but, by raising the temperature, the product decomposes, often with an explosion. These facts, of which a more detailed account will be given in another paper, are easily intelligible, by admitting the production of acetate of cyanogen,—

$$C_4H_3AgO_4 + ICy = IAg + C_4H_3CyO_4,$$

which would be decomposed at a higher temperature.

Anhydrous sulphuric acid absorbs anhydrous hypochlorous acid, yielding a stable, dark-red liquid; the excess of anhydrous acid can be separated from it by distillation without its disengaging the faintest trace of chlorine; but at a higher temperature sulphate of chlorine decomposes energetically.

These facts, I think, suffice to support my ideas, but I own that my task is far from being accomplished. The complete Memoir contains, moreover, some new researches on protochloride of iodine, which, for want of space, it is impossible to reproduce here.—*Comptes-Rendus*, vol. lii.

Observations on the Presence of Certain Elements, ordinarily very rare, in Common Substances, by M. H. SAINTE-CLAIRE DEVILLE.

I HAVE previously shown that vanadium is found in those aluminious matters so abundant in nature, and the researches of M. Beauvallet and M. Terreil have proved that the commonest clay contains this hitherto rare metal. The method I have employed for the demonstration of this unexpected fact has enabled me to detect in certain relatively abundant minerals the existence of several substances rarely found in mineral formations. I will now detail successively certain substances which I have studied from this point of view.

1. Rutile.—The rutile of Saint Yrieix, which is now found ordinarily in commerce, has a very curious composition, which I am about to indicate whilst exhibiting the analytical method which I have applied to it, for the purpose of resolving the special problem I have imposed on myself. Heat pulverised rutile, with three or four times its weight of hydrate of potash mixed with a little nitre. This should be effected at a red heat, and, when large quantities are operated upon, in an iron vessel.

Treat the cooled pulverised mass with water, when there remains insoluble pertitanate of potash, oxide of iron, and silico-aluminate of potash, &c. A dark green alkaline solution is also obtained, from which the colouring principle (manganese) can be separated by a few drops of alcohol. The liquid, filtered and treated with excess of sulphuretted hydrogen, assumes a beautiful red purple. A little titanic acid, and sometimes coloured sulphides, are precipitated. Filter the liquid, and exactly saturate it with hydrochloric acid; heat slightly, and when all the sulphuretted hydrogen has disappeared a heavy brown powder is separated on filtering, which roast in a platinum vessel. A fusible and deeply-coloured substance is then obtained, and a crystalline white sublimate.

The fusible matter is vanadic acid, recognisable by all its most marked characteristics: coloration of phosphorus-salt, green in the reducing flame, and yellow in the oxidising flame, then by nitre into a red-orange; easy formation of a deep yellow chloride, which is produced with disengagement of chlorine in contact with hydrochloric acid, and which assumes an azure blue tint, when the excess of hydrochloric acid is driven off. The volatile matter is molybdic acid, which in the reducing flame colours phosphorus salt blue-black, and with phosphoric acid and ammonia yields an insoluble yellow phospho-molybdate, &c.

100 grammes of rutile yields by this method 1·030 gramme of roasted sulphides, or else a mixture of acid, having the following composition :—

	Grammes.
Titanic and stannio acid, with platinum from the vessels	0·211
Vanadic acid	0·323
Molybdic acid	0·486
	1·020

It will be perceived that rutile is a substance from which vanadium may be extracted with great advantage.

2. Cerite of Batnas.—When 100 grammes of cerite are treated with potash and sulphuretted hydrogen in excess, by the process just described, we obtain by the precipitation of a sulpho-salt by means of hydrochloric acid and roasting in close vessels at a low temperature, an acid mixture of the following composition:—

	Grammes.
Acid	0·050
Acid and traces of vanadium	0·510
	0·560

This proportion of telluric acid is furnished by the 100 grammes of cerite. For the cerite I am indebted to M. Bahr, the learned Professor of Chemistry in the Stockholm Mining School, who had the kindness to send me considerable quantities. It is with these valuable materials that M. Damour and myself have prepared the elements for a work, now in course of execution, on the metals so numerous and so difficult to separate, discovered by the illustrious Danish chemist, M. Mosander.

3. Wolfram.—I have ascertained the presence of small quantities of tantalic acid in the Wolfram of St. Leonard, but in such small proportion that I should not have mentioned it here if this fact did not confirm the discovery of M. Damour of tantalic acid in the Wolfram of Chanteloube in the Limousin.

4. Cryolite.—In cryolite, also, I have found very small quantities of a substance possessing all the characteristics of hyponiobic acid. I have, however, not obtained sufficient to examine it in all its reactions. But my opinion on this subject is corroborated by the fact that niobite has been discovered in Greenland cryolite. I have sent a beautiful specimen of the mineral, found in large fragments, to the *Ecole des Mines*.—*Annales de Chimie et de Physique.*

On the Saccharine Matter contained in Acid Fruits: its Origin, Nature, and Transformations, by H. BUIGNET.

The following is a *résumé* of a long paper on this subject which appears in the February and March Numbers of the *Annales de Chimie et de Physique*:—

From the experiments performed during my investigations, the following conclusions may be drawn touching the nature, origin, and transformations of saccharine matter in fruits:—

1. The sugar which forms originally in acid fruits is cane sugar, $C_{12}H_{11}O_{11}$, and in its properties and rotatory power is identical with that obtained from the cane or beetroot.

2. During the maturation of fruits this sugar undergoes a peculiar influence, and is gradually converted into inverted sugar, $C_{12}H_{12}O_{12}$, identical in its qualities and rotatory power with that obtained by the action of acids or glucose ferment on cane sugar.

3. If, when the fruit is perfectly mature, we examine the saccharine matter, we shall find it differently constituted in various fruits. Sometimes it is composed of pure and simple inverted sugar, as in the grape, gooseberry, and fig; sometimes it contains a mingling, in various proportions, of cane sugar and inverted sugar, as is the case with the pineapple, apricot, peach, plum, &c.

4. The cause of this variety is not, as we may well believe, the acidity of the fruits. Experience shows that organic acids, by reason of their relative proportion, their diluted state, and the feeble temperature in which they act, have little power to change the cane sugar with which they are associated. Moreover, there is no connection between the acidity of fruits and the changes exhibited in their saccharine matters. In the lemon, which is excessively acid, more than a quarter of its saccharine matter exists in the state of cane sugar: whilst in the fig, which has scarcely any acidity, the whole consists of inverted sugar. In the apricot, peach, and Mirabelle plum there is about 70 per cent. of cane sugar; while no trace of it is found in the grape and cherry, which, upon analysis, are found to contain a much smaller proportion of acid.

5. The differences presented in the relative proportions of the two sugars seem to be due to a nitrogenised matter, acting as a glucose ferment, analogous to that recently extracted by M. Berthelot from brewers' yeast. By treating crushed gooseberry seeds with cold water, a liquid is obtained which, when cold, changes to inverted sugar the cane sugar contained in the juice of the fruit.

6. The comparative influence of the acid and ferment is exemplified by two parallel experiments made on the juice of the same fruit,—one in which the ferment is precipitated by alcohol; the other in which the acid is neutralised by carbonate of lime. In the first case, the sugar remains a very long time without sensible modification. In the second, on the contrary, it is completely transformed, even in twenty-four hours. The same result is obtained by experiments made on the fruit of the banana. No trace of free acid is to be found in its juice, examined at any period of its vegetation, and yet, if artificially ripened, about two-thirds of the saccharine matter is in the state of inverted sugar.

7. There is an affinity so close between cane and inverted sugar that it is with difficulty that they can be separated. Thus, cane sugar loses its crystallising property when associated with even a very small proportion of inverted sugar. Further, it is thus that protoxide of lead, which acts so differently on the two sugars separately, yet exercises the same action on them when they are mixed together.

8. The best method for separating cane sugar from the fruits which contain it is that indicated by M. Peligot for the analysis of molasses, and consists in forming a saccharate of lime, separating it by boiling, and decomposing it by a current of carbonic acid. It is impossible to obtain any sensible quantity of this sugar in a crystallised state without repeating the treatment with lime, and rendering alcoholic the syrupy solution from which it ought to separate. Observing this twofold condition, I have been able to obtain crystallisable sugar from the peach, apricot, Mirabelle plum, apple, &c.

9. The abundance of starch in the Vegetable Kingdom has led to the supposition that this substance is the true source of the saccharine matter of fruits. Nevertheless, it is impossible to discover its presence in green fruits, either by the microscope or by iodised water. On the other hand, the sugar to which the starch gives place in the artificial transformations that we can make it undergo is a right-handed glucose, with a rotatory power

equal to + 53°; whilst it results from the experiments detailed in this Memoir that the sugar found in acid fruits is cane sugar, either wholly or partially inverted.

10. In green fruits a peculiar principle exists, possessing the property of absorbing iodine with even more energy than starch does, and of forming with this metalloid a perfectly colourless compound. This principle is of an astringent nature, and appears to be allied to tannin in many of its properties. It can be estimated with as much facility as the saccharine matter itself. We know, by experimenting upon it at different stages of maturity that its proportion diminishes as that of the saccharine matter augments.

11. By adding to the juice of green fruit as much iodine as it is capable of absorbing, a precipitate is soon formed by the combination of the iodine and the astringent matter. If we collect this precipitate and carefully wash it, in order to disembarrass it of anything soluble it may have retained, it will form sugar under the influence of weak acids and a suitable temperature.

12. The sugar extracted from the tannin of the gall-nut, by the action of sulphuric acid, of medium concentration and of suitable temperature, is a right-handed glucose, having exactly the same rotatory power as starch glucose. The sugar furnished by the tannin of green fruits, under the same conditions, is also a right-handed glucose, identical with sugar of starch. In this respect tannin affords no better help than starch to a satisfactory theory of the origin of sugar in fruits.

13. Much starch and tannin are found in green bananas, and the two principles diminish progressively and simultaneously, so that no trace of either of them is to be discovered in the ripe fruit. The sugar found in their stead is cane sugar.

14. There exists, then, an essential difference between the products of Art and of Nature with respect to the transformation of either tannin or starch into sugar. There is also a marked difference between the saccharine matter of fruits, according to whether it is produced by the action of vegetative force or irrespective of its influence. Experiments prove that the sugar which continues to be formed in bananas after they are gathered is not cane, but inverted sugar.

TECHNICAL CHEMISTRY.

On Copper Amalgam, *by* H. Dussance.

I have read in many scientific and medical Journals of the discovery of a new alloy by Mr. Oersheim, which alloy has plastic properties, and is an amalgam of copper. This alloy is not new; for in 1853 Professor Peligot, in his lectures at the Conservatoire Imper. des Arts et Metiers, at Paris, spoke of it. At that time I was his assistant, and prepared great quantities of it, and analysed it.

This alloy presents the remarkable property of being soft when recently prepared, and after a short time it becomes so hard that it is difficult to break it to analyse it. Dentists used it to fill teeth, but I believe its use was abandoned. On account of its property of being soft at first, its greatest use is for the moulding of medals. The best way to prepare it is the following:—

1. Take sulphate of copper and dissolve it in water acidulated by a few drops of sulphuric acid; put in this bath a sheet of iron, perfectly clean; the copper is precipitated in the form of a reddish powder.

2. Wash that powder quickly with hot water, and, after mixing it well in a mortar with a few drops of nitrate of mercury, wash it well again with hot water.

3. Add mercury to it in a mortar, and mix it well, until it has the form of a paste that can be worked with the fingers.

4. Wash well with hot water till the washing liquid is perfectly clear.

5. Press the alloy in a deer-skin till all the excess of mercury is out, and expose to the air. In less than an hour it becomes very hard.

A singular property I must notice is the action of zinc on this alloy. If the mercury contains a little zinc the alloy becomes very hard, but in breaks in pieces by the least knock.

Here is the analysis of it:—

Analysis in the Ordinary State.

Copper . .	25·75	25·81
Mercury . .	74·25	74·19
	100·00	100·00

Density . 11·48

Analysis when Soft.

Copper	25·43
Mercury	74·57
	100·00

Density . 11·033

Analysis after it has been Soft and Hard again.

Copper	25·48
Mercury	74·52
	100·00

Density . 11·036

Such is the composition of this alloy, which is given for a new discovery, and which was described seven or eight years ago, and was called *alloy of the dentists.*—*American Journal of Pharmacy.*

The Society of Arts' Annual Exhibition of Inventions.

(*Continued from page 214.*)

No. 92. *Method of Coating Electric Telegraph Wires*, exhibited by Joseph Rogers. The conducting wires are coated with a suitable insulating material. Strips of woven fibre, tapes, or felt, previously saturated with a compound that furthers the insulation, are then bound round. Cords or wires, which have been previously covered with yarn, and prepared by a heated cement, are laid round in one or more series; if more than one series, then each alternate series is laid in an opposite direction, and bound round with wire, wire and yarn, or woven fibrous material, or braided over, the binding wires being used to regulate the gravity and protect the cable from abrasion. No. 93. *Submarine Telegraph Cables*, by the same Exhibitor. The conducting wire, or strand of conducting wires, is first insulated with india-rubber, or gutta-percha, or both; they are then surrounded with a strengthening of flat plaited bands or cords, which have been previously prepared by immersion in a cauldron of heated cement, and are bound round and compressed in one or more series previous to receiving an external braiding of yarn, which is again saturated with a compound which renders the cable impervious to water or the attacks of animalculæ. If more than one conductor is used, they are laid together and bound round with strips of woven fibre, tape and thread, but braiding over is preferable; any number of conducting wires can be enclosed. No. 133. *Patent Mica Garden Labels*, exhibited by Frederick Leiss. These labels are manufactured of the mineral mica, in transparent laminæ. The inscriptions being between sheets of this substance, and so protected, stand all degrees of heat, cold, or humidity. No. 134. *Patent Gravity Potato Selector*, exhibited by James Anderson. The finest, mealiest, and most nutritious potatoes are always denser and heavier than the soft and waxy. By taking advantage of this difference in their specific gravity, the light and inferior potatoes are made to swim on the surface of a solution of salt, while the heavy and good sink to the bottom. By this contrivance the dry and mealy potatoes are separated. In order to classify potatoes into three qualities, good, medium, and inferior, let a solution having a specific gravity of 1·100 be put into one jar and a solution having a specific gravity of 1·080 into another. Very good potatoes only will sink in the first jar; the medium and inferior will remain floating on the surface. Let these be removed to the second jar, containing the solution of the lowest density; the medium quality will sink in it while the inferior will remain floating on the surface. This instrument is based upon a curious fact, which may perhaps be sometimes made use of in private houses. We doubt, however, if it would "pay" the greengrocer to sort out his goods in this manner. No. 130. *Hancock's Patent Butter Machine*, exhibited by Andrew M'Laren and Co. This machine is for purifying butter from acid and buttermilk, and also for cooling it in warm weather, without touching it with the hand. The butter is taken from the churn and placed in a cylinder, and pressed with a screw piston through a perforated plate at the bottom of the cylinder, with a tub of cold water. This operation may be repeated, and the butter will be thereby chilled and purified in the hottest weather. Butter having been thus treated will keep good much longer than when it is worked by the hand, as the heat of the hand imparts a greasy character to the butter, which militates much against its keeping quality, even if the hand could extract the buttermilk from the butter as well as this process. No. 140. *Artificial Stone, Building Bricks, &c.*, exhibited by C. Paine, Dippenhall Silica Works. These imitations of Bath stone, bricks, and brackets, are all made of the building material "Soluble Silica," which is stated to be more durable than natural stone, and less costly. It can be made of any colour. No. 148. *Carbonic Aerial Water and Air Purifier and Cooler*, exhibited by Joshua Jackson. This apparatus consists of a series of disks, fitted with pure wood charcoal, and arranged one above another (either suspended on chains or fixed on a stand) to the number of six, or more or less according to the requirement of the case. Above these disks is fixed a reservoir to hold the impure water. This reservoir is usually a bell-shaped vessel, with a valve or tap at its convex end. Below the disks is a receiver, which is either a jug, pan, or tub, or else a vessel similar to the reservoir. Above the purifiers is a disk, called a filter, which is either placed between the purifiers and the reservoir, or inside the latter. When the valve or tap is opened in the reservoir, the impure water flows through the filter, which retains its mechanical impurities, and then through the whole series of purifiers (which absorbs its gaseous impurities) until it falls into the receiver. While passing through the purifiers, the water is exposed to the cooling and refreshing action of the air, which gives the apparatus an advantage over an earthen or glass filter, which excludes the air. When this apparatus is suspended over a burning lamp, it absorbs the noxious gases arising from the combustion of the oil, gas, or spirits, and also any such gases that may be floating in the air, thus keeping the air in the apartment sweet and fresh. When the purifiers are saturated with impurities, they must be boiled in water for half-an-hour, which must be repeated, if necessary, until they become as pure and efficient as when new. This apparatus is peculiarly applicable to rain-water. No. 158. *Patent Heat Regulator*, exhibited by C. Varley. The object of this apparatus is to ensure any desired uniform temperature in ferneries, forcing-houses, greenhouses, or any chamber heated by gas. There is a tube to the upper end of which is attached a chamber, composed of a series of tubes to increase its surface. The other end of the tube is bent into a **U**, and has a small chamber, with a stuffing-box attached to it; the lower portion of this tube is filled with mercury; the remaining portion of the longer limb of the tube and chamber is filled generally with spirits of wine; there is a smaller tube sliding through the stuffing-box into the larger tube, with a fluid joint, to allow of the motion of the smaller tube. The gas flows from the main through the small tube into the larger one, and out between the two tubes, and thence on to the burners which heat the chamber. As the temperature of the chamber rises, the fluid contained in the larger tube and in the chamber expands, raising the mercury; and when it has raised it to a certain degree, the mercury closes the mouth of the smaller tube, and cuts off the gas. On the temperature falling, the mouth of the smaller tube is unclosed, and the gas turned on. A small burner, in direct connection with the main, is kept constantly burning, to re-light the gas when turned on, or it can be arranged that the flow of gas is only lessened, and never turned off. There is a graduated scale, over which an index attached to the small tube slides, and by sliding this tube up or down the temperature is determined at which the gas shall be turned on and cut off. There is a modification of this apparatus, where, instead of sliding the small tube nearer or further from the surface of the mercury, the adjustment to the desired temperature is obtained by raising or lowering the mercury which forms the valve nearer or further from the mouth of this tube, by means of a plunger worked with a rack and pinion. By this arrangement all stuffing-boxes and moveable joints are avoided. The apparatus is made entirely of iron, and as there is nothing to get out of order, there is no possibility of derangement. Modifications of this apparatus are used to control the temperature of chambers heated by other means, and they are made of various sizes, according to the purpose for which they are applied. No. 173. *Improved Manufacture of Cork Stoppers*, exhibited by A. B. Seithen. The novelty consists in that the corks, instead of being cut, are ground, which gives them any desirable shape, a much smoother surface, with less waste in the material. No. 174. *Improved Artificial Corks*, by the same Exhibitor. The corkwood is cut into fine veneers, and rolled up to the desired size, the end being cemented, thereby producing, from thin wood, large and stout stoppers, of an even elasticity and porous substance, while those cut by hand have a good and a bad side, according to the different growth of the material employed. These are each very useful contrivances, and will be found of great value to the chemist. No. 184. *Steane and Palling's Patent Apparatus for Preventing Candles from Dropping and Guttering*, exhibited by John Gedge and Son. This apparatus consists of a small metal tube with projecting arms, and a metal or porcelain cap. The tube is first passed over the wick of a candle, and afterwards the cap, which rests upon the arms of the tube supported by the candle; as the candle burns the tube becomes heated, and forms a well of fluid tallow or composition near the wick, and the weight of the cap resting upon the arms of the tube, it descends with it as the candle burns. The result is, that the outer circumference of the candle forms a border, over which the tallow will not run, even if the candle be moved rapidly from place to place, and the cap prevents the flame overlapping on to and guttering the candle. No. 213. *Patent Plastic Leather*, exhibited by R. Seager and Co. This is a compound of leather, india-rubber, and gutta-percha. Leather shreds are submitted to an alkaline solution under gentle heat, until reduced to a fibrous condition; they are then rubbed through a wire cylinder; the fibre so obtained is then placed in a masticator with gutta-percha, india-rubber, or a mixture of both, the proportions being according to the texture of the material required; if pliable, more india-rubber; if hard, more leather. The pliable material is adapted for shoe bottoms, being damp-proof, and stated to be more durable and less expensive than common leather. That which is hard and rigid is adapted for picture frames and other ornamental designs, either plain, bronzed, or gilt, as shown by the specimens exhibited; its colour may be so blended as to resemble wood carvings. No. 219. *Safety Medicine Bottles*, exhibited by William Toogood. These bottles are made with contracted necks, so as to allow the fluid to flow in drops; some of them have their necks ground in and moveable for filling; others have an opening at the bottom, for filling, which is closed by a cork, while others are made of dark coloured glass, with fluted sides.

PHARMACY, TOXICOLOGY, &c.

Analysis of Commercial Glacial Phosphoric Acid, by John M. Maisch.

Within a few years past, phosphoric acid and its compounds have attracted the attention of medical practitioners, and are now extensively used, frequently in the form of solutions and syrups, in which free phosphoric acid is employed for keeping the earthy and metallic phosphates in solution. In many of the formulas of these preparations, the glacial phosphoric acid, as it occurs in commerce, is assumed to be the monohydrated HO, PO_5; but those who have made them will have frequently noticed that sometimes the phosphoric acid employed is unable to dissolve the same quantity of phosphates which at other times is taken up by it. As a reason for this, we generally assign the presence of variable proportions of fixed earthy matters, which cannot be volatilised by the fusion of the acid; and it is a fact that almost all of it, the way we meet with it in commerce, yields a white precipitate on being over-saturated with ammonia.

At the suggestion of Professor Procter, I have taken up the subject with the view of ascertaining the nature and the amount of these impurities, and the state in which the acid exists ordinarily.

In the first place, my inquiries were directed to obtain glacial phosphoric acid from different manufacturers; in this I was unsuccessful. By inquiring of a number of friends, I ascertained that they are all using an article made by Merck, of Darmstadt, a number of whose chemicals enjoy a well-merited reputation among us. This induced me to select at random from a larger lot of this acid, three specimens, which were washed with cold water, and dried by means of bibulous paper.

On neutralising an aqueous solution of glacial phosphoric acid, to effect which the generation of heat has been carefully avoided by placing the dish in cold water, ammonia will sometimes throw down no, or only a slight, precipitate, which is increased by the subsequent application of heat. The precipitates thus collected and heated amounted to 8·18, 4·16, 0·80 per cent., and in a fourth instance no precipitate at all was obtained. In the first case I had collected from 550 grains of acid 45 grains of precipitate, and intended to use this for a quantitative analysis; but meeting with an accidental loss of a portion of it, I am only able to give the qualities of these impurities.

The precipitate was dissolved in diluted muriatic acid, and the solution filtered; a residue of silicic acid remained on the filter amounting to 1·5 grains = 0·273 per cent. of the phosphoric acid, or 3·333 per cent. of the weight of the precipitate. The filtrate was mixed with strong alcohol, and sulphuric acid dropped in as long as a precipitate was produced, which was separated by a filter and well washed with alcohol. The acid filtrate, from which the alcohol had been evaporated, yielded a granular white precipitate on being over-saturated with ammonia, which was insoluble in chloride of ammonium; carbonate of potassa produced no change in the cold, but, on boiling, a white precipitate, which was soluble in acetic acid, and this solution was not precipitated by oxalic acid, but yielded a white granular precipitate on the addition of chloride of ammonium, ammonia, and phosphate of ammonia in succession. The filtrate, therefore, contained phosphate of magnesia.

The residue on the filter was wholly soluble in a large quantity of water, and this solution was affected by reagents in the following manner:—Phosphate of soda, turbid; after a while, white precipitate; oxalate of ammonia, immediately a white precipitate, insoluble in acetic acid; ammonia, ferro- and ferrid-cyanide of potassium did not disturb it. These reactions prove the presence of lime, and the absence of alumina, and of iron, and other heavy metals.

The whole precipitate was therefore composed of silicic acid, phosphate of lime, and phosphate of magnesia. I must mention here that a portion of the same crystals was tested with chloride of barium and caustic potassa, but neither sulphuric acid nor ammonia was found in any of the specimens examined.

When the glacial phosphoric acid, dissolved in cold water, is immediately over-saturated with ammonia, filtered from the precipitate, and mixed with a solution of chloride of ammonium and magnesium, a precipitate of ammonio-phosphate of magnesia occurs only of that portion of the phosphoric acid which has, by the action of a moist atmosphere and the consequent gradual absorption of water, been converted into the terhydrate or the common phosphoric acid; an excess of the double chloride produces in the filtrate no further precipitate, until it is heated to boiling. By setting the solution aside, the meta- or pyro-phosphoric acid is gradually converted into the common tribasic acid, but this change is effected in a short time on the application of heat. To determine whether all the acid has been changed in this manner, it will suffice to add more of the double chloride to the ammoniacal solution and boil again; this must be continued until a precipitate ceases to be separated.

But inasmuch as meta- and pyro-phosphate of lime and magnesia are kept in solution by the corresponding phosphate of ammonia, it will be necessary to boil the original solution of the acid for some time, allow it to cool, and supersaturate it with ammonia. The filtrate must then be repeatedly boiled, and some ammonia must be added to the cooled liquor until no further precipitate occurs. We may then be sure that all the earthy phosphates have been separated. The combined precipitates are now exposed to heat until they cease to lose weight; their weight is then deducted from the original weight of the phosphoric acid, and the remainder may be looked upon as the weight of pure hydrated phosphoric acid, provided that in the beginning we have satisfied ourselves of the absence of sulphuric acid and ammonia. Though many works on chemistry recommend to test for nitric and hydrochloric acids, I have never, either on any former or the present occasion, found either of these two; but sulphuric acid I have in one or two cases met with, not however in Merck's.

The ammoniacal solution of phosphoric acid is then precipitated with a solution of chloride of ammonium and magnesium, and after standing for several hours, filtered. The precipitate is washed with a diluted solution of ammonia, until the washings, when oversaturated with pure nitric acid, produce no precipitate in nitrate of silver.

To determine from the precipitate the quantity of anhydrous phosphoric acid, I have followed two ways: if its quantity was large, I have dried it upon a brick tile at ordinary temperature, ascertained its precise weight, ignited a convenient portion of it, and then calculated the weight which would have resulted from the ignition of the whole. If the quantity of the precipitate was small, the whole of it was ignited, together with the filter, the amount of ashes of which was ascertained by incinerating a portion of the same sheet of paper, and deducting the weight of ashes from the whole weight obtained. The result in either case is pyrophosphate of magnesia, $2MgO, PO_5$, which contains 63·33 per cent. of PO_5.

The following Table contains in the first column the amount of phosphoric acid employed; in the second, the weight of impurities; third, the per-centage of the same; fourth, the weight of $2MgO, pPO_5$; fifth, the weight of PO_5; sixth, its per-centage of the pure hydrate; and in the seventh column the per-centage of the free PO_5 in the specimen:—

I. Grs.	II. Grs.	III.	IV. Grs.	V. Grs.	VI.	VII.
550	45·0	8·1818	609·86	386·12	76·46	70·20
100·75	0·8	·794	122·77	77·75	77·79	77·19
48·20	none	—	63·22	40·04	83·48	83·48
—	—	4·16	—	—	—	—

Pure metaphosphoric acid, HO, PO_5, contains 88·806 per cent. PO_5, and the best specimen which I have examined contains 5·32 per cent. less, while between the highest and lowest there is a difference of 13·28 per cent. in the available acid, and after the deduction of the earthy phosphates, the difference is still 7·02 per cent. It cannot be doubted that the large amount of impurities found in the first specimen is accidental, and probably derived from the vessel in which the phosphoric acid was fused; the purity of the other two specimens, taken out of the same jar, speak in favour of this supposition, and it appears to me, on account of this great difference, as if most likely different portions of the acid, fused at the same time in one vessel, may contain a variable proportion of impurities.

From the amount of water alone no conclusion can be arrived at as to the proportion of the three phosphoric acids present at the same time. To ascertain this, an acid, known beforehand not to contain any impurities, after its solution in cold water and over-saturation with ammonia, may be precipitated with chloride of ammonium and magnesium; this precipitate will contain all the common phosphoric acid. The acid of the filtrate is then to be converted into the ordinary variety, and precipitated as before; this precipitate will now contain all the meta and pyrophosphoric acids, the amount of each of which can be easily calculated from the remaining weight after the deduction of the terhydrate.—*American Journal of Pharmacy.*

PROCEEDINGS OF SOCIETIES.

SOCIETY OF ARTS.

Wednesday, March 20, 1861.

Professor H. W. Williamson *in the Chair.*

(Continued from page 221.)

In France, M. Selligue had obtained tars by distilling the bituminous schists of Autun, and the paper coal and bituminous slate of the coal formation. He also purified the oils, and produced burning fluids, which had a considerable sale in France. In 1847, Mansfield obtained a patent for the separation and purification of volatile liquids from tar, and his benzole (which he was the first to obtain, nearly pure, on a large scale) met with a ready sale as a solvent of caoutchouc. Mr. Lowe had, a few years before, applied the lighter portions of coal naphtha to increase the luminosity of coal gases; and naphthalized gas was for a time a familiar term.

In 1849-50, works were erected at Weymouth, in Dorsetshire, for carrying out Du Buisson's process. Works were also carried on near Wareham, in the same county, by the Bituminous Shale Company, and they advertised the sale of oils from shale as—"Oil No. 1, or volatile; Oil No. 2, or rough oil, which contains paraffine, grease, and varnish; Oil No. 3, or machine oil, which contains paraffine, grease, and varnish, grease liquid, alkaline ditto manufactured, asphalte or varnish." It appears, also, that Mr. John Thomas Cooper, the Consulting Chemist, in 1847, prepared paraffine oil, lubricating oil from paraffine, and paraffine itself, from the distillation of coal. In 1849, the Boghead Cannel became known, and was used as a source of paraffine and paraffine oils. Mr. James Young's patent bears date 1850, and its origin may be referred to about the year 1847, when Dr. Lyon Playfair called Mr. Young's attentions to a petroleum spring in Derbyshire, which he thought might produce some oils useful to the manufacturer. On examining this spring, it was found to be in an old coal mine. The coal had been worked out, and from the roof, which consisted of sandstone, there exuded a thick, dirty-looking oil, known as petroleum or rock oil. Mr. Young obtained from it some useful lubricating oils, and as the supply was but limited, he invented a process for obtaining rock oil from coal, the varieties best suited to his purpose being parrot-coal, cannel-coal, gas-coal, and, lastly, Boghead mineral. Mr. Young is entitled to the merit of having invented a number of useful processes for the separation and purification of these oils, without apparently being aware of what had already been accomplished in the same branch of industry. Mr. Young, with his partner, Mr. Meldrum, erected works on the Boghead estate, 15 miles from the Forth, for the advantage of being within reach of the Boghead cannel, or, as it is now called, the Torbanehill Mineral. Their process is as follows:—The retort is placed vertically, with a hopper on the top; the fire is applied by a kiln below; the retort is cased with fire-brick for about three-fourths of its height, so that the heat passes through this to the retort. As the coals come down, they are distilled by the heat, and the products are passed through a worm pipe into a refrigerator, where they are condensed. The first product is described as a crude oil, containing paraffine. It is distinguished from tar in not drying by exposure to the air, and in being lighter than water. The rectification is by the application of sulphuric acid and soda, with two or three distillations. About two-thirds of the oil is run off as oil, part for burning and part for lubricating. The solid remainder contains paraffine, from which it may be extracted by the ordinary methods of purification.

In the Great Exhibition of 1851, Mr. Young obtained credit, and justly, for the production of paraffine in considerable quantities. The former processes, although containing all that was necessary for its production, were not commercially successful. Either they do not produce the article in sufficient quantity, or at a sufficiently low cost to be profitable. Mr. Young has the great merit of first drawing public attention to the commercial value of paraffine, and of pointing out the sources from which it could be profitably extracted.

In Germany, paraffine works were started in 1855, at Beuel (near Bonn), Ludwigshafen, and Töplitz, and paraffine was soon applied to many uses, and met with a large demand. Manufactories were also established in France and Austria, and most extensively in several parts of the United States of America.

In the last-named country the first attempts were made on the bituminous shales of Dorchester, New Brunswick. Extensive works were erected at Brooklyn, near New York, Pittsburgh, Baltimore, and at several places along the Ohio valley and river. The oils produced are in great demand, exceeding till lately the supply, as solvents, lubricators, and for illumination. They give a whiter and more brilliant light than any fixed or fat oil, and are much cheaper. Hence they limit the demand, and keep down the price of fish, lard, and sperm oils; but complaints have been made that the oil is sent out in a crude and impure state, retaining much of the tar and creosote impurities.

The term Photogen has been applied to the oils or naphthas obtained from shales, brown coals, cannels, and especially the Boghead variety, and used for illuminating purposes. It is also known as paraffine oil. It differs in specific gravity from ordinary coal oils of the same boiling point, varying from ·820 to ·830, while common coal naphtha has a density of ·850. When photogen has a higher density than that indicated, and a very high boiling point, it probably contains paraffine. The photogen may also be obtained from ordinary bituminous coals, by distilling them at a temperature of about 700° C. Coal is broken small, heated in iron retorts, and the tar is received through a very wide worm into tanks. In some cases the coals are distilled by the heat of their own combustion, or by a modification of Lord Dundonald's plan. The tar thus obtained is distilled, and the lighter portions, after purification, form what is called Boghead naphtha, or as, in Germany and elsewhere, the distillate is divided into two portions, the more volatile being photogen, and the less volatile solar oil. Both oils are purified by concentrated sulphuric acid, which removes the highly coloured and odorous constituents of the distillate, and also by an alkali, which removes carbolic acid and its congeners, together with the remains of the sulphuric acid and some sulphurous acid. In this country the heavy and light oils are mixed together, so as to produce a fluid of medium density and volatility. The more volatile hydro-carbons give great inflammability and fluidity, and they are also more odorous than the less volatile portion of the distillate, which is the true paraffine oil.

At the establishment near Bonn, the lignite of the neighbourhood, known as leaf or paper-coal, is distilled at a low red heat in iron retorts. A blackish tar and ammoniacal liquid are the product; the former is said to yield 90 per cent. of oils, 50 per cent. of which are thin enough to burn in lamps. They are purified by means of sulphuric acid and alkaline leys.

Wagenmann's process is with bituminous coal, which is broken into small lumps, sprinkled with milk of lime, to get rid of sulphur, dried in a desiccating furnace, and distilled in retorts. The liquid products are received into a large reservoir, kept at a constant temperature of 30° Centigrade, where the tar separates from the ammoniacal liquor, and the latter being mixed with the residue of the large retorts, furnishes a good manure. The tar is pumped up into the purifying apparatus, mixed with sulphate of iron, and distilled in vessels of the capacity of 350 gallons, by means of super heated steam. The products are condensed in a leaden coil, and consist of volatile liquids of the specific gravity of ·700 to ·865, heavy oils for lubrication, ·865 to ·900 and paraffine, ·900 to ·930. These three substances are treated each with 4, 6, and 8 per cent. of sulphuric acid, 1⅛th and 2 per cent. of hydrochloric acid, and 1 per cent. of acid chromate of potash, with which they are agitated for half an hour. They are than left for three hours, poured off the dregs, and mixed with 2, 3, and 4 per cent. of a ley of caustic potash (50° B.). Lastly, each of the products thus purified is placed in a still, and distilled by superheated steam. No. 1 mixed with No. 2, so as to obtain the specific gravity of ·820, produces the mineral oil or photogen, which is burnt in lamps fitted for the purpose. Part of the product distilled from No. 2 having a specific gravity ·860 to ·700, forms the solar oil, which may be burnt in Argand or Carcel lamps; the rest of No. 2, mixed with some of the product of No. 3, furnishes the lubricating oil for machines. The remainder of No. 3 is introduced into a vat, where the temperature is lowered till it crystallises. In the course of three or four weeks, the paraffine crystallises in large tablets, and is separated from the adhering oil by means of centrifugal machines, making 2000 revolutions per minute. The paraffine is melted and rolled into squares, and subjected to a strong hydrostatic pressure. It is then again melted, and treated with 50 per cent. of strong sulphuric acid, at a temperature of 180° C. In two hours the paraffine separates from the acid, and is washed with water; it is then run into cakes, and pressed in the hydrostatic press while hot between two layers of hair cloth. It is once more melted, mixed with 5 per cent. of stearine, and maintained for some hours at a temperature of 150° C. in a leaden apparatus. It is lastly mixed with one per cent. of a solution of caustic potash (40° B.), and at the end of two hours all the impurities will be separated, and the paraffine may be drawn off as limpid as water.

In a valuable article on photogen, by Mr. Greville Williams, in the last edition of "Ure's Dictionary," is a table showing the materials employed for distilling these oils, with the per-centage of tar of the oils and of paraffine. The raw material consists of Trinidad pitch, the Boghead cannel, or Torbane mineral of Scotland, Dorset shale, Belmar turf from Ireland, George's bitumen from Neuwied, paper coal and brown coal from various parts of Germany, and Rangoon tar. But of all the substances hitherto employed none are so rich in results as the last.

This brings us to speak of some of Nature's distillations. Various bitumeniferous productions are formed in vegetable matter during its conversion into coal. Among these are mineral oil, an inflammable fluid, sometimes forming powerful springs, and frequently occurring in carboniferous deposits. Likewise naphtha, a transparent and nearly colourless liquid, burning with a copious flame, and strong odour, and leaving no residuum. Springs of naphtha may burst forth during mining operations, as in the coal-shale of Derbyshire, where, in driving a level, a large quantity of this liquid poured forth and covered the surface of the water in the level, and being accidentally set on fire, it formed a burning spring which lasted several weeks. There is also petroleum, a dark coloured substance, thicker than common tar, which rises in immense quantities from some of our own coal beds, and from the carboniferous strata of some parts of Asia and elsewhere. Petroleum springs do not seem to depend on combustion, as has been supposed, but to be simply the effect of subterranean heat. And it is not necessary that the depth should be very great beneath the surface to give a temperature equal to the boiling point of water, or of mineral oil. In such a position the oil may be supposed to suffer a slow distillation, and so find its way to the surface, or it so impregnates the earth as to form springs and wells, as in Persia and India. Petroleum springs are those whose waters contain a mixture of petroleum and the various minerals allied to it, such as bitumen, naphtha, asphaltum, and pitch. They are very numerous, and in many cases undoubtedly connected with subterranean fires which raise or sublime the most subtle parts of the bituminous matters contained in rocks. Petroleum springs occur in abundance in Modena and Parma, in Italy, but the most powerful of which we have any account are those on the Irrawadi, in the Burman empire. In one locality there are said to be 250 wells, which yield 400,000 hogsheads of petroleum annually. The heat by which these chemical changes and transformations produce from vegetable matter such enormous quantities of petroleum, may also cause this substance to be forced up to the surface, in its native state, where by exposure to the air it becomes inspissated, and forms the different varieties of pure and earthy pitch or asphaltum, as in the case of the vast accumulations in the island of Trinidad. Bitumen is an inspissated mineral oil, of a dark brown colour, with a strong odour of tar. It sometimes occurs of the consistence of jelly, bearing some resemblance to soft india-rubber, and as it will remove the traces of a pencil, it has been named "mineral caoutchouc." Native bitumen and the substance of that name obtained from certain varieties of coal, when heated, if not identical, are closely allied, or yield products which are so. The varying proportions of bitumen which different varieties of coal are capable of yielding, depend on the amount of change which the vegetation has undergone since its deposition. In some cases the internal heat, accompanied by moisture and great pressure, has expelled all the volatile matter, and reduced the coal to the state of anthracite. The amount of volatile matter in coal and bituminous minerals may vary from 10 to 63 per cent., forming the different varieties of dry and fat coal. The bitumen of coal resembles the bitumen of nature, known as asphalte and mineral tar, in its sensible qualities and general appearance; but it does not contain the same proximate principles; nor does it by dry distillation yield the same fluids. "They belong, however, to the same natural group or series, and tend to strengthen the opinion generally held that bitumen, petroleum, and asphalte, arise from the decomposition of fossil vegetation. The natural bitumens always contain some volatile oil ready formed, and their varieties depend on the greater or less proportion of this volatile oil present in them."

Petroleum, the Lecturer said, is the most abundant source of paraffine and photogen. The wells of Pennsylvania afford from ten to fifty barrels of oil per diem, or even more. Last year oil was discovered about fifty miles from Cleveland, Ohio, when some fifty wells were at once sunk, and afterwards several hundreds, yielding from ten to sixty barrels daily.

The Burmese naphtha, or Rangoon tar, was next noticed. Here we find the following description of the separating processes:—

"The Rangoon tar obtained from peat, coal, &c., have been formed by their decomposition.

"In the first place, the Rangoon tar is distilled with steam at 212° Fahr. The distillate contains a mixture of a number of volatile hydro-carbons, which it is difficult to separate on account of the diffusible nature of the vapours, however different their boiling points. In practice a second or third distillation is adopted, and the products are arranged according to their boiling points or densities, which vary from ·627 to ·860, and although they all originally came over with steam at 212° Fahr., their boiling points range from 80° to upwards of 400° Fahr. All these liquids are colourless, and solvents of caoutchouc. Those of low specific gravity are known as Sherwoodole and Belmontine. They have considerable detergent power, removing stains from silk without impairing delicate colours. The distillate of higher specific gravity burns with a brilliant white flame, and as it cannot be ignited without a wick, forms a useful lamp fuel. Sometimes these lamp fuels may not be sufficiently fluid at ordinary temperatures to ascend the wick. Messrs. Price and Co. have introduced a modification of the old cocoa-nut oil lamp for overcoming this difficulty. A piece of metal is continued from the metal wick-holder down into the reservoir, and by its conducting power gently raises the temperature of the fuel.

"A small per-centage of hydro-carbons of the benzole series comes over in the first operation. By treating it with nitric acid, nitro-benzole, and other substances valuable in perfumery, are eliminated.

"After the first distillation, about three-fourths of the original material remains. This is fused and purified from extraneous ingredients by means of sulphuric acid. A black precipitate is thrown down, which, after being washed, resembles native asphaltum. The purified fluid is removed to a still, and heat is applied by steam through heated iron tubes. The distillates are arranged according to their distilling points, from 300° to 600° Fahr. Those obtained at 430° Fahr. and upwards contain paraffine.

"It is stated in the Memoir above referred to that at 212°, 11 per cent. of fluid hydro-carbons distil over, entirely free from paraffine; that between 230° and 293°, 10 per cent. more fluid distils over, containing only a very small quantity of paraffine; that between 293° and 320° the distillate is very small in quantity; but from that to the fusing point of lead, 20 per cent. more is obtained, and although containing an appreciable amount of paraffine, it remains fluid at 32°. At this point of the distillation the products begin to solidify on cooling, and there is obtained 31 per cent. of a substance of a sufficient consistency to be submitted to pressure. On raising the heat considerably, 21 per cent. of fluids and paraffine distil over. In the last stage of the operation, 3 per cent. of pitch-like matters are obtained, the residue in the still, consisting of coke containing a little earthy matter, amounts to 4 per cent.

"The results of this distillation are given in the following table:—

At 212°, free from paraffine	11·0
230 to 293°, 293 to 320°, } a little paraffine	10·0
320 to fusing point of lead, containing paraffine, but still fluid at 32°	20·0
At about the fusing point of lead, sufficiently solid to be submitted to pressure	31·0
Beyond fusing point of lead, quantity of paraffine diminishes	21·0
Last distilled, pitchy matters	3·0
Residue in still, coke containing a little earthy impurity	4·0
	100·0

"The above distillates are all lighter than water. Most of the paraffine may be separated from them by exposing them to a freezing mixture, so that no less than between 10 and 11 per cent. of this valuable solid hydro-carbon may be obtained from the Burmese naphtha."

Our last quotation shall be the concluding paragraph of Mr. Tomlinson's paper, which terminated somewhat abruptly :—

"Bolley states that most of the commercial paraffines contain stearic acid, that when paraffine is melted, it is readily acted on by chlorine, when it gives off bubbles of hydrochloric acid gas, and retains some of the acid tenaciously. In the compound thus formed, some of the hydrogen is replaced by chlorine; it is tolerably soluble in benzine, and the solution may be readily spread on paper, wood, &c. The name of Chloroffine has been proposed for this substance. Bolley says that he has met with paraffine with as high a melting point as 149·9°, and Laurent has found it as low as 91·4°.

"Leaving the question of the composition of paraffine to be settled by competent authorities, there can be no doubt as to the value of this beautiful substance as a candle-making material. The illuminating power is considerable. It is stated that a paraffine candle, weighing one-eighth of a pound, will give as much light as a spermaceti or stearine candle weighing one-sixth of a pound. The low temperature at which it fuses, and the high temperature to which it may be raised without decomposition, namely, upwards of 600° F., point it out as a valuable material for a bath for chemical purposes. The lubricating properties of the fluids obtained in the second distillation have already been referred to. Fixed oils are decomposable into acids which corrode metals; the paraffine oils have no such property, so that they do not corrode the metal bearings of machinery, nor is the brass work of lamps injured by the hydro-carbons burnt in them. As paraffine is not acted on by acids or alkalies, it has been recommended to guard, by means of paraffine, the stoppers of bottles holding those substances. A paraffine paper for holding caustic alkali has also been suggested. And, lastly, we may remark, that paraffine realises the desideratum pointed out by Liebig many years ago, of a solid olefiant gas, although it is somewhat singular, that in the last edition of his 'Chemical Letters,' that great chemist still appeals to science to produce his favourite solid carburet of hydrogen."

The discussion upon the preceding paper would present but few special points of interest to our readers, even if the space at our command permitted us to report it, which, however, is not the case. The proceedings terminated with the usual vote of thanks.

CHEMICAL SOCIETY.

Thursday, March 21, 1861.

Professor Brodie, *F.R.S., President, in the Chair.*

Messrs. H. O. Huskisson, A. C. McLean, W. V. Simons, and E. B. Brown, M.D., were elected Fellows.

Dr. Williamson read the following paper, "*On Thermo-Dynamics in Relation to Chemical Affinity.*" The subject which I have the honour of bringing before the Society is one which does not belong exclusively to the domain of chemistry: it involves of necessity considerations derived from other departments of science, which are not always taken notice of by chemists. I may, perhaps, be permitted, before entering upon the subject itself, to premise a few words, merely in order to describe the position of the previous question upon which I found my remarks, for the purpose of ascertaining whether my understanding of it agrees with that of the other members of the Society. I allude especially to those admirable results which have attracted the attention of the whole scientific world, in England and abroad, for many years past, as being amongst the most important of modern times. I mean the relation between what are usually called the "forces;" the first fact being, that when one of the forces disappears, some other force is—to use the ordinary term—"produced" in its place; that, in fact, there is no such thing in nature as *destruction* of force, any more than there is such a thing as destruction of matter. Of course that is well known to all the members of the Society, but the part of that subject to which I have especially to allude, and the results which, more especially, I shall have the honour of laying before you, are those which have been elicited by the research of Mr. Joule and other distinguished gentlemen as to the relation between mechanical force—the motion of masses—and heat. I shall take for granted, in the remarks I shall make, that you all believe that heat is nothing else than the motion of small particles of matter. I am not aware of any serious doubt having been raised, by the investigators of that subject, in regard to that proposition. I shall, therefore, assume that to be the case in my remarks; and that a certain definite amount of mechanical force is always produced where heat disappears, provided there be nothing else produced at the same time but mechanical force. Of course the members are all aware that Mr. Joule first determined with accuracy the weight falling through a certain distance,—the number of foot-pounds, as he designates them, which correspond to a certain quantity of heat. He determined that the unit of heat, which is one pint of water heated one degree F. (the standard usually adopted in this country), if transformed completely into force, would give rise to 772 foot-pounds of force; that is, that a pound would be raised 772 feet; or that 772 pounds would be raised one foot, which is the same thing. As a matter of fact, he found that when a pound weight fell from 772 feet towards the earth, it consumed that power in friction; and when he measured the amount of heat evolved, it corresponded to one degree of heat, calorimetrically designated.

Now, the term "latent" which is familiar to everybody, as applied to heat, has for some time been chiefly applied to certain cases of the disappearance of heat, and the production of something else; but I think it would be desirable, on account of the resemblance of other cases of the disappearance of heat and the production of other forces, to extend the term, which is used only in certain cases, to those other cases in which a similar process occurs. I will not give here the particular instances in which I should suggest the extension of the term; I will only mention a couple by way of illustration. For instance, not only would I apply it to the fusion of ice and the evaporation of water, inasmuch as we can regain heat when we solidify water or condense steam; but I would similarly apply the term to the expansion of the permanent gases. If we had a certain quantity of air in a cylinder, and if I were to raise the piston of that cylinder so as to expand the air, I should have a certain fall of temperature in that enclosed air, and the heat contained thus in that air has to be distributed equally in a greater space. I should consider that the heat in that case became latent. But more especially would I consider it important chemically to apply the term to those cases of the disappearance of heat in which the chemical force is overcome whilst heat disappears. Thus if we decompose water by a galvanic current, and if we determine the quantity of heat which makes its appearance during five minutes of the passage of the current; and if we then remove this liquid and put in a thin wire of such length that it should have the same resistance as the liquid to the passage of the current, and if we then measure the amount of heat in the wire, it is well known, from the researches of Mr. Joule, Mr. Wood, and others, that in the first experiment, where we decompose the water, we get considerably less heat evolved than in the second. We do not get the same quantity of heat as we do in the second experiment where we cause this force to act upon the resisting wire through which the current is forced to pass. It is stored up, in fact, in the oxygen and hydrogen gases which are evolved during the former process. Now, I would submit that cases of that kind ought as much to be called cases of the absorption of latent heat, as those transformations to which we are accustomed to apply the term. I will give you one more instance before I leave that point, because it is one of extensive occurrence; and that is, the ordinary process of the growth of plants in which it is well known that by an action, unfortunately unknown to us at present, upon the surfaces of what I was going to call the "lungs" of plants—the leaves of plants, the rays of the sun cause a process which is the opposite of combustion, a process by which oxygen is evolved, and combustible bodies—wood and so forth—made: and there is no doubt that heat is absorbed as much as in the other processes to which we are accustomed to apply the term "latent heat."

Now, in chemistry, the physical basis which we all adopt for our own reasonings upon the more difficult transformations is that of the atomic hypothesis. We do admit that there is a limit to the divisibility of matter, and it is to be assumed that masses of matter such as we can deal with are built up of certain small particles which we call "atoms." I have no intention of entering into the evidences of this theory, because, of course, they are quite as well known to you as to myself; I merely allude to it in order to admit frankly that I use that theory, and that it is a necessary condition of the truth of the arguments which I shall have the honour of laying before you. If that theory be wrong, of course what I shall say will fall to the ground; but I believe I am not overstating it when I describe it as the only physical theory at present given for the transformations of matter.

Now, from many points of view, we chemists are bound in all our reasonings to take at the commencement facts which have been established in the study of other phenomena; because, after all, the matters we have to deal with—solids, liquids, and gases—are the same as natural philosophers have to deal with; and, if any one truth has been ascertained by natural philosophers, or "physicists," as we call them, regarding matter, that truth is equally true for chemists; and I say, therefore, that if it be proved by physicists that certain forces are acting upon the small particles of matter by which displacement of them must occur, and if it be proved by physicists that certain small particles must and do exist, I say that chemists have no right to shut their eyes to the fact, and to reason upon the constitution of matter as if it were built up of little particles which are absolutely at rest. However, without drawing *à priori* such a conclusion, I would rather beg leave to admit what I conceive most members of the Society must be prepared to consider reasonable, that these small particles of matter—gaseous, liquid, and solid matter—are, under all circumstances in which we have to deal with them, relatively to one another in a state of vibration. That is admitted, I believe, by everybody—of gases; it is also admitted, usually, of liquids, but with regard to solids I believe that many persons experience some reluctance to admit that the constituent atoms can be in a state of motion relative to one another. It is not natural for some persons to conclude that a solid which retains, for a certain time, any particular form should have its particles in motion, for one would think that in such a case it would alter its form or that it would yield to pressure; yet a number of cases, all the results of such motion, are known to everybody; and I submit that the belief of the constituent atoms of any solids being at rest relatively to one another is inconsistent with well-established facts derived from natural philosophy, and even from chemical considerations; and I shall therefore extend this hypothesis to the three different states of matter, and not confine it to the liquid and gaseous alone.

Now, some years ago I had occasion to propose a theory by which the decompositions of two salts which are dissolved or liquified by any process can be represented in accordance with these considerations. I will briefly state an outline of them; and what I shall endeavour to show to the Society will be not only the convenience of this mode of representing the constitution of matter in itself, and its accordance with what we know of chemical affinity; but also the confirmation which it receives from the results of our observations in other departments of knowledge.

Now, in order to represent the movements of atoms by which we obtain the results which we designate by the general name of "chemical affinity," it is necessary to take by way of example, the case of two salts, which, for convenience, shall be of a constitution somewhat analogous to one another, and to assume that they are mixed with one another in a liquid form, either by fusion or by solution in water. I will, by way of example, first take the case of hydrochloric acid and nitrate of potash—

$$\begin{matrix} Cl\ H \\ NO_6\ K \end{matrix}$$

and I will take the old atomic weights. Now, when these two bodies are mixed, and when the reaction of the one upon the other commences, the interchange of materials causes the formation of two other compounds—chloride of potassium and nitrate of hydrogen.

$$\begin{matrix} Cl\ K \\ NO_6\ H \end{matrix}$$

I conceive it will be accordant with the ordinary acceptation of the term "affinity," if I say that the greater affinity exists in the new compounds; for, in comparing the quantities of two bodies which co-exist in the same circumstances, the greater affinity will be at work, in holding together those that remain in greater quantity. I only judge of the force by its effects, and I conceive that the only measure we can have, and the only way in which we can judge, of the relative forces which will hold together different compounds which are mixed with one another is that of comparing the quantities of the two which remain with one another. It will be clearer by taking an example. For instance—supposing the hydrochloric acid and the potash to unite, I will call that interchange by which they decompose one another, the "decomposing change," as that is a convenient term; and let us assume that the forces, by way of example, which hold together the hydrochloric acid and the nitrate of potash, and the forces which hold together the elements in the other compound are equal to, in the first example, the forces which hold together the products of their decomposition—chloride of potassium and nitrate of hydrogen. Now, I say in that case the quantities of the original salts, and of the products of their decomposition will of necessity be equal, if the affinity which holds together the atoms of the original substances is equal to the affinity which holds together the atoms of the new salts. If, on the contrary, there are less of the products of decomposition contained in the liquids after once it has gained a state of equilibrium than in the original compounds, I should then say that the affinity of the elements for one another, according to this new arrangement, is less than the affinity of the elements in the old arrangement of the particles.

Let us suppose another numerical case. I really do not know in this case what the proportion is, for we have no numbers; but let us assume that the affinity of the substances, chloride of hydrogen and nitrate of potash, is twice as great as the affinity of the same elements for one another in the form of chloride of potassium and nitric acid and water. The result would be, that as soon as the mixture of the original salts had been left for a sufficient time, and the equilibrium had established itself between the decomposing forces, and the reproducing forces of the original substance, that we should have according to the starting-point which I have adopted, twice as many of the original atoms as of the compound formed by the decomposition. Now, that being premised, the dynamical consideration, or the consideration of the process of the motion of the atoms, which we identify as chemical affinity, to which the formation of the new compounds is due, has to be established in the following manner:—By way of illustration, suppose we have 100 atoms of the original compounds, and that I only get 50 atoms of the products of decomposition; and that this proportion is the result of the forces which hold these together being twice as great as the affinity of the elements in the new compounds for one another. Let us assume that under these circumstances no further change in the composition of the liquid takes place, and that no further disturbance in the proportions occurs; now, the fact that hydrochloric acid in the liquid is constantly decomposing nitrate of potash would disturb its equilibrium, and the fact that chloride of potassium and nitric acid are interchanging would also disturb it in the opposite direction; and if this equilibrium is maintained undisturbed, we must admit that these 100 atoms of hydrochloric acid are re-acting as many times in the unit of time, and that these 100 atoms of the original compound are interchanging bases as often in the same time as these 50 atoms of the products of decomposition are interchanging bases so as to reproduce the original compound. I mean to say that the per centage of atoms which are decomposing so as to reproduce the original bodies must be as much greater as the actual number of atoms in these products of decomposition. I conceive in that statement I am making no hypothesis whatever. If it be true that salts when dissolved in liquid are interchanging bases constantly so as to form a product of decomposition of this kind, and if it be also true that this process of interchange stops at a certain point, and does not change the whole of the original salts, but only goes so far as to decompose a certain proportion of them, then the equilibrium must be owing to the fact of the equality of those products,—that is to say, the number of atoms in the original salts multiplied by the proportion of them must be equal to the number of the products of decomposition multiplied by the per-centage of those which are interchanged in the unit of time, so as to reproduce the original compounds.

The only general theory of chemical action which has been worked out to any considerable extent that I can at present allude to is that of Berthollet, which explained the decompositions which occur by an assumption which has prevailed in the infancy of many branches of science, and that is the assumption of a metaphysical principle called "predisposing affinity." I dare say most of the members of the Society will agree with me that that is merely a name—and not a very good name, perhaps—for our ignorance of a process. For instance, when we say of the action of zinc and hydrochloric acid that the predisposing affinity of the acid makes zinc decompose water which it could not have done by itself, it is certainly a method of reasoning which, although excusable in the infancy of science, is not excusable at the present time. We should, therefore, put away such entities, and disuse such principles by simply stating the fact that zinc does displace hydrogen and unite with chlorine. The illustrious Berthollet said that when two salts are mixed, and when, by the interchange of bases, one or more insoluble products can be formed, the knowledge that such results would ensue induces the salts that you mix to decompose when they would not otherwise. Berthollet's theory has generally been used in connection with predisposing affinity. On the old assumption, chloride of silver would be produced by hydrochloric acid decomposing nitrate of silver in solution,—the nitrate of silver undergoing decomposition from the "looming in the distance," I suppose, of the consciousness of this result. I wish to show how, in this case, we can, by simple statement of fact, explain those results, and explain the beautiful general law of Berthollet, that, when we mix two salts, which, by interchanging bases, would form an insoluble salt, of course that result takes place. Of course the salts interchange bases. This is well known to be a truth, so established as to amount almost to a natural law. Now, according to this principle, we see at once, without any assumption whatever, why the result which Berthollet points out must ensue. If the hydrogen and silver interchange, producing chloride of silver and nitrate of hydrogen, the chloride of silver being an insoluble body gradually falls to the bottom—gets out of the way of the nitric acid, preventing a change by which the original compounds would again occur. Thus this fact of the insolubility of one of the products prevents that process by which the original bodies would, under ordinary circumstances, be gradually reproduced in proportion as they were decomposed; and I conceive that that is one, and not an unimportant, use of the application of this general fact of the motion of particles in solutions of this kind—a motion by which analogous particles are constantly undergoing a certain interchange.

Now, from other considerations in chemistry we are unfortunately not able to derive as exact information as we need, numerically, regarding the relative quantities of heat contained in matter; I mean, not even the specific heat of elements which have been studied with great attention and care by the distinguished Berthollet and Regnault, and others. I conceive that the only direct physical estimate we can have at present of the quantity of heat or motion contained in the particles of any amount of matter must be derived from a measurement of the quantity of heat which it absorbs in undergoing a given rise of temperature —one degree Fahr., for instance,—and in this subject our information is unfortunately defective in some respects, owing, among other reasons, to the great variations in the state of matter under different conditions: for one and the same body has been found to possess a different specific heat at different times; and it has not been as yet possible to follow out the connection between the different states of one kind of matter. Doubtless much of the disagreement of the results we have on that important subject is owing to the insufficient observations of the different conditions under which the different observations were made. For instance, one would be disposed at first to say that those metals which require the most heat to pass into a different form contain most specific heat. But, unfortunately, our measurements have only been made within the limited range of the thermometer. We do not know the quantity of heat absorbed by any substance when it is gradually warmed from the observed zero up to a given temperature; and it is more than probable that the actual specific heats which we observe in elements and in compounds, at temperatures under which we observe them, are very different, and do not stand to one another necessarily in the same relation as the total quantities of heat contained in the substances stand to one another; because some bodies absorb heat and increase their specific heat, more rapidly than others; therefore, by change of temperature certain differences may occur. However, taking the specific heats of bodies as we possess them, I think, allowing for all these imperfections, and others, we have notwithstanding a very advantageous light upon our subject. In the first place, we are able to compare the proportions of one and the same element in different states. Take the element carbon, for instance, which occurs in a variety of forms, some more easily igneous than others, and capable of combining more readily with oxygen, and burning very readily, as in the form of lamp-black; the same element occurring in other forms, such as coke, plumbago, or diamond itself, which possess chemically this property in a very much less powerful degree. It is well known from the researches of M. Regnault, M. Silberman, and others, that some varieties of carbon have a less specific heat than others, that some absorb less heat in order to undergo a certain rise of temperature than the more chemically active varieties; and it has actually been shown that those varieties which have the greatest specific heat evolve, on combustion, the greatest quantity of heat. Take another instance. Phosphorus, as is well known, can be transformed by various processes into another state which is more dense —in which the particles are more closely aggregated together. It is well known that the red phosphorus does not melt so easily as the other sort; (more especially is it known that it has a less specific heat; and estimating this specific heat as the measure of the motion in its particles, the particles of the red phosphorus are in a less rapid state of vibration or momentum than the particles of the clear phosphorus. Then also we find, side by side with this, that the particles of red phosphorus, in combining with oxygen to form phosphoric acid, evolve less heat than clear phosphorus. The kind that has got less heat in it evolves less on combustion. Again, which is more important for our purpose, the particles of the red phosphorus are less easily ignited, having a less tendency to undergo the process of combustion, than the particles of the clear phosphorus. They have a less active tendency, under the ordinary temperature, to combine with oxygen. And again, if we immerse red phosphorus in sulphate of copper, it is well known that none of the metal is reduced by it. That is a distinctive test between red and clear phosphorus. In one case oxygen is taken from the sulphate of copper, and it effects the precipitation of the copper; so that in this respect we see by comparing the two states of one and the same element that the clear phosphorus is transformed, and that the particles of the red variety are deprived of a portion of the vibration which they possessed in the state of clear phosphorus, and that the properties of the element are for the time altered so as to be less active chemically. That is a most important point which I wish to establish on that particular case; but I think that the comparison of the specific heat of analogous elements—elements which belong to one and the same natural family—may serve to establish the same conclusion, perhaps even in a more striking and practically useful manner. For instance, we will take Deville's law as an approximation to the truth, that the quantity of heat contained in each unit of weight of one element is as much less as the atomic weight of that particular element is greater, or the products of the atomic weights of each element. Let us take merely the case of some elements belonging to one well-characterised natural family, and which are perfectly analogous to one another. Take, for instance, the natural family of chlorine, iodine, and bromine. Now, Deville's law enables us to express in terms, by the thermo-dynamic theory, that an atom of chlorine which is light has the same momentum—the same actual mechanical force in each atom—as an atom of bromine or iodine; and, accordingly, if these atoms are moving in a medium with no resistance, the velocity with which the atom of chlorine will move must be as much greater than the velocity with which the other atoms are moving, as its atomic weight is less. Thus Deville's law, if it be true, is merely another statement of the fact.

And now let us see how a comparison of the chemical properties of these elements will bear upon our previous notion. The theories we were just now reviewing had regard to the effect upon affinity of rapid motion compared with the effect of less rapid motion. Let us ask ourselves which of these three elements—chlorine, bromine, or iodine—is the most active chemically. I will take one particular case of the relative action of chlorine, bromine, and iodine in combining with one and the same element—hydrogen. It is well known that chlorine combines more powerfully with hydrogen than bromine does; and that bromine, in its turn, combines more readily than iodine does, and it has been shown by a measurement of the quantity of heat evolved in the respective combinations that chlorine evolves a great deal more heat in its union with hydrogen, than bromine, and bromine more than iodine. We should accordingly, in the case of their decompositions, say that chlorine has the most affinity for hydrogen, and iodine the least, and we should put at the top of the series, as regards affinity, the element which has, as I just now said, the greatest velocity of motion. I say that Deville's law leads to the confirmation of the result which we have arrived at before, that the body which has the greatest affinity, evolves the greatest amount of heat. For it has been shown that the quantity of heat evolved when chlorine unites with hydrogen, is greater than when bromine unites with hydrogen; and that when chlorine displaces bromine a certain quantity of heat makes its appearance.

Now, amongst the other considerations which may reasonably be referred to in order to obtain a confirmation, or verification, or authentication of these results, I may refer to the process of diffusion, in which particles are left to themselves with as little disturbing force as perhaps under any experiment one can refer to. Now, it is well known that in diffusion the process amounts generally to nothing else than what I have just now affirmed to be the normal action of particles upon one another. Suppose we put into a jar a quantity of hydrochloric acid, and fill up the jar with water, we find that the hydrochloric acid, although heavier than the water, gradually makes its way to the top of the jar, and the water goes down in the place of it. But we really do not know, it having never been proved, that the hydrogen of the hydrochloric acid goes up, and that the hydrogen of the water goes down. All we can say is, that chlorine goes up and oxygen goes down. The fact stated in its simplest terms amounts merely to an interchange of constituents—an interchange of the chlorine for the oxygen without the aid of any external force whatever. Now, it is well known, from the researches of Mr. Graham, that after measuring the quantity of hydrochloric acid which diffuses itself in a given time, if a similar experiment be made with hydrobromic acid, it diffuses in about the same time; and if in a third experiment hydriodic acid be taken, and the quantity diffused in the same time, under the same conditions, be measured, it is found they are just about equal. Now what does that amount to? In equal weights of hydrochloric acid and hydriodic acid the hydrochloric acid diffuses as many more atoms than the hydriodic acid as its atomic weight is less than that of the hydriodic acid. The atomic weight of chlorine is 35½, and that of iodine is 127; 35½ would go between 3 and 4 times into 127; so that there are 3 or 4 times as many atoms of chlorine diffused as there are atoms of iodine. Bromine is 80; so that there would be about 2 atoms of iodine for about 3 of bromine. In fact, we find that those elements which we had reason, from other considerations, to consider as possessing the most rapid motion, actually do move out quickest of the three particles we have been considering, as we had reason to believe from Deville's law. The observations of the velocity in other cases of the same beautiful phenomena lead us to results which are accordant with this provision. I do not mean to say that I could explain all the results which have been observed, by this or any other theory; yet there are striking coincidences of the kind, and they consist of the cases of simple and well-known compounds which can hardly be supposed to be accidental.

Now, when we are judging of the force with which bodies unite, we generally are inclined to think that those elements which, on combining, evolve a great amount of heat are uniting more powerfully than those which, on combining, evolve less heat; that the disturbance which is produced by the intensity of the heat which is evolved on combination, does suggest itself to the mind as the measure of the amount of force with which the combination takes place; and, more than that, I think that the notion of chemical affinity itself usually carries along with it this further idea—that the more completely the product of any particular combination has lost the properties of the bodies of which it was formed, the more inert it is, and the less disposed to re-act in any way whatever: the more powerfully do we consider the elements of that body to be held together.

I will now describe the results of the experiments with specific heat. We should conclude from what I have just said, that those bodies which, on combining, evolved much heat, would form a compound containing less heat than others, which, on combining, evolved less heat. It would seem that the greater the evolution of heat on combustion, the less would be the specific heat of the product compared with that of the constituent bodies. We ought to find, when bodies unite with great energy, and when they form a product in which the elements are held together with great force, and great heat is evolved, that the product will have a much less specific heat than the original bodies. And, now, upon this subject I am really at a loss to know what to say. I have looked carefully and repeatedly over the results of the observations on the subject; and I have found many cases which certainly agree with this notion, but also many that do not. There are many determinations from very competent authorities of the specific heat of compounds and determinations of the specific heat of the substances from which those compounds are formed, in which it is found that the products actually contain more specific heat than the original constituents. Now, I conceive, in the first place, in order to compare properly and fairly the product, for instance, of baryta and sulphuric acid—sulphate of baryta—one ought to take it as formed by a specific process. One measures the quantity of heat which is given off when sulphuric acid combines with baryta, but we ought to take its specific heat and compare it with the specific heat of the different bodies. Experiments of this kind in direct relation have not, to my knowledge, been as yet made on any extensive scale; but, certainly, I think I am not overstating the case when I express my belief that in some of the best examined cases an agreement of the kind occurs. For instance, amongst the compounds of sulphur with metals, numerous instances are found in which the sulphides have had very considerably less specific heat than the sulphur and the metals before combining; and I think they are, perhaps, the most fair cases. Sulphur and the metal can be weighed, and the product can also be weighed in a solid state, and there is a great resemblance between the elements and the compound. I think, certainly, that taking the specific heat of oxygen and a metal, and measuring the heat evolved when the metal is burnt in oxygen, and then taking the weight of the solid oxide is hardly fair, because the change of state is hardly consistent with a direct comparison of the substances. However, although many cases of chemical combination do appear to agree as far as regards the capacity for heat of the products, in the manner which is stated in the process I have been submitting to you, there are some cases which not only do not agree with it, but which, I conceive, ought not to agree with it. There are certain bodies obtained by means of indirect action. Take, for instance, the case of anhydrous nitric acid, which everybody knows cannot be got originally from oxygen and nitrogen. Anhydrous nitric acid, which we have every reason to believe ensues, has more heat stored up in the particles, than the oxygen and the nitrogen contained in them when uncombined. Now, it is well known that this particular body, anhydrous nitric acid, by a very slight disturbance of the equilibrium of its particles, undergoes a violent explosion, in which heat is evolved, and free oxygen and free nitrogen liberated; so that the indirect process by which the bodies were persuaded to enter into that particular combination, may, in each case, have been accompanied by an evolution of heat; but the circumstances under which we afterwards examine it being totally different, the alteration of specific heat enabled it to retain a greater quantity than the elements in their free state.

Take as another case the compound hydriodic acid. According to Messrs. Favre and Silberman when elements unite without the presence of water, you would actually need to add heat to the free iodine and the free hydrogen. It is known that the compound when formed, especially by itself, is extremely unstable, and readily gives off its hydrogen. The elements separate with such extreme facility that they are evidently not held together with great force, and such cases as that rather afford a confirmation of the case in which it is generally found, that when bodies contain less heat they are then moving more slowly, and are more inert, and have less mechanical motion in their particles, in accordance with the notion with which we started. But in the consideration of chemical force of combination, without more exact data than any we have obtained, such cases as those which I have had the honour of submitting to you are unsatisfactory at the best. No general notion can lead to any accurate scientific results. I do not mean to say they may not have their uses in order to classify bodies in a particular way, but the only process which I know by which one can get anything like a quantitative comparison of the relative forces with which different elements unite, is by the working of that beautiful machine—the galvanic battery. I know of no other process by which we can in so easy and certain a manner compare the forces which are at work in different cases of chemical combination. In order to avaid too remote an analogy with the actual process, and in order to express the matter in exact terms, I ought to classify chemical action a little, in a manner that I think will be readily admitted to be applicable to it. For instance, when we have the case of the oxidation of a metal by nitric acid, as when we throw copper into nitric acid, it is said that is a process of oxidation. It is true enough that the copper is oxidised, but it is equally true that it is a process of de-oxidation; because we have to think of nitrogen as well as oxygen. And so in the greater number of other cases chemical action consists, either directly or indirectly, of two processes. The case of zinc and sulphuric acid might be said to be one of combination, which happens when the zinc is thrown in contact with the sulphuric acid and combines with oxygen; but you might equally say that it is also a case of liberation of the hydrogen. We will take the case of a battery of a very simple kind. [The section of a battery was drawn upon the black board.] We will take merely a plate of zinc and a plate of platinum in it. In this cell I have some hydrochloric acid. This shall form the chain between the zinc and the platinum, and there is my connecting wire. In this ideal battery all the hydrogen is being evolved from the water. We have combination of the zinc with the chlorine; a combustion is going on there. On the platinum plate we have the opposite process—the separation of chlorine and hydrogen. If we were to call this process of the combining of the zinc "burning," we should call the separation of the hydrogen, to use an un-English expression, "un-burning." Now, if we follow out the appearances which present themselves in special relation to heat, we find that the process in its general features is not accordant with what we know. For if we burn zinc in chlorine, we get a very large quantity of heat evolved, which we do not find makes its appearance on the surface of the zinc plates in the battery. There is not that amount of heat evolved, although there is no doubt that the chloride of zinc is forming; and on the other side where hydrogen is being separated from the chlorine we do not find that quantity of heat absorbed, or cold produced, by this un-burning. Everybody knows there is a frigorific mixture on this plate, and combustion, or the means of producing heat, on the other plate. All the heat we get produced is the difference between the zinc which is combining, and the hydrogen which is being uncombined. For instance, to take Favre and Silberman's determination of the heat in the presence of a large quantity of water; of hydrogen combined with chlorine: we will call the heat 40,000, which is evolved when 1 gramme of hydrogen unites with 35½ grammes of chlorine, when one equivalent of zinc unites with chlorine we have 56,000 degrees of heat, and it is well known from the experiments of Mr. Joule and Mr. Wood that if you measure the total quantity of heat which is evolved in the whole battery—I mean in the connecting wire and the battery altogether—measuring the heat which is produced by the simple resistance which that wire overcomes, let it do what kind of work you like—if you measure it you find it is exactly equal to 16,000 degrees of heat, which is equal to the heat which the zinc evolves in combustion, minus the heat which the hydrogen being evolved absorbs on being separated from its compounds. Now, the question arises how, in the arrangement of our machine, the heat which the hydrogen wants in order to be evolved is communicated. The zinc has evolved 56,000 degrees of heat, but the hydrogen wants 40,000 of them. It is a question how the hydrogen gets that quantity of heat. It is well known that in the liquid which connects the two plates of zinc and copper there is a certain motion of the particles,—that when the chlorine which is next to the zinc has combined with it, and chloride of zinc is formed in that way, there is an atom of hydrogen left next the zinc. When an atom of chlorine combines, chlorine again takes the place of the chlorine which has gone out, so that the process we had at first re-establishes itself, in fact, in the liquid which connects the positive and negative metals. We have a constant change of places of analogous elements; we have, moreover, a number of properties, which we describe in general terms as a current of electricity passing through the liquid; and we have, besides, a perfectly similar appearance in the wire which connects the plates themselves—there is a current of electricity passing through the wire exactly as much as the current of electricity passing through the liquid. Of course the state of the particles in the wire is the same as the state of the particles in the liquid, and produces the same effect; and there must be a state of motion in the particles of the wire of the kind that we know to occur in the particles of the liquid. We cannot distinguish one atom of copper from another atom of copper to see how they are moving; but in all the cases we have an identity between the effects produced in the liquid itself; and it would be certainly unwarrantable, as far as I know, to assume that a different process occurred, so as to produce this identity of effects. So that the result comes to this, that we consider the connecting wire as serving to carry over from the zinc which is combined with chlorine, to the hydrogen which is uncombined, and separating, to carry over all the heat which the hydrogen needs in order to be set free, and constitute free hydrogen; and that the particles of the wire are in a state of vibration which is communicated, by waves, from one particle to another—from the zinc, which is burning, to the hydrogen, which is un-burning. Now, in the battery it is well known that by interposing wires of different resistance, we can measure the forces which are at work; and I think you will agree with me, that what occurs is precisely identical with those processes which occur if a lump of zinc is thrown into some hydrochloric acid. Hence we have free hydrogen being evolved over certain points of its surface, and there is no doubt whatever that when those points are less active they evolve hydrogen more easily. There is no doubt that on the surface of a lump of zinc there are little local galvanic currents occurring, the only difference being, that in this beautiful arrangement we keep all the combining process in one portion of the apparatus, and the separating process in another, so that the one cannot take place without the other; and therefore by interposing different resistances we can operate with the most beautiful perfection, and measure, with the most wonderful accuracy, the force, by the amount of resistance which is overcome. I conceive there is no test for the measurement of forces to be compared with the amount of work which can be done. There is no doubt that where a body which is combining evolves very much more heat than the body that is separating absorbs, then great resistance can be overcome; and the force by which the one element displaces another may be measured by the quantity of heat which is evolved in the displacement. I believe, in stating that, I am not advocating any hypothesis whatever, but merely giving well-established results from the best observers.

(To be continued.)

NOTICES OF PATENTS.

Salts of Alumina. T. Richardson, Newcastle-on-Tyne.

The ideas which gave rise to the former patents of this chemist for the use of sulphurous acid, are pushed still further in another patent, in which the action of the same acid upon aluminous minerals is claimed.

Artificial Fuel. J. Inglis, Glasgow.

There have, perhaps, been as many silly processes patented for forming artificial fuel as for any other thing whatever. Mr. Inglis's patent takes an unenviably distinguished position among them. He mixes excrement, "human or animal," with any kind of wood in the form of chips, saw-dust, or shavings, and coal-dust. Fortunately for society, this patent was not proceeded with. It is probably, therefore, a failure in practice.

Improvements in the Manufacture or Production of Nitrate of Potash and Soda, and in the Application and Use of Certain Products of such Manufacture in the Manufacture of Soap. W. Anderson, Glasgow.

The patentee prepares nitre by means of the well-known double decomposition which ensues between potashes, or any salt of potash, and nitrate of soda. The nitrate is not procured directly, but by evaporation of the spent liquors obtained in the manufacture of soap with a mixture of potashes and nitrate of soda. The nitre is obtained by successive evaporations, until, at last, carbonate and caustic soda begin to deposit. The final mother liquor contains much caustic soda, which may be used for the preparation of hard soap or any other purpose for which soda, or soda ash, is applicable. One of the improvements claimed is the conversion of the sulphate of potash contained in the potashes of commerce, into nitre. This is effected by double decomposition with the nitrate of lime, obtained by neutralising with lime the nitric acid washed away from palm oil after bleaching it with nitric acid.

Evaporating-Pans for the Manufacture of Salt. J. Corbett, near Bromsgrove.

The patentee makes the evaporating-pans of cast instead of wrought-iron plates. The castings are constructed with flanges to enable them to be bolted together. A trough or depression is also claimed running either all round, or only on two sides of the pan. This trough is for the same purpose as the depressed end of common salting down pans, namely, to allow of the salt being raked together as it deposits during evaporation. The patentee also claims, both in cast and wrought-iron pans, making a gradual rise from the ends and sides to the middle.

Manufacture of Cyanides of Barium and Strontium. W. Clark, Chancery Lane, London.

This is a patent for the cyanuration of barium and strontium in presence of carbon. The process is precisely similar to that by which cyanide of potassium has often been obtained. The patentee mixes carbonate of baryta with sawdust and charcoal, and, after placing it in a tube heated to redness, passes nitrogen gas over the mixture. The current of gas is kept up until the mass is cold. On treating with water the contents of the tube large quantities of cyanide of barium will be obtained. We do not think this is likely to be a profitable method of obtaining cyanides. The patentee does not seem to be aware of the researches which have been made by numerous chemists of eminence upon the action of atmospheric nitrogen at high temperatures upon mixtures containing carbon, iron, and alkalies.

CORRESPONDENCE.

Magenta Dye.

To the Editor of the Chemical News.

Sir,—I enclose you a small sample of Magenta powder which is soluble in water. When dissolved it will dye either silk, wool, or cotton, without any mordant or assistant. If you will go to the trouble of pouring a little hot water on the powder, and then pass the solution through a piece of cotton (so as to separate the insoluble matter), then dip a little silk or wool in the liquor, you will find the same dyes a beautiful shade and about the brightest in the market. I have made this on a new principle.

The French have sent over here a paste Magenta, which will dye silk, but not wool, and nothing near so bright as the enclosed sample. The reason I send you this is to show that a Magenta can be made in a powder, and be quite soluble in water and dye on any fabric without using any spirits of wine or methylated spirit.—I am, &c.

Magenta.

[The sample of Magenta dye has answered very well in our hands in dyeing both silk and wool. If such an article could be introduced into commerce at a reasonable price, it would, we doubt not, meet with a ready sale.—Ed. C. N.]

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—I beg leave to say that the only series of experiments I ever performed upon the adulteration of bread was made on loaves purchased at forty different shops in the metropolitan district of Lambeth; and that, in nine out of forty samples, I was unable to detect the presence of alum.

My experience in the matter of food-adulteration has led me to believe that the morality of dealers in eatables and drinkables, though not unimpeachable, is quite equal to that of professional grievance-mongers and dealers in mares'-nests.—I am, &c.

Wm. Odling, M.B., F.R.S.

Kennington-road, London, S.

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on April 18, when a discourse will be delivered "On the Application of Electricity to the Explosion of Gunpowder," by F. A. Abel, F.R.S.

Silver in Sea Water.—Captain Maury, of the American Navy, has been making some calculations, based upon the discovery of traces of silver on the copper bottom of a vessel docked at Valparaiso, and from these calculations Captain Maury concludes that at least 200,000,000 million of tons of silver exist, in an exceedingly diffuse solution, in the waters of the ocean.

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, April 1, 1861, William Rutherford Ancrum, Esq., Stephen Jennings Goodfellow, M.D., and William Newmarch, Esq., were elected Members of the Royal Institution. Rev. Charles Forster, H. G. De Mussy, M.D., Rev. A. Denny, and W. E. M. Tomlinson, Esq., were admitted Members of the Royal Institution. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same. The following is the arrangement of Lectures for the ensuing week:—Tuesday, April 16, at 3 o'clock, Professor Owen "On Fishes." Thursday, April 18, at 3 o'clock, Professor Tyndall "On Electricity." Friday, April 19, at 8 o'clock, John Ruskin, Esq., "On Tree Twigs." Saturday, April 20, at 3 o'clock, Max Muller, Esq., "On the Science of Language."

Uses of Tea.—At a recent meeting of the Society of Arts a paper was read on "The Uses of Tea in the Healthy System," by Dr. E. Smith. The author began by stating the opinions brought forward by Professor Johnstone and other chemists on this subject—views which were still very generally held. These might be summed up thus: that by the use of tea—1. The waste of the body is lessened; 2. The body is nourished; 3. By lessening waste, we may lessen supply, and yet the bodily powers shall be duly sustained. He stated his reasons for differing from these statements, and explained the nature of the experiments which he had undertaken in order to ascertain the actual effect produced by tea. If there be an abundance of food in the system, and that especially of the farinaceous or fat kinds, tea is a powerful digestive agent, and by promoting the transformation of food, it aids in nourishing the body; but with a deficiency of food in relation to the waste to the tissue by exertion, or the waste of heat by cold, or by too profuse evaporation from the skin, it wastes the tissues of the body, and lowers the vital powers. He, therefore, considered that it was not suitable as a partial substitute for food, as was generally supposed; for it increased the waste of the body.

ANSWERS TO CORRESPONDENTS.

*** All *Editorial Communications* are to be addressed to Mr. Crookes; and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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Tar.—We intend to give a report of Mr. Perkin's Lecture "On the Colouring Matters Obtained from Coal-tar," which will be delivered before the Chemical Society on May 16.

Silicic Ether.—We do not know where this can be obtained. It certainly cannot be got ready made, and few manufacturing chemists would undertake to make it unless it were ordered in quantity. Dr. Frankland was asked £6 per lb. for it in quantity.

T. W. B.—We never heard the name of the gentleman you mention.

C. R.—1. We do not know at present; but any process would answer if devised by a skilful chemist who knew what he was about. 2. No attempts have been made except as experiments. 3. It would: but not in sufficient quantity to pay for the battery power employed.

PULVIS JACOBI *VER.*, NEWBERY'S.

To the Medical Profeſſion of Great Britain and Ireland.

GENTLEMEN,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports,*" *vol.* 1, *p.* 317.

> "Since this caſe came into my poſſeſſion, I have been led to make a good many clinical experiments, which have enabled me to verify the reports which I had heard of the efficacy of JAMES'S POWDER, in ſometimes removing the APOPLECTIC DIATHESIS in perſons advanced in life. It is, moreover, worthy of obſervation, that JAMES'S POWDER has been of remarkable utility in certain inſtances of DETERMINATION OF BLOOD TO THE HEAD, which occurred at an early period of life, and threatened to end by effuſion. Laſtly, in two caſes of GENERAL PLETHORA, in which, however, the head was more affected than any other part, JAMES'S POWDER was exhibited by me with perfect ſucceſs."

Alſo *vol.* 1, *p.* 320.

> "The following very ſimple method of exhibiting JAMES'S POWDER, in caſes of undue determination of blood to the head, is that which I have generally purſued. The patient is made to begin with a very moderate doſe, not more than two grains at bedtime, and to increaſe the doſe by half-a-grain every night, until ſome ſenſible effect is produced upon the ſtomach, bowels, or ſkin. Should the ſtomach be affected with ſickneſs, the doſe muſt be leſſened by one grain on the following night. By the addition of a little rhubarb to it, a larger quantity of JAMES'S POWDER may be adminiſtered than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen or twenty grains taken for a conſiderable period without any inconvenience."

The following extract (ſhowing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet,*" *June* 16, 1860, p. 590,) as follows:—

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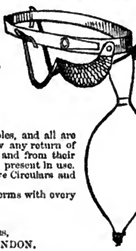

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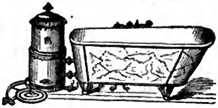

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THE
CHEMICAL NEWS
AND
Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

No. 72.] Saturday, April 20, 1861. {Price 4*d.* Post Free 5*d.*

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THE CHEMICAL NEWS.

VOL. III. No. 72.—April 20, 1861.

TECHNICAL CHEMISTRY.

On the Adulterations of Food, &c., in Melbourne, *by* WILLIAM SIDNEY GIBBONS.

I SUBJOIN a few notes of observations that I have made from the year 1852 to the present time, on articles of food largely consumed and commonly adulterated. In the present paper I shall therefore confine myself as much as possible to cases that have come under my own notice in Melbourne. The first case of contamination that I remember to have noticed here was the presence of copper in some raspberry-jam, imported from Hobart Town in 1852. The contamination was obviously accidental, and the risk of its recurrence is reduced by the extensive use of enamelled iron preserving vessels. I remember, however, that the announcement did not appear to awaken any anxiety, but, on the contrary, seemed to be regarded as something very funny by the audience to whom it was incidentally mentioned in a lecture.

Flour.—I find in my notebook a record, bearing date November, 1856, of the examination of several samples of flour at one time. They were eight in number, and of the eight, four were notably adulterated with plaster-of-Paris. Many cases occurred about the same time of the seizure of flour, which, after examination, was condemned as unfit for sale, the vendors being punished whenever the act could be brought home to them. Most of the flour so condemned was musty, or otherwise deteriorated by organic change. About the same time a somewhat singular circumstance occurred which is worthy of mention in this connection. I had been lecturing in a suburb, not then famous for the production of good bread, on the chemistry of bread. After discussing the various phenomena connected with nutrition, with bread-making, &c., I proceeded to illustrate the most familiar adulterations by actual experiment, and by the citation of cases. When I concluded, some local bakers, virtue impelled, rose to say a few words for the credit of the craft. One had been in the trade for twenty-seven years and had never heard of alum, another was all innocence on the subject of potatoes, and a third was horrified by the bare mention of plaster. I was about to disclaim all personal allusion, as I did not know at which of their shops my sample had been bought, when a gentleman in the body of the crowd rose, and having premised that he was unconnected with the "mystery," told the following pertinent anecdote without comment:—"He had imported a cargo of whitening, an article in great demand among the makers of effervescing draughts, but unfortunately for him his shipment arrived in cold weather, and was stored at a cost almost beyond its then value. There it remained for some time, 'eating its head off' in the store, for there was no hope of a rise until the next spring; when one day, to his surprise and delight, the broker in whose hands it was placed, called and reported the profitable sale of the lot. 'What on earth occasions the demand at this season?' inquired the delighted vendor. 'Flour's up,' was the laconic reply."

Bread.—Although I am willing to believe that bread is less injuriously, and less extensively doctored here than in London, it is often far from being what it professes. Damaged flour and deteriorated grain are certainly sold specially for the behoof of fowls and pigs, and contribute their share to the prevalent diseases among the former, but they nevertheless often find their way to the table. I have lately had some samples of bread under examination for fungi; but although I could have taken a hint from the oft-quoted Dauphine, and have eaten pastry, rather than such bread, I was not sufficiently satisfied of their presence to feel justified in condemning it; as while the conditions for the growth of fungi subsisted, the plants themselves were not to be found. I have already made mention of potatoes as a common, I may say, a general adulteration of bread, not in the form of potato-flour or starch, but entire or simple boiled mashed potatoes. The fraud here is the substitution of a less for a more nutritive article, and one too that holds a larger quantity of water. The same objections, with others on which it is needless to enter, apply to the use of rice.

Spirits.—To the liquors sold in Melbourne I have had opportunity of giving special attention, and I have notes, the publication of which, with names and addresses, would probably astonish the persons to whom they refer. It must not be supposed that liquors sold over bars are the same as those delivered into cellars, or from bonded stores. Such little trifles as water, salt, alum, burnt sugar, frequently enter into the composition of the nobbler, and one or other may almost always be found. Were it otherwise, there would, probably, be a much larger amount of drunkenness produced with the expenditure of smaller sums of money. People could get "wholesomely" drunk too soon upon spirits of the full strength, and the most profitable kind of trade—viz. tippling, would run a risk of being curtailed; while if the diluent alone were added—and I believe that this may be the only addition in some few of the best houses—the reduction could not be carried to so great an extent as is common. To take an example, one of many, over the bar of a house that shall be nameless, one of a class, a spirit (rum) was sold, which was within a fraction of forty degrees of proof below an average sample obtained from a respectable wine merchant, and employed as a standard for comparison. The solid constituents were very little reduced, and the extractive matter, including the gum, sugar, &c., were but little less. How then had the change been effected? The strength had been brought down by water, the colour, and part of the flavour restored by burnt sugar, and other, perhaps less agreeable, organic matters, and the roughness which might tickle unrefined palates was imparted by alum, the constituents of which were present in larger quantities than could have been attributable to the water. In the matter of brandy, less consumed, perhaps, by the class of customers thus played upon, I found less doctoring of the sort described. Brandy is even more exposed to doctoring than rum, but to do it well requires rather more nicety, and the ingredients are not of the same rough and ready kind. In the series of experiments to which I refer, I find a brandy fifty-three degress below the assumed standard. In this instance there had not been any addition of alum or other mineral astringent, probably the water was almost the only adulteration, the use of some highly concentrated astringent, such as catechu, being alone admissable under the circumstances. Neither capsicum nor tobacco were found, nor had any mineral acids or metallic salts, except common salt and alum, been added to any of the spirits examined. The advance in price was the same as in the former case cited, but the bulk was doubled, so that the profit bordered on the fabulous.

Beers.—In the beers it was more difficult to discriminate between the work of the English and the Colonial brewer, and to determine the publican's part in the doctoring. I believe, however, that his share is limited to collecting all the drains of the counter, and the other beer-engine waste flow into a butt, whence it returns in combination with original beverages, and to drawing from two butts. The latter operation has place more particularly with ale, the pipe going down from the engine is branched, one end going into the English brand, to whose name it answers, and the other into some colonial abomination; or, perhaps, into the *omnium* before mentioned, though the chief use of that is, no doubt, for the ready production of half-and-half.

Confectionery in its most artificialised forms is a fruitful source of disease; and well it may be. The most noxious types of those really intended for eating (though many ornamental devices not addressed to the palate yet find their way into the stomachs of children) are the comfits of dead white, body coloured externally. In some of these I found the following colouring matters: the red was a harmless organic pigment, one of the lakes commonly used; the yellow was the poisonous chromate of lead, and formed a thick coating on the surfaces of some of them, while in others the pigment was diffused; the same deadly paint was used in combination with the red to produce orange; the blue was ultramarine, and the same pigment faced with red furnished a purple. The white opaque mass, though uninviting enough, was nearly all sugar, and did not contain any plaster-of-Paris or other earthy matter.

Coffee.—Perhaps the most remarkable example of local adulteration and sophistication is coffee. I long regarded it as the sole case of the kind that demanded notice. The question "What is coffee?" should now have for its reply, "Coffee is a manufactured article, varying according to the products of the country in which it is made; in Melbourne it consists in part of the berry whose name it bears, and in great part of gram, maize, peas, roasted corn or potatoes, and chicory." The case of coffee is a very singular example of the gradual perversion of public taste, by means of gradually increasing adulteration. Formerly, in England, coffee in its purity was a staple commodity, its flavour was well known and was highly esteemed. Presently chicory was introduced with specious and false recommendations as an improver, an economiser, &c.; then those grocers who had before used it increased the proportion to keep pace with the times, and before long the coffee with an admixture of chicory, might be called chicory with an admixture of coffee. The chicory then became a mixture, as it is now, and the diluting ingredient required a disguise in the shape of colour; and the coffee mixed with mixtures became a very indefinite compound, and needed all sorts of sophistication to correct flavour and colour. At this point, then, when coffee is merely a subordinate ingredient in the compound, begins the history of our adulterations, and the Melbourne manufacturer avails himself of the state of the public palate to acclimatise the usages of the London market. The state of the markets dictated whether maize, or gram, or peas, or potatoes should be the staple, and they were flavoured and coloured with chicory, enough coffee being added to tone the whole, to impart a modicum of their essential qualities which are possessed by coffee alone of all the ingredients, and to satisfy what served the artist in place of a conscience.

A few words in conclusion. Without intending to defend the traders who either adulterate or sophisticate our food, I must distinctly throw the blame upon the consumer, who is the cause. No one who goes out of his way to a cutting shop, or who endeavours to purchase an article at a less sum than it can remuneratively be produced for, has any right to complain of adulteration, nor is he entitled to raise a cry against sophistication, if he insist on an ideal standard of colour or texture. A manufacturer of genuine mustard, then almost the only one, tried for years to get his mustard into the English market, but was always at a disadvantage. It was of a dingy-brown colour, whereas the sapient public would not be content with anything but bright yellow. Of course, said public was accommodated by the help of starch and turmeric. And so it will be with everything else. If people set their minds on having coffee at ninepence per pound instead of eighteenpence, grocers would be found to supply the article.—*Dublin Medical Press (from the Australian Medical Journal).*

Natural Oxide of Silver.

We extract the following remarks on the above subject from the *London Review*:—

No little excitement has been caused by a recent announcement in the *Times* of the existence of silver, in the form of oxide, in the neighbourhood of the ordinary metallic silver deposits. The result being, first, that from a ton of ore, in its natural state, where the normal average of yield of the precious metal would be 13 ounces, by the new process an average of 113 ounces of silver would be obtained, the overplus being derived from that which has hitherto been rejected as useless; and, second, that the cost of this new method of reduction is relatively so small as to render the expense but little more than nominal. The letter in which this announcement is made prefaces it by stating that, up to within a very recent period, the existence of such a thing in nature as oxide of silver has been denied by the most eminent chemists, but that science has now demonstrated that it does exist in enormous quantities; and the writer concludes by saying that the importance of this discovery to our own mines, where silver is often found in close approximation to copper and other metals, will be very great, but to some of the foreign ones, where the deposit is large and unmixed with other metals, the result, in the shape of returns, will be almost fabulous. This letter was evidently intended merely as a preliminary to a longer one, but still anonymous,—

from "R. G.,"—in which more detail is given, and the happy locality hinted at. Like the former one, it commences with the assertion that scientific men, of the highest chemical attainment, either still wholly deny the existence of natural oxide of silver, or assert that if it does exist it can only be extracted in such small laboratory experiments as to render the discovery commercially valueless. The writer then proceeds to confirm the previous statements, asserting that Cornish gozzans, which, in their natural state, give, upon assay, from 5 to 13 ounces of silver per ton, yield, after the new method of treatment, from 54 to 216 ounces, whilst some have been found to be fabulously richer. Taking into consideration the great value of labour in England, it will not pay to work ores containing silver only in less quantity than 10 to 12 ounces per ton. It follows from this that the lowest yield, according to the new statement, 54 ounces, would bring in an immense profit. The expense of raising, reducing, and crushing these neglected ores could not exceed 3*l.* per ton, whilst the value of 54 ounces of silver would be over 13*l.*, and that of the average yet obtained, per ton, over 35*l.*

These statements are very extraordinary, and deserve the careful attention of every one interested in the development of England's mineral wealth; whilst the fact of their publication in the *Times* renders it doubly important that they should not pass unchallenged by men of science. Divested of all theoretical conclusions, they simply amount to this, that certain silver ores, which yielded, according to the ordinary mode of assay, from 5 to 13 ounces of silver per ton, were shown, by another method of assay, to contain from 54 to 216 ounces. The conclusion is, therefore, jumped at that the cause of the discrepancy between the two assays is, that the lower numbers represent silver in its ordinary state, whilst the higher figures show what may be obtained by a different mode of working; the increased amount of silver arising from the presence of the precious metal in some mysterious combination with oxygen, the existence of which compound in nature the public are inferentially led to believe is only just beginning to be admitted by the most eminent chemists, and we are to assume that the product, oxide of silver, although present in the ore in such enormous quantities, possesses so remarkable a disinclination to give up its metallic constituent under ordinary chemical treatment, that the usual methods of assay are quite inadequate to detect its presence.

Now, we do not for a moment intend to dispute the fact that valuable silver ore has been and is habitually thrown aside as worthless; such a thing only coincides with every day experience. Every new discovery in the metallurgical or mechanical treatment of ores, every new chemical fact which is made out in reference to the precious metals, is liable to be followed by the discovery that millions of tons of mining refuse will now "pay" to work over again. Not long ago, the adoption of a very simple suggestion of a practical chemist was the means of very considerably increasing the yield of gold in one of the Australian gold-fields; immense quantities of "tailings," the accumulated refuse of many years, being by this means found to yield nearly as much gold as the original ore. A similar instance occurred in our own iron districts. One of the best and richest ores was for many years cast on one side as valueless, until a metallurgical chemist pointed out to the ironmaster that he had under his feet unlimited quantities of iron ore better than what he was putting himself to a considerable expense to bring from some distance to his furnaces. Again, not many years ago, a clever mineralogist showed that gold was very widely distributed in various rock and strata in the British isles, and published analyses to show that in many localities it would pay for its extraction. We are, therefore, quite prepared to believe that the same holds good with the less precious metal, silver, but we must really take exception to the unscientific manner in which the announcement has been made public. In the first place, why is it assumed that the silver exists in the form of oxide? From the pains taken by the writers to set aside the universal opinion of chemists, they seem to admit that this is a weak point of their argument. Oxide of silver can be prepared artificially with the greatest readiness; its properties have been studied and experimented upon over and over again, and the opinion which chemists have in consequence arrived at, that it is an extremely improbable substance to be found in a mineral state, is confirmed by the fact that no work on mineralogy mentions such a body. It is one of the most unstable bodies ordinarily met with; simple exposure to the air, to rain, to light, or to heat, at once cause its decomposition, or passage into something else,—carbonate or chloride, both known minerals. When we add to this that it would require the utmost refinement of chemical skill to be able to pronounce definitely as to its presence in so heterogeneous a mixture as Cornish gozzan, it will be seen that we require more evidence than a simple anonymous assertion to credit its existence.

The inference which we are led to draw from the discrepancies between the former and latter assays also deserves comment. To a properly qualified person, the assay of a silver or any other metallic ore, and giving the theoretical yield per ton, would present no more real difficulties than the act of counting the books in a bookcase, and giving a correct return of their numbers, would to an intelligent child; whilst the methods adapted by the assayer are so searching that the occurrence of other bodies in combination with the silver (whether it be as oxide, chloride, bromide, sulphide, &c.) can no more interfere with his results than can the different coloured bindings cause a false return of the number of books to be arrived at. The fortunate possessor of these Cornish gozzans evidently had the first assay made by a person who was not qualified to perform such an operation. He now finds that his mineral is richer than he expected; but what need was there to trumpet the discovery in the *Times*, prefacing it with the remark, that whereas *eminent chemists* denied the existence of such a thing in nature as oxide of silver, *science*, on the contrary, had demonstrated that it does exist.

New Discovery in Printing with Mauve, Magenta, &c.

We have recently been shown some extremely beautiful specimens of muslins printed by a new process in mauve. The bloom and lustre are superior to anything we have seen; but perhaps the most remarkable fact connected with it is, that it stands boiling, soap, and violent rubbing fully as well as the fastest madder purple. The process is also applicable to Magenta, and—which, perhaps, shows its universality of application better than anything else—it may be used for the new chrome green: moreover, almost any number of colours may be combined in the same pattern with as much ease and success as in working them separately. One great feature in this process is, that the whites are retained of the utmost purity, whereas by the dyeing process, with gallic acid and tin, they are by no means all that can be desired. From what we have been able to gather, it is evident to us that the albumen, gluten, and dyeing processes for mauve will be quite put on one side as soon as this important discovery becomes properly known. We have not been able to ascertain as yet the nature of the process, which, we believe, has, as yet, only received provisional protection; as soon as the patent is completed we shall hasten to lay a criticism upon it before our readers. We may perhaps mention that it is rumoured that the discovery originated at Messrs. W. Miller and Sons' print works, of Springfield, near Glasgow.

THE STOCKPORT WATER SUPPLY.

The Stockport Water-works Company have tried another action against some proprietors of print-works who discharge their poisonous refuse into the Company's source of supply. We may at once express our surprise that it should ever have been found necessary to bring such an action. The introduction of arsenic into the process of calico-printing is of but recent date. The town of Stockport has been supplied with water from the present source for five-and-twenty years. It seems to us, then, more than astonishing that, setting aside the legal rights of the case, a manufacturing firm of great wealth should turn their refuse liquor, known to be strongly charged with a virulent poison, into a stream which they were well aware yielded the supply of water for drinking purposes to a populous town; and our astonishment as chemists is not at all lessened by the knowledge that the means of removing the poison before the liquor is discharged into the stream is obtainable with little trouble and at small cost. It is satisfactory to know that the defendants in the former action have adopted a plan which perfectly succeeds in arresting the poison; and there can be no reason why the Messrs. Potter, and the owners of other print-works on the stream, should not do the same. They have only to take the advice of a skilful engineer and a competent chemist, and they will soon be taught how they may treat their refuse so that it may be discharged into the river without a trace of arsenic.

The plaintiffs in the action are evidently not animated by any vindictive feeling: they offered to refer the case to arbitration, but the offer was declined, the defendants resting their case on a techincal objection to the right of the plaintiffs to take the water, which they admitted was to some extent contaminated. All this is very well in a legal point of view; but there is a higher point of morality involved in these cases than can be adjudicated upon in a court of law. We have before urged the dangers which may arise from proving the ubiquity of arsenic. It greatly increases the probability of that poison being selected for criminal purposes when a ready defence is suggested that the presence of arsenic in the body of a person supposed to be murdered may be accounted for by its presence in the water the deceased habitually drank. Mr. Grove would no doubt skilfully avail himself of this fact, if at the next Chester Assizes he should be called upon to defend a prisoner accused of poisoning, and he would be quite justified in doing so.

Let us hope, then, that we shall hear no more of these actions. Stockport may be a very healthy place, and it is quite possible that a much larger quantity of arsenic than was found in the water taken at the George Inn may be perfectly harmless. But we would still contend that to allow the minutest quantity to pass into the water supply of a large town is a criminally negligent act, and we shall be glad to hear that the Messrs. Potter have taken, and acted on, the opinions we have suggested, in which case they will have the consolation of knowing that they have obeyed the injunction which commands us to "avoid the appearance of evil."

The following is a condensed report of this trial:—

At the Chester Assizes, last week, an action was tried, brought by the Stockport Water-works Company, against Messrs. Potter and others, the owners of certain print-works on the Mersey, or its tributaries, for throwing certain noxious substances or poisons into that stream, and so polluting the water, to the detriment of the plaintiffs' property. The case against the defendants was substantially the same as against Messrs. Turner, Norris, and Co., in the action tried last year, and reported in the Chemical News, Vol. ii., p. 111.

In his opening speech for the plaintiffs, Mr. Collier suggested that the matter should be referred to some competent and disinterested individual, who should visit and examine the works, and then decide what should be done to abate the nuisance, and the plaintiffs would abide by his decision. Mr. Grove, on the part of the defendants, refused to accede to this proposal, and the case proceeded; the trial lasting four entire days. The chemical evidence for the plaintiffs was much the same as in the former case.

Dr. Lyon Playfair said he had tested samples of water taken above and below Messrs. Potter's works for arsenic. Above the works there was none. The stream below the works contained arsenic in suspension, but none in solution. Samples of mud taken above the works, contained minute traces of arsenic. In one pound of dried mud taken below the works, he found five grains of arsenic, with some lead and a little copper. The mud of the Company's reservoir contained arsenic. In water taken from a tap at the George Inn, Stockport, he had found traces of arsenic. The arsenic in the water above Messrs. Potter's would be insoluble, but that taken at the George Inn, was directly attributable to the print-works. The mud of many rivers contained arsenic.

Dr. Miller, in his evidence, confirmed the finding of the arsenic in the water and mud below the works, and the complete absence of the poison in the water above the works. In the water taken at the George Inn, Dr. Miller said he had found about one-hundredth of a grain of arsenic in the gallon. The presence of arsenic is not referable to natural causes, but immediately referable to the print-works.

Mr. Crace Calvert had examined the water at the same time as Drs. Miller and Playfair, and had obtained just the same results. Calico printers are able to neutralize the effects of the poison before throwing it into the stream. It is now done at Messrs. Turner's works.

Mr. Bateman, civil engineer, in his examination, said that in his opinion a settling reservoir with filter could be constructed by Messrs. Potter, which would have the effect of intercepting the arsenic and other poisons. By a settling reservoir, he meant one which holds the refuse, to be periodically emptied by mechanical agency.

Mr. Grove, for the defence, after making some technical objections to the right of the Company to use the water from Nabb's Pool, ridiculed the idea that arsenic in such infinitesimally small quantity could do any harm at Stockport. It was even admitted in evidence, he said, that arsenic existed in waters which were not mineral; and the quantity existing here was so small that it might be looked on as a normal ingredient. Professor Playfair had said that there was hardly anything which did not contain arsenic, whether food or drink; but in the present case it existed in such small quantities that it was not injurious to health, as was testified by the mortality of Stockport, which was no higher than that of other manufacturing towns. It was impossible that any arsenic could get to Stockport to do harm, for it had been stated in evidence that lime, magnesia, and oxide of iron, which exist in this river in great quantities, tend to combine with it, and render it innocuous, and no more deleterious to health than ordinary sandstone. It had also been stated that so much arsenic had been found in the mud of the river; but it should be remembered that people did not eat mud, and carbonate of lime tended to precipitate arsenic. The learned gentleman then contended that on Dr. Playfair's own showing there was sufficient salts in the water to precipitate any arsenic that might find its way there, and expressed his surprise that so little of the poison had been found at the outlet from Messrs. Potter's, which showed the extreme caution used there.

No evidence was called for the defence.

Mr. Baron Channell summed up at great length, and submitted ten questions to the consideration of the jury, who, after two hours' deliberation, returned a verdict which was substantially for the plaintiffs, with forty shillings damages, leave being given to move for a new trial on the points of law.

PHARMACY, TOXICOLOGY, &c.

On Foreign Disulphates of Quinine, by Julius Schweitzer.

A notice in the *Pharmaceutical Journal* for March 1860, p. 463, about the deficiency of weight in samples of foreign quinine, drew the attention of chemists generally to this preparation; and as an expensive article offering some difficulties in analysis is readily believed to be open to fraud and adulteration, a suspicion was cast on all the somewhat cheaper foreign quinines. Having had occasion, since the appearance of this notice, to examine a quantity of recently-bought German quinine, my results proved so different to all expectation that I was led to extend my inquiry to as many different quinines as it was possible for me to collect. By the assistance of some friends I obtained original and (with one exception) unbroken samples, which had found their way into this country in the ordinary course of business, before the appearance of the article above alluded to. These comprised specimens prepared by almost all the principal quinine manufacturers of Europe—Pelletier of Paris, Thomas of Argenteuil, Jobst of Stuttgard, and Zimmer of Frankfort, to which I added that of Messrs. Howard of Stratford. All differed somewhat in colour, lightness, and crystalline structure, the appearance being in favour of Messrs. Howard's, Jobst's, and Thomas's preparation. Pelletier's quinine, while nearly half as light again as any of the former, presented by its minute broken crystals the less sightly appearance of a loose spongy mass. On the appearance of the second German sample, I can offer no opinion, as it came into my hands some time after the bottle had been opened, and it had evidently been badly kept.

In analysing these quinines, the correct weight of the whole samples was first ascertained, and then the amount of moisture and water of crystallisation determined by carefully drying a certain quantity on a water-bath and noting the loss. The sulphuric acid and pure quina were estimated by dissolving equal weights of the anhydrous disulphates in water slightly acidulated with nitric acid, dividing the solution into two parts and precipitating one with nitrate of baryta and the other with ammonia. Both precipitates were collected, washed, dried, and weighed, and the sulphuric acid calculated from the sulphate of baryta. The purity of the precipitated quina was further ascertained by solution in ether, redissolving the ethereal extract in water slightly acidulated with sulphuric acid, and allowing the solution to crystallise for the estimation of the pure disulphate of quinine.

The amount of sulphuric acid and basic quina obtained by simple precipitation so closely corresponded to each other, and to the theoretical values of base and acid given by Pereira and others, as to leave no doubt of each being a pure disulphate. In the subsequent estimation of the crystallised disulphate, the discrepancies arise partly from the actual solubility of quina itself in water and alkaline ammoniacal solutions, and partly from the unavoidable loss which may well take place when a vegetable alkaloid is subjected to various chemical manipulations. To avoid, however, as far as possible, every irregularity in the results of these analyses, all the quinines were treated exactly alike, the same quantity of alkali, acid, and water being used in each case, so that the causes of error might bear equally on each.

	German. (Bought after Notice.)	English.	French.	French.	German.
1. The quinine was packed or sent out . . .	In tin box lined with paper	In plain tin box	In bottle	In bottle	In bottle
2. Weight	Rather short	Good	Very good	Good	Could not be ascertained, as part had been used
3. Appearance	Perfectly crystalline. White	White, crystalline	White, crystalline	White, loose mass	Yellowish
4. 100 grains by weight measured	850 grains	800 grains	775 grains	1200 grains	600 grains
5. Moisture and water of crystallisation . .	15 per cent.	13·3	14·8	11·6	3·3
6. Deficiency of water of crystallisation . .	1 per cent.	2·7	1·2	4·4	12·7
7. 100 grains of the dry disulphate precipitated, dissolved in ether and recrystallised, gave . . .	87·0 per cent.	84·0	86·6	82·3	81·6
8. Loss in recrystallising 100 grains of the dry disulphate as indication of quinoidine .	4·0 per cent.	6·0	6·6	7·0	7·6
9. Cinchonine	None.	None	None	None	None

In re-crystallising a disulphate of quinine containing quinidine in its minimum of water (30 parts), we meet with a loss, which will be greater in proportion to the amount of quinidine in the sample, and considering the mother liquor as a saturated solution of disulphate of quinine, we may add this to the re-crystallised disulphate and estimate with tolerable accuracy the amount of quinidine by the remaining difference in weight. The performance of these operations, however, requires the greatest possible nicety, for the disulphate of quinine is easily decomposed by heat and converted into uncrystallisable quinine, which might lead an unskilful analyst to estimate the quinidine at too high an amount.

Perfectly crystallised disulphate of quinine contains 16 per cent. of water of crystallisation; but in this all the samples were more or less deficient (as will be seen by a reference to the tabular statement). The crystallised disulphate of quinine is, in fact, a salt whose small delicate crystals are easily damaged, and subsequently part with their water of crystallisation, and it is simply a matter of age and keeping to make this change more or less considerable and apparent. When kept in a close sealed vessel, and in contact with paper, or any other absorbent substance, the liberated moisture may be readily observed in the damp condition of the paper; while in the absence of the latter, as in a stoppered bottle, the same moisture would remain mechanically mixed with the quinine. On exposure to air, however, the same loss would speedily take place. We know by experience that this change from the crystallised to the anhydrous state is by no means uncommon, as many a dispensing chemist can testify. Quinine in medicine chests, when these are sent to be refilled, will never look as it did when sent out; and even when none has been used from a bottle, a considerable loss of weight may be found. This loss of crystalline structure and of weight is a serious matter to those who are obliged to keep large quantities of the disulphate of quinine; and as moisture on the quinine itself proves less objectionable than the same moisture in the paper, we would recommend manufacturers to avoid lining their quinine tins with anything at all, to keep the orifices for filling them as small as possible, and to select for closing their smaller bottles such corks and paper as are least likely to absorb moisture. The buyer and retailer will always suffer some loss, but, by taking similar precautions, he may keep his loss within the least bounds.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, March 21, 1861.

Professor Brodie, *F.R.S., President, in the Chair.*

(Continued from page 221.)

But another question attaches itself to this. The chief use of the results that we obtain in this manner is that they agree and give us tangible evidence of those same results which one arrives at from a general unquantitative comparison of the action of bodies upon one another. I mean to say that the processes where bodies contain much specific heat, and in which the particles are in a very rapid state of motion, which on combining evolve much heat, and accordingly form compounds containing little motion of particles, are those in which the force is greatest during combination, and that the tendency of bodies to re-act chemically, to combine, I will not say in direct proportion to their specific heat, because that would be a quantitative statement which is not authorised at present; but, at all events, that those bodies in which the particles are in the most rapid state of motion are those which have the greatest tendency to re-act chemically, and *vice versâ*. Whether this takes place in direct proportion to the rapidity is a numerical point which I will not attempt to speak to.

There are one or two other points which I may be allowed to raise before I leave the subject. One is, whether one ought to suppose that in the case of two elements of very different weights—take a heavy one like iodine, and a light one like hydrogen—which, if they contain the same specific heat, must have very different velocities of motion—whether the two elements, which have very different weights, on uniting assimilate their movements to one another. I think the matter ought to be stated clearly, if possible. There are some reasons which would rather incline me to believe that something of the kind occurs. It is singular how difficult it is to get heavy elements and very light ones to hold together with any great force. Everybody knows in such cases how very unstable the compound is compared with the stability of the compounds of elements more like one another in their atomic weights. When one compares the compounds of iodine with metals to those of chlorine with metals, or to those of chlorine with non-metallic bodies like phosphorus, there are a good many cases in which we find that phosphorus or copper would take up a quantity of chlorine, and that it is incapable of holding in combination as many atoms of iodine. I believe it seems to be not improbable that these effects may be owing to the difficulty which iodine has to assimilate the motion of its atoms to that of an element very dissimilar from itself in weight, and which is more inclined than itself to move with great velocity.

I will not detain you longer than to add my apology for bringing before the Society considerations still so incomplete as these must necessarily be at present; but I felt if there were any body in the world before which I could venture to bring them, it was the Chemical Society of London. My excuse is chiefly a hope that it would lead the members of the Society to collect facts, and to consider the bearing of facts that have been already established in connection with this fact; because it is, I believe, undoubted, that the atoms of matter are in a constant state of motion relative to one another.

The Chairman, in the name of the Society, thanked Dr. Williamson for the highly interesting and philosophical discourse which he had just delivered, and invited the gentlemen present to express their views on the subject which had been brought forward.

Dr. Odling: Among the very interesting topics which Dr. Williamson has suggested, there is one which, I must confess, struck me as being rather weak, and that is the relation of the amount of heat given out in combination, to the altered specific heat of the products. I was struck by one observation that Dr. Williamson made in reference to the diffusiveness of chlorine, and bromine, and iodine. He said that all cases of diffusion could not be explained in that manner, but here we had, with three remarkably simple bodies, well-defined illustrations of the fact, that the mobility of the particles was in proportion to their specific heats; and arguing from these well-defined compounds, he expressed an opinion that this must be something more than a coincidence. This, he said, or seemed to imply, ought to be the normal condition of things, to which the others, which are at present irregular, will be brought in time, when we are able to ascertain the interfering causes. Now, in precisely a similar manner, I would make a remark upon specific heat. If we take those remarkably simple compounds, the iodides, the chlorides, and the bromides, the rule is that the specific heat of the product is identical with the specific heats of the elements entering into combination. That rule seems to be constant and absolute in connection with proto-bromides, proto-iodides, and proto-chlorides. For instance, the specific heat of chlorine is 6, that of silver 6, and that of chloride of silver 12; the specific heat of chlorine is 6, and of lead 3, and that of chloride of lead 9, omitting decimals. Now, this is true of proto-chlorides, proto-bromides, and proto-iodides, and I may say of per-chlorides also; and it is not only true of these compounds, but of many others, the sesquioxides and sesquisulphides, for instance. Now, I would rather be inclined to regard this circumstance in something of the typical light in which Dr. Williamson regards the phenomenon of diffusion. I think his view with regard to the relation in specific heat between the two bodies which unite and the products of their union, rests on a somewhat unsubstantial basis.

Dr. **Tyndall**: I am not quite competent to follow the whole of the discourse; but the thoughts which have pervaded it are certainly quite physical in their character, and as such command my sympathy. Professor Williamson appeared at the commencement disposed to extend the term "latent heat,"—to push it beyond its present limits, and to apply it to phenomena to which the term is not now applied. I go quite with him, as far as to say, that these phenomena to which he has referred—for instance, expansion of gas, have quite an equal right to have the term applied to them as a change of aggregation; but instead of extending this term to other phenomena, because they have the same character as those where change of aggregation is concerned, I think it would be rather better—and I submit this to his consideration—to abolish the term altogether. Sooner than extend it to others, let us rather eradicate it from those phenomena to which it has been hitherto applied. I am perfectly persuaded that you will agree with me that the term is founded upon a misconception. For instance, you have a certain mechanical action for the purpose of raising that black-board. Suppose it is raised by Professor Williamson's own arm, some of the heat would be expended in the raising of the board. Would you say that that heat was rendered latent in the board? I do not know if the term will quite apply there. And thus, if we take the case of a pile-driving engine, the weight was raised up to a certain height by the action of steam; in raising it a certain height against the force of gravity a certain amount of heat would be expended. Now, there is a certain amount of gravitating force stored up in this weight, and when the weight is liberated it descends, and, therefore, an amount of heat must be evolved equal to what was expended in lifting it; but I don't think the term "latent heat" would apply to the weight suspended. Now, I would again submit to Professor Williamson—and the fixing of these terms in science is an important point—whether it would not be better to eradicate this term, which was based on the material theory of heat, and the assumption that it was actually hidden in the pores of a gas and squeezed out of this gas, such as aqueous vapour, upon condensation; and to substitute something more consonant with the real physical changes that take place.

Dr. **Longstaff** said it seemed that the use of terms in this discussion was of great importance. He entirely concurred with the opinion of the last speaker that the term "latent heat" should be abolished: he did not believe, however, that its use had arisen from the theory of the materiality of heat. He had arrived at precisely the opposite conclusion, and it was only by retaining that theory that the term "latent" could be abolished.

Dr. **Williamson**: I may, perhaps, be permitted to make the remark, with regard to Dr. Odling's interesting criticism, that I was not unaware of the fact that a certain amount of agreement has been asserted between the specific heats of bodies and those of their constituents. But I think Dr. Odling must be also well aware that cases of disagreement are considerably more numerous. When agreement was shown, the experiments in most cases were exceedingly rough. (No, no.) I think that physicists are rather inclined to lay stress upon those determinations which came somewhat near, because they thought that was the law which ought to govern the phenomena. However, I do not profess to be able to prove from the result of experiments on the subject that the specific heats show diminution generally; but the experiments certainly are not consistent with the notion of equality. A few cases of agreement are as much exceptions as anything else; and I think that the dynamic theory of heat leaves us no choice. With regard to Dr. Tyndall's suggestion about latent heat, I am not at all fond of the idea of heat being stored up, because it is not stored up; but still, when a term is in general use, I conceive it better not to be too critical. I should certainly by no means be the last to give up the term if physicists are willing to give it up; although I must say, that if that board be lifted by a mechanical force, I don't see why we should not call the heat latent in it, as well as when water evaporates and is lifted into the atmosphere; for the quantity of heat which disappears when water is converted into steam does certainly serve to lift it into the atmosphere, just like lifting a machine or a pile. However, I shall be very glad to give up the term. Dr. Longstaff's notion of the material nature of heat would lead us into questions which perhaps it would be rather difficult to settle to-night. I am not at present disposed to extend our notion of matter beyond that which is ponderable; therefore, according to that test of matter, which I am disposed to retain at present, I should not call heat a kind of matter.

Dr. **Tyndall** said that if a weak current of electricity were sent in a certain direction through a bismuth and antimony junction of a thermo-electric pile, cold would be developed; or when a certain amount of heat fell upon the face of the pile it was actually converted into electricity, or another kind of force. This might be called a case of latent heat, just as in the other case.

Dr. **Williamson**: One just as much as the other.

Anniversary Meeting, March 30.

Professor **Brodie**, *F.R.S., President, in the Chair.*

The Report of the Council was read, from which it appeared that the Society consisted of 342 Fellows, 30 Foreign Members, and 10 Associates. During the year there had been a loss of 3 Fellows by death, and an acquisition of 22 new Fellows, making an increase of 19. At the Ordinary Meetings of the Society, there had been 33 papers read, and 4 lectures delivered. The following were elected Officers and Council for the ensuing year:—*President*—A. W. Hofmann, Ph.D., LL.D., F.R.S. *Vice-President*, (who have filled the office of President)—W. T. Brande, F.R.S.; B. C. Brodie, F.R.S.; C. G. B. Daubeny, M.D., F.R.S.; Thomas Graham, F.R.S.; W. A. Miller, M.D., F.R.S.; Lyon Playfair, Ph.D., C.B., F.R.S.; Colonel Philip Yorke, F.R.S. *Vice-Presidents*—H. Bence Jones, M.D., F.R.S.; Robert Porrett, F.R.S.; Alfred Smee, F.R.S.; A. W. Williamson, Ph.D., F.R.S. *Secretaries*—Theophilus Redwood, Ph.D.; William Odling, M.B., F.R.S. *Foreign Secretary*—E. Frankland, Ph.D., F.R.S. *Treasurer*—Warren De la Rue, Ph.D., F.R.S. *Council*—Thomas Andrews, M.D., F.R.S.; William Francis, Ph.D., F.L.S.; J. H. Gladstone, Ph.D., F.R.S.; G. D. Longstaff, M.D.; W. Marcet, M.D., F.R.S.; John Mercer, F.R.S.; Normandy, A. R. L. M.; W. H. Perkin; H. E. Roscoe, Ph.D.; Edward Schunck, Ph.D., F.R.S.; John Stenhouse, LL.D., F.R.S.; and Robert Warington.

Thursday, April 4, 1861.

R. **Warington**, *Esq., in the Chair.*

Messrs. T. Wood, R. Collyer, J. Hearn, and F.

Norrington were elected Fellows, and Mr. J. H. Smith an Associate.

Dr. **Frederick Guthrie** read a Paper "*On some Derivatives from the Olefines.*" This communication was a continuation of the author's previously read papers on the action of compound halogens on the hydro-carbons of the formula Ɵ_nH_{2n}. The test of fractional solution was applied to prove the homogeniety of the bodies $\text{Ɵ}_2H_4S_2Cl$ and $C_5H_{10}S_2Cl$. When either of these bodies is partly dissolved in alcohol, the entire substance has the same per-centage composition as the dissolved part.

The relation between $\text{Ɵ}_2H_4S_2Cl$ and $\text{Ɵ}_2H_4S_2$ was established by showing that the action of chlorine, in both cases, gave rise to $C_2\genfrac{}{}{0pt}{}{H_2}{Cl_3}S$. When $C_2H_4S_2Cl$ is recomposed by KO HO the body $\text{Ɵ}_2H_4S_2HO_2$ is formed: just as $C_5H_{10}S_2HO_2$ is formed when $C_5H_{10}S_2Cl$ is recomposed by the same reagent. Further the chlorine in $C_5H_{10}S_2Cl$ was replaced by cyanogen and sulpho-cyanogen by the action of the corresponding potassium compounds; the bodies

$$\text{Ɵ}_5\ H_{10}\ S_2\ Cy.$$
$$\text{Ɵ}_5\ H_{10}\ S_2\ S_2\ Cy.$$

being produced.

From this and previously described reactions, the conclusion was drawn that—towards chlorine bodies of the formula,

$$\text{Ɵ}_n\ H_{2n}\ S_2\ Cl.$$

act like the sulphides of chloriferous radicles,

$$\text{Ɵ}_n\ H_{2n}\ Cl,\ S_2$$

while towards metallic oxides, hydrated oxides, cyanides and sulphocyanides, they act like the chlorides of sulphuriferous radicles,

$$\text{Ɵ}_n\ H_{2n}\ S_2,\ Cl.$$

The author regarded this as additional evidence of the impotence of a single formula to express the construction or potential recomposition of a body.

The body $\text{Ɵ}_5\ H_{10}\ S_2$ was formed when $\text{Ɵ}_5\ H_{10}\ S_2\ Cl$ was submitted, in alcoholic solution, to the action of Zn.

The body $\text{Ɵ}_5\ H_{10}\ Cl_2$ was produced by the reaction between $\text{Ɵ}_5\ H_{10}$ and PCl_5.

From the already described body,

$$C_5\ H_{10}\ 2NO_4$$

the remarkable transformation into

$$\text{Ɵ}_5\ H_{10}\ Cy_2 + 5HO$$

was effected by the action of cyanide of potassium. From the penthydrated bicyanide of amylen, attempts to form the pimelate of potash by the action of caustic potash, were unsuccessful.

By the action of zinc-ethyl upon $\text{Ɵ}_5\ H_{10}\ S_2\ Cl$, a body was formed having the composition of the bisulphide of œnanthyl, viz.—

$$\text{Ɵ}_5\ H_{10}\ S_2\ \text{Ɵ}_2\ H_5 \text{ or } \text{Ɵ}_7\ H_{15}\ S_2.$$

A reply was given to M. Wurz's critique as to the rational formulæ to be assigned to the bodies,

$$\text{Ɵ}_n\ H_{2n}\ S_2\ Cl_2$$
$$\text{Ɵ}_n\ H_{2n}\ S_2\ Cl$$

and their derivatives.

Dr. **Frankland** remarked upon the extreme importance of the reaction by which Dr. Guthrie had apparently succeeded in converting a member of the amylo-valeric into a member of the œnanthylic group. Supposing that by a similar reaction effected by zinc-methyl or zinc-ethyl we were able to displace the chlorine of chloride of methyl or chloride of ethyl by a radicle, we might successively build up all the primary carbon groups from one another.

Dr. **Odling** referred to the analogy of the reactions of protosulphide and bisulphide of chlorine upon ammonia and amylene respectively. With ammonia we obtained the sulphochloride of chlorammonium $NH_3Cl.ClS$, the sulphide of chlorammonium $(NH_3Cl)_2S$, and the bisulphide of chlorammonium $(NH_3Cl)_2S_2$. With amylene we obtained the sulphochloride of chloramyl $C_5H_{10}Cl.ClS$, the sulphide of chloramyl $(\text{Ɵ}_5H_{10}Cl)_2S$, and the bisulphide of chloramyl $(C_5H_{10}Cl)_2S_2$. He advocated the reference of these last compounds to the mercaptoral types.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, April 2, 1861.

J. P. **Joule**, *LL.D., President, in the Chair.*

Mr. John Parry and Mr. Thomas Carrick were appointed Auditors of the Treasurer's accounts for the present Session. M. Durand Fardel, M.D., was elected a Corresponding Member. Messrs. Henry Brogden, John Thomas Hobson, Ph.D., William Alexander Cuningham, and George Robert Haywood, were elected Ordinary Members.

Mr. T. T. **Wilkinson**, F.R.A.S., exhibited an Amulet, which, having been worn by an African chief who was captured in war about a century ago, had been recently examined, when it was found that the centre boss of about an inch and a-half diameter contained an Arabic manuscript, the translation of which, kindly undertaken by Professor Theodores, was laid before the meeting.

Mr. **Wilkinson** also laid before the meeting some Geometrical Theorems.

A Paper was read by W. **Fairbairn**, LL.D., &c., "*On the Temperature of the Earth's Crust, as exhibited by Thermometrical Observations obtained during the sinking of the Deep Mine at Dukinfield.*"

During the prosecution of researches on the conductivity and fusion of various substances, an opportunity occurred of ascertaining by direct experiments, under favourable circumstances, the increase of temperature in the crust of the earth. This was obtained by means of thermometers placed in bore-holes, at various depths, during the sinking of one of the deepest mines in England, namely, the coal mine belonging to F. D. Astley, Esq., at Dukinfield, which has been sunk to a depth of 700 yards.

The increase of temperature in descending, shown by these observations, is irregular; nor is this to be wondered at, if we consider the difficulties of the enquiry and the sources of error in assuming the temperature in a single bore-hole, as the mean temperature of the stratum. At the same time it is not probable that the temperature in the mine-shaft influenced the results. The rate of increase has been shown in previous experiments to be directly as the depth, and this is confirmed by these experiments. The amount of increase is from 51° F. to 57¾°, as the depth increases from 5⅔ to 231 yards, or 1° in 99 feet; but, in this case, the higher temperature is not very accurately determined. From 231 to 685 yards, the temperature increases from 57¾° F. to 75½°. This is a mean increase of 1° in 76·8 feet, which does not widely differ from the results of other observers. Walferdin and Arago found an increase of 1° in 59 feet; at Rehme, in an Artesian well 760 yards deep, the increase was 1° in 54·7 feet; De La Rive and Marcet found an increase of 1° in 51 feet, at Geneva. Other experiments have given 1° in 71 feet. The observations are affected by the varying conductivity of the rocks, and by the percolation of water. The Author has exhibited upon a diagram, in which the ordinates are depths, and the abscissæ temperatures, the results obtained betweem the depths of 231 and 717 yards. The strata of the mine are also shown in section. Additional to these, the Author gives a Table of similar results in another pit at the same colliery, taken between the depths of 167½ and 467 yards, and showing an increase of temperature of 1° in 106 feet of descent.

Assuming as an hypothesis, that the law thus found for a depth of 790 yards, continues to operate at greater depths, we arrive at the conclusion that at 2½ miles from the surface, a temperature of 212° would be reached, and at forty miles a temperature of 3000°, which we may suppose sufficient to melt the hardest rocks. The author then discusses the effect of pressure and increased conductivity of the rocks in modifying this result. If the fusing point increased 1°·3 F. for every 500 lbs. pressure, as is the case with wax, spermaceti, &c., the depth would be increased from 40 to 65 miles before the fluid nucleus would be reached; but as the same increase is not observed with tin and barytes, the influence of pressure on the thickness of the crust cannot yet be determined. Again, Mr. Hopkins has shown that the conductivity of the dense igneous rocks is twice as great as that of the superficial sedimentary deposits of clay, sand, chalk, &c. And these close-grained igneous rocks are those which we believe must most resemble the strata at great depths. Now, if the conductivity of the lower rocks be twice as great as that of the strata in which the observations were made, correcting our former estimate, we should probably have to descend 80 or 100 miles, instead of 40, to reach a temperature of 3000°, besides the further increase due to the influence of pressure on the fusing point. On entirely independent data, Mr. Hopkins has been led to conclude that the minimum thickness of the crust does not fall short of 800 miles, in which case the superficial temperature of the crust would have to be accounted for from some other cause than an internal fluid nucleus.

A paper was read by J. C. Dyer, Esq., Vice-President, entitled "*Brief Notes on the Nature and Action of Steam in Relation to Boiler Explosions.*"

He stated that several essays had lately appeared on boiler explosions, wherein discordant theories and opinions are offered on the action of steam in some anomalous cases of explosion, and which may justify the bringing before the Society a few established facts and principles relating to the subject, in the hope of arriving at more settled views concerning causes and effects in such cases than appear to prevail at present among our most distinguished engineers. The author objected to the appeals made to Dr. Dalton's theory of atoms for explaining the nature of steam as an elastic force mechanically employed, since the law of definite proportions of Dalton had no reference to elastic vapours except as to the constituents of the liquors whence they arise. He then cited the fact that water, like steam, is an elastic body, and the pressure would therefore be of the same nature and force above and below the water line in a boiler; but that explosions from fractures above or below that line would have different effects, owing to the amount of expansion of water and steam being so widely different when issuing from similar apertures and under the same pressure. Many obscure cases of explosion would be explained by the more or less rapid generation of steam issuing under those circumstances, as set forth in the paper. That free space, when suddenly and amply afforded, is to highly heated water, under great pressure, nearly the same as fire is to gunpowder. And this will account for the most destructive cases of boiler explosions; whilst those of a more harmless nature show that the fractures were small at first, and then gradually extended.

He also objected to the term "superheated steam," as being inapplicable to it in any state; because, when steam is in contact with water, it will be of the same temperature as the water; and if heated apart from water, the same laws of expansion by heat apply to steam as to air, and neither can be "superheated," though made very hot.

Again, steam can never be "mixed up with the water" in a boiler when both are under the same statical pressure, and the steam formed will rise into the chamber, so that the water will always be in contact with the boiler except when steam is drawn off. Still, in rapid escapes, it may drive out water and become entangled therewith, as in many explosions.

It having been shown that most, if not all, explosions are occasioned by simple steam pressure, acting on the weakest parts at first, and thence extending more or less rapidly, it would seem needless to seek for any other cause or force to account for them; yet, in some cases, the effects appear to imply a more sudden and violent action, like that of explosive compounds. In such instances, may they not arise from the actual decomposition of the water by heat alone? Although we have high authority (cited) against this, yet the Author held it rash to conclude that water could not be resolved into its constituent gases by direct action of heat from the boiler upon water pressed into contact with the metal plates. It has been proved, long since, that by heat, in the most intense form known to us—that of electricity—water is decomposed and both of its constituent gases are liberated. Therefore, since no evidence has been adduced to show that this does not take place in any water when so confined and heated, the affirmative may at least be possible, and seems probable, in some instances, as before named.

However, he held it desirable that the question should, if possible, be set at rest by experiment; and to this end a method was suggested for putting the matter to a direct test; but he might not be able to make the experiment himself, and hoped it might be done by some one more competent to the task.

SECTIONS FOR STATISTICS AND SOCIOLOGY.

March 12, 1861.

A Paper was read by Mr. Thomas R. Wilkinson, furnishing statistics connected with the sale of beer and spirituous liquors, with a view to enquire whether such sale had any effect on the criminal returns, and how far the Legislature would be justified in a further interference with it.

The number of licensed victuallers in the United Kingdom in 1860, was 107,024, and the number of beer sellers, 45,198.

The licensed victuallers represent interests which directly concern 50,000,000*l.* worth of property, and maintain 1,000,000 people.

The revenue derived from beer and spirits in the year 1832 was 15,000,000*l.*, or 29·41 per cent. of the whole revenue. In 1858, this revenue amounted to 21,000,000*l.*, or 31·34 per cent. of the whole revenue. The increase of this revenue had been 40 per cent., whilst the increase of the population was 25 per cent.

In 1831, the amount derived from licences for the sale of

		£.		£.
Beer	amounted to	..273,871	And the year 1859 to	..384,178
Spirits	,,	..395,697	,,	..563,400
Wine	,,	.. 73,026	,,	.. 88,238
		£742,594		£1,035,816

An increase of 39·62 per cent.

An Act for closing public houses, from Saturday night at twelve o'clock, until one o'clock on Sunday, came into operation in 1847-8.

For eight years before the passing of that Act, the annual average number of persons taken into custody by the Manchester police was 10,658, whilst after the Act was passed the annual average was only 5371.

MICROSCOPICAL SECTION.

March 18, 1861.

A communication from Captain Anderson, R.M.S. *Canada*, written at sea on his homeward voyage, excited considerable interest. He states that Dr. Wallich's pamphlet would be communicated to the Boston Society of Natural History by Professor Agassiz, who was particularly interested in the Ophiocoma found at so great a depth. The Professor is now engaged in preparing a work upon the natural history of that class of echinoderms, which he has studied for many years, and claims to have the finest collection of those animals in existence, made on the coasts from Greenland and Labrador to Mexico and round Cape Horn to California. Since the publication, in 1848, of the "Principles of Zoology," by Agassiz and Gould (a copy of which is presented to the Section), the Professor has ascertained that the system of tubes and water pores, described at page 123, exists in all animals which much vary their depths of water in the sea, and in the herring they may be seen with the naked eye along the side of the neck.

With reference to the removal of tallow from soundings, Dr. Hayes, the Assayist for the State of Massachusetts, stated to Captain Anderson that heated turpentine poured amongst the soundings, will remove all the tallow with it through filtering paper; the operation should be twice repeated, and the residue finally washed with sulphuric ether.

Dr. J. **Bacon** presented a copy of his Report upon the Chemical Composition and Microscopical Characters of the Pearl, said to have been formed in the interior of a cocoa nut at Singapore, in the possession of Frederick Bush, Esq., and exhibited by Dr. Winslow.[1]

Captain Anderson, in a very able manner, gives the outline of a plan which has occurred to him for rendering available to science the services of commanders of merchant vessels and seamen generally, in collecting specimens of natural history and information in natural science, whether in zoology, ethnology, botany, or meteorology, for which they have such facilities in all parts of the world, for the use of those scientific institutions which may desire to join in it and also with a view to elevate the mercantile marine of England in the social scale by stimulating a taste for such knowledge amongst seafaring men. The consent and co-operation of ship owners will of course be necessary, and Captain Anderson seeks the additional influence of merchants and scientific bodies. The subject met with the unanimous approval of the members present; and it was resolved that the portion of Captain Anderson's letter relating to it should be published at the expense of the Section, for the purpose of eliciting opinions upon the feasibility of the scheme, and upon the best practical method of carrying it into execution.

Commander M. F. **Maury**, U.S. Navy, forwarded copy of a letter from Lieutenant John M. Brooke, the inventor of the Detaching Deep Sea Sounding Apparatus, and enclosing for the Section a number of soundings from the North Pacific Ocean, which were obtained with small twine and spherical weights of about 70lbs., which were detached upon contact, and left at the bottom of the ocean; Lieutenant Brooke observes that "Nine consecutive casts (soundings), varying from 2000 to 2900 fathoms, were made with the same piece of twine and detaching apparatus, which last weighed less than 1lb." As the specific gravity of a wet flax line is nearly that of water, a line that can be pulled down by a weight, may be pulled up by hand, provided the weight be detached at the bottom. One of the specimens obtained in 3030 fathoms, nearly three and a-half miles, is the greatest depth from which material has yet been brought up from the ocean bed. Lieutenant Brooke sent also a few specimens obtained by soundings in shallow waters, on the east coast of Niphon, Japan, by him during his boat voyage from Simoda to Hallodadi in 1855, under the orders of Commander Rodgers, U.S. Navy.

Mr. **Binney** described to the Section the appearance of certain nodules found in the middle of a seam of coal, in the lower part of the Lancashire coalfield which contain fossil wood associated with marine shells. Specimens of the former were exhibited to the members, the most perfect of which was that of Sagenaria, the old Lepidodendron elegans, in transverse, parallel, and tangential sections. The marine shells associated with the fossils belong to the genera Aviculopecken, Orthoceras, Nautilus, &c.

Mr. **Brothers** exhibited a Section of Pearl, Isthmia nervosa, infusoria, &c.

Mr. **Whalley** exhibited living Diatomaceæ from Southport.

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 28, 1861.

Mr. E. W. **Binney**, *F.R.S., F.G.S. &c., in the Chair.*

Professor Clifton, of Owens College, was elected a member of the Section. The following gentlemen were elected Officers of the Section for the ensuing year:—*President*—Mr. Robert Worthington, F.R.A.S. *Vice-Presidents*—Mr. Baxendell, F.R.A.S.; Mr. Binney, F.R.S., F.G.S. *Treasurer*—Mr. George Mosley; *Secretary*—Mr. Thomas Heelis, F.R.A.S.

A Paper was communicated by Mr. **Atkinson**, from Mr. T. T. Wilkinson, F.R.A.S., of Burnley, entitled "Geometrical Theorems relating to the Triangle and its Inscribed and Escribed Circles."

Mr. G. V. **Vernon**, F.R.A.S., produced Manuscript Copies of thirteen years Barometrical Observations made at Greenwich, which had been kindly communicated by the Astronomer Royal, showing the observations once a day for every day instead of the daily means, and presented such series to the Section, for which present he received its thanks.

Resolved, that the Secretary be requested to prepare and issue a circular, applying for copies of Logs and Meteorological Observations made at sea, and especially on board the various mail steamers.

PHARMACEUTICAL SOCIETY.

Wednesday, April 3, 1861.

T. N. R. **Morson**, *Esq., President, in the Chair.*

The first paper read was "*On the Preparation of Smelling Salts*," by Mr. A. **Allchin**. Mr. Allchin considers the ordinary method of making smelling salts by filling a bottle with small fragments of sesquicarbonate of ammonia and adding a little perfume and some strong liquor ammonia to be altogether unsatisfactory. The contents of a bottle so filled soon lose their pungency, and a nearly inodorous residue remains. His own plan is first of all to convert the sesquicarbonate into the monocarbonate of ammonia, which is accomplished in the following way. Forty ounces of sesquicarbonate of ammonia are broken into fragments about the size of filberts and placed in a jar having a well fitting lid. Into this is afterwards poured 20 ounces of liquor ammonia, sp. gr., 880°. This mixture is frequently stirred for a week, and the jar is then set aside in a cool place for three or four more weeks. If the mixture is not stirred for the first week it sets as hard as a stone; but after stirring it becomes solid and dry, but can be easily removed from the jar. It is now reduced to a roughish powder something like salt of tartar, and in that state it is ready for filling the bottles, and improves by keeping. When placed in the bottles some volatile essence or strong ammonia perfumed with essential oils is added. The volatile essence Mr. Allchin uses and recommends is the first given in Professor Redwood's edition of Gray's Supplement to the Pharmacopœia, and is as follows:—Take of English oil of lavender and essence of musk, of each four drachms; oil of bergamot, two drachms; oil of cloves, one drachm; otto of roses, ten drops; oil of cinnamon, five drops; strongest liquor ammonia, one pint. In the above way, a salt is made which retains its pungency as long as any remains in the bottle. One that was filled five years ago was exhibited to the meeting, and although nearly all the contents had evaporated, what remained still possessed a pungent, agreeable odour.

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[1] Page 290, vol. vii., Proceedings of the Boston Society of Natural History for 1860.

Mr. Squire noticed the brown colour of the salt, which he attributed to the oil of cloves in the perfume, and suggested that the salt would be improved if it were quite colourless. He also asked why Mr. Allchin preferred an aqueous solution of ammonia to the alcoholic. It was well known that a large quantity of a particular smelling-salt was exported from England, and Mr. Squire said he believed this salt owed its superiority and its fame to the alcoholic solution of ammonia used in its preparation.

Mr. Waugh said he preferred a salt which was not liable to change colour. Great discredit attached to articles which changed in appearance after leaving the hands of the vendor, and it was a great matter to avoid the possibility of such changes.

Professor Redwood considered the scent quite a secondary matter. The most important thing was the preparation of a salt which should retain its pungency a long time, and Mr. Allchin had shown how such a salt might be prepared. Sesquicarbonate of ammonia was a compound of two salts, one pungent and one scentless. The one soon evaporated, and what was left in the bottle was worthless. It was important to obviate that. Rose's method of procuring a perfectly volatile carbonate of ammonia was to distill the sesquicarbonate quickly in a retort, when the part which condensed furthest from the body of the retort was the neutral carbonate. Mr. Allchin's method was much simpler. In the common way of filling a smelling-bottle there was no chance of adding sufficient liquor ammonia to convert the sesqui- into the mono-carbonate.

Mr. Allchin said he preferred the aqueous to the alcoholic solution of ammonia, because he believed the latter did not contain enough ammonia to effect the conversion of the sesquicarbonate, and because the water combined chemically, and formed a definite compound.

Mr. Haselden said that smelling-salts could be made colourless if the oil of cloves were omitted from the perfume. The use of alcoholic ammonia, he thought, would be expensive, and of no practical value.

The Chairman said the Society was much indebted to members who brought practical subjects like this before the meetings. He believed it was preferable to use aqueous ammonia, for the reason that the water entered into combination with the salt, which the alcohol would not.

Mr. Squire could not agree with the Chairman. Alcoholic ammonia must possess some advantages, or it would not have been used by those who had exported the salts for more than a hundred years. He believed, too, that alcohol could be made to take up as much ammonia as water if the condensation was effected at a low temperature. Mr. Squire then referred to the preparation of sal-volatile, and repeated, that as the cloves were the cause of the coloration of this preparation when made according to the Pharmacopœia, he hoped they would in future be omitted from the formula. The oil of cloves had a high boiling point, and did not come over until all the spirit had passed; but as the carbonate of ammonia was not very soluble in spirit, it was necessary to have some water, and with it the oil of cloves distilled. He thought it would be found convenient to have two preparations: one like the Edinburgh, a simple solution of caustic ammonia in spirit; and another, like the London compound spirit of ammonia, without the cloves.

A Member stated that a mixture of eight parts chloride of ammonium, four parts carbonate of potash, and two parts sesquicarbonate of ammonia made a superior smelling salt.

Mr. Haselden then read a short paper on "*The Tincture of Actea Racemosa*," which he believes is best made by percolation. The tincture so prepared has a darker colour, a greater density, and seems in every way preferable to one made by maceration. Proof spirit was said to be the best menstruum, and the proportions used were four ounces of the root to a pint of spirit.

After some inquiries as to where the actea root could be purchased,

Mr. Hanbury inquired if any member could tell where black hellebore root could be bought. He believed it was not to be found in the market. Of four samples he had purchased as black hellebore, two were actea, and the others neither actea nor hellebore.

Mr. Squire said when he wanted black hellebore root he dug up the Christmas rose.

Professor Bentley remarked on the striking resemblance between the roots of the actea and black hellebore. All the specimens of what was called black hellebore in the market he had found to be the roots of the actea spicata. The adulteration or substitution was not a recent one, but it was unknown to Dr. Pereira. Specimens called black hellebore, which had been in the Museum fifteen years, were actea root.

Professor Redwood exhibited to the meeting an improved form of Condenser; and Mr. Hanbury called attention to two fungoid bodies—*Mylitta lapidescens* and *Sclerotium stipitata*—which were used as medicines in India.

This was the last Pharmaceutical meeting of the season.

CORRESPONDENCE.

Copper in Mineral Waters.

To the Editor of the Chemical News.

Sir,—A number of the *Comptes-Rendus* has just been shown to me. I notice in it that M. Béchamp, in a letter to M. Dumas, "On the Presence of Copper in the Mineral Water of Balaruc," has the following, with regard to the cupreous sulphate it contains:—"It (*i.e.*, the sulphate of copper) explains the purgative properties of this water much better than the presence of the other mineral ingredients." How can this be? The water, according to the analysis of St. Pierre, holds three magnesia salts in solution, and to these ought to be attributed the aperient properties of the spring. Sulphate of copper is an *astringent*. How could M. Béchamp make so wrong a statement? Pereira says, "As an *astringent* it has been used with great benefit in chronic diarrhœa and dysentery."—I am, &c.

Sheridan Muspratt, M.D., &c.
Professor of Chemistry.

College of Chemistry, Liverpool.

The Adulteration of Food.

To the Editor of the Chemical News.

Sir,—Although it is far from my desire to criticise too minutely the communications of those who hold an opposite view to myself on this question, yet I think I may be allowed to remark that Mr. Scott's last letter on this subject is scarcely characterised by that simple diction and purity of style which we have a right to expect from one who is so ready at pointing out the deficiencies of others in these respects. Mr. Scott evidently avoids giving a direct answer to any question that may be asked, and the manner in which he attempts to twist the obvious meaning of words to suit his own ends comes with a very bad grace from one who takes me to task for not bringing forward my opinions in a "simple, straightforward manner," complains of my "perversion of his statements and contortion of his writings," and suggests most gratuitously that I have some "mysterious" purpose to serve in stating my views on this question; in short, his arguments and train of reasoning appear to me to be those of one who is anxious to support former statements rather than of one who is simply desirous of arriving at the truth. My question regarding essences admitted of a full and satisfactory answer being given in a very few words, and there was certainly no necessity for endeavouring to set it aside, in a manner which is evidently intended to be anything but complimentary.

My "useless" query as to the quantity of alum found was based upon the belief that many chemists were in the habit of weighing as alumina a precipitate which often contained a large proportion of phosphates. In following the published methods for the detection of alum in bread, I had found it necessary to adopt some means for the separation of phosphoric acid from the precipitate produced by ammonia before I could obtain results that were to be depended upon. Your very excellent article on this subject in No. 70 of your Journal so fully enters into all the bearings of the case that it is unnecessary for me to say more than express my belief that had such a method of analysis as that you propose been adopted, we should never have heard of bread containing such large quantities of alum, nor would the use of this adulterant have been asserted to be so general.

After stating his "utter inability to reply to the interrogation," and much preliminary nonsense about arithmetical means, real and theoretical quartern loaves, &c., Mr. Scott condescends to furnish the information which it must have been quite evident the question was intended to elicit. It seems that the quantities found by Mr. Scott were very variable, and ranged between the extremes of 19 and 182 grains to the quartern loaf. Presuming that Mr. Scott, in the prosecution of these analyses, adopted the method which he has publicly recommended for the detection of this adulterant, it is evident, from your own observations, that a considerable proportion of the precipitate which was weighed as alumina consisted really of phosphoric acid, lime, and magnesia, and it is impossible to say how far the extreme variation of the weight of the precipitate was due to that circumstance. It is very certain, however, that his results must be open to correction, and that his assertion—to adopt a favourite expression of his own—can only be taken for what it may happen to be worth. It appears from a letter of Dr. Odling, in your last week's Journal, that the information respecting his analysis of bread, which was published in a letter to the Editor of the *Journal of the Society of Arts*, was altogether erroneous, and that the fact is, that out of 40 samples examined, he found 31 to contain alum. Now, I am quite ready to admit that this is so much evidence in favour of Mr. Scott's assertion; but, granting that every precaution had been taken, and the presence of alum fully proved in these 31 samples, yet I feel confident that Dr. Odling would be very far from saying, on the strength of the examination of 40 samples, that 77 per cent. of the bread of London is adulterated. The question whether the presence of rice and potatoes is indicated by brittleness of structure, or crumbly texture, is one upon which those who are interested in the matter can easily satisfy themselves by direct experiment. The next time I have sample loaves made from flour purposely adulterated with known quantities of foreign ingredients I shall be most happy to furnish you with samples, to be forwarded to Mr. Scott, on condition that he will analyse them and allow a statement of the quantity of each ingredient he finds to be published in your Journal, along with that of the quantities of the adulterants actually employed, which shall be forwarded to you in a sealed letter, to be opened on the receipt of Mr. Scott's numbers. As regards the reliance which may be placed on the evidence of merchants and manufacturers, the idea that men of the standing of such firms as Messrs. Huntley and Palmer, Messrs. Phillips and Co., and Messrs. Crosse and Blackwell require a certificate of character from Mr. Scott before their statements may be quoted as worthy of credence can only excite a smile. By what right does Mr. Scott assume that his statements are entitled to be received as truth, whilst the testimony of men whose knowledge of the subject has been acquired from long experience is to be disregarded? or, why is it to be supposed that, whilst self-interest warps the judgment of the merchant or manufacturer, it can have no such influence on the professional man?

It is very certain that nothing can tend more to shake the faith of the public in chemical science than the assumption on the part of its professors of a position to which it is clear they have no title.

It is, moreover, unfortunate that we find these very lofty ideas associated with a course of conduct with which they are strangely inconsistent. It is surely beneath the character of one who claims to be considered as a man of science to associate himself with the "puffing" advertisements of the day, or lend his name to statements which are intended to serve the same purpose among the educated classes as is effected by "Cheap Jack" among the lower orders, by means of enormous handbills and a voluble tongue.

The neighbourhood of London has lately been favoured with an abundant supply of circulars from Messrs. Dakin and Co., announcing that they have "retained the scientific assistance of Dr. Hassall, M.D. Lond., Analyst of the *Lancet* Sanitary Commission, Author of 'Food and its Adulterations,' 'Adulteration Detected,' and other works." Messrs. Dakin and Co. have magnanimously resolved to supply an alleged defect in the Adulteration Act, and have determined upon giving a special warranty with every article of food or drink they supply, and thus to "render themselves liable under the Act," as if it were compulsory to do so, whilst the public are further to be protected by a Quarterly Report from Dr. Hassall of the results of the analyses of articles purchased from time to time at their establishments.

After this flourish of trumpets, let us see what it all amounts to:—

Tea, coffee, arrowroot, sago, tapioca, and various others are warranted "genuine as imported."

Cocoa, chocolate, and mustard, "genuine as received from the makers."

Mixed spice, "genuine as received from the grinding-mills."

It is, in fact, simply a declaration that Messrs. Dakin do not for themselves adulterate, a fact which I am disposed to believe to be true as regards the greater number of respectable houses in the trade.

Mr. Scott's assertion about strychnia in bitter beer already serves as a heading for the hand-bill of a Westminster brewer, whilst Messrs. Brown and Polson make Mr. Scott their mouthpiece through whom they cry their wares and seek the public custom.

Does it ever occur to these advertising gentlemen that a chemical certificate for purity can only be regarded as appertaining to the sample actually examined, or to the bulk which such sample strictly represents, and can afford no criterion whatever of the quality of other samples sold by the same firm?

Surely it is the duty of those chemists who have any regard for the credit of their profession to discountenance in every possible way practices which can only tend to bring upon it contempt and ridicule. The position of an analytical chemist is undoubtedly a high one, and the tasks he has to perform involve much labour, careful research, and patient study. He ought however to recollect that he draws to some extent, on the faith of the public who have to take much upon credit, inasmuch as they have often not the means of disproving his assertions. Of all classes of professional men, it behoves him to be especially careful to guard against anything either in his conduct or statements which by any possibility can have a tendency to lessen that confidence which as a class they are warranted in claiming.—I am, &c.

VERAX.

Alum in Bread.

To the Editor of the CHEMICAL NEWS.

SIR,—In reading the various letters in your valuable paper on this subject, and the difference of opinion thereon, the suggestion occurred to me that it would be much better that the analyst could say, "There is the alum obtained from the bread," which he can easily do if he is the chemist he professes to be, instead of trusting to a precipitate, which is doubtful. In a case of poisoning by arsenic the poison has to be produced in court. Why not the alum likewise? It is to be hoped that any baker prosecuted for such adulteration will insist on the production of the alum with which he has been said to adulterate his bread. I saw the alum produced lately from an ounce and a-half of bread to such an extent that it could be judged of by the taste and pronounced such by parties tasting, unaware of what it was or whence it came.

I am, &c. STRAIGHTFORWARD.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Action of Nitrous Acid on Cuprous Oxide.—Lenssen mentions (*Journal für prakt. Chemie*, Bd. lxxxii. s. 50) a curious reaction of nitrous acid. If a drop of red fuming nitric acid be added to a tolerably dilute solution of cuprous chloride rendered strongly acid by hydrochloric acid, a deep indigo blue colour is produced of magnificent intensity. A similar result follows when a solution of nitrite of potash is used instead of red fuming nitric acid. On heating the mixture with the addition of an alkali the reaction is destroyed. By adding ammonia carefully, the blue colour of the ammoniated copper compound may be seen at the upper part of the test-tube, and in the lower part the indigo blue of the Cu_2O, NO_4 (?) the two being separated by a stratum of clear liquor. A solution of cuprous chloride indicates the presence of a very small amount of nitrous acid, but the reaction only takes place in a strong hydrochloric acid solution. Sulphuric acid, however, may replace hydrochloric acid, a mixture of equal volumes of hydrated sulphuric acid and water being used, and the reagents being added after the mixture has cooled.

Preparation of Crystallized Soda Alums.—Gentele describes (*Journal für prakt. Chemie*, Bd. lxxxii. s. 56) the following method of getting crystallized chrome and iron soda alums:—Dissolve one equivalent of soda alum in a small quantity of water, and one equivalent of potash chrome alum in much cold water without the assistance of heat, and then mix the solutions. Some ordinary pale, reddish-coloured potash-alum immediately separates, and on spontaneous evaporation more, in large crystals. When the fluid has evaporated to a small bulk, it is separated, and a layer of alcohol is poured over without mixing, and the vessel is set in a cool place, whereupon after some time, octahedral crystals of soda chrome alum shoot out. These are not red like the potash chrome alum, but green. Crystals of soda iron alum can be obtained in exactly the same way. These are colourless.

II. ORGANIC CHEMISTRY.

Solid Acids of Cocoa Butter.—Oudemans has carefully examined fresh cocoa butter, and decides that no acid having the formula $C_{26}H_{26}O_4$ (the cocinic acid) is present. The solid acids, he says, (*Scheik. Onderz.* D. iii. p. 84) are a mixture of a little palmitic and myristinic with much laurinic acid.

New Salts of Oxamic Acid.—E. Bacaloglo describes (*Journal für prakt. Chemie*, Bd. lxxxi. p. 379) some new oxamates which he has prepared in Erdmann's laboratory:—

Neutral Oxamate of Lead, $PbO + C_4H_2NO_5 + HO$, obtained when oxamate of ammonia is decomposed by neutral acetate of lead, is a white crystalline salt, easily soluble in hot and slightly soluble in cold water. The solution has an acid reaction. At 100° the salt loses its equivalent of water, without undergoing any apparent change, and it may be heated to 175° without further decomposition.

Basic Oxamate of Lead, $2PbO + C_4H_2NO_5$, is a white powder insoluble in water, obtained when oxamate of ammonia is precipitated with basic acetate of lead. When treated with boiling water this salt is obtained in an anhydrous state.

Oxamate of Copper, $CuO + C_4H_2NO_5 + HO$, is a blue granular powder, soluble in hydrochloric acid, and slightly so in water and nitric acid. When a dilute solution of oxamate of ammonia is added to a moderately strong solution of sulphate of copper only a cloudiness is produced; but on boiling, or on the addition of a small quantity of nitric acid, the greater part of the oxamate of copper separates. Like the basic lead oxamate, the copper salt appears to lose water on boiling.

Oxamate of Nickel, $NiO + C_4H_2NO_5 + HO$, is most easily made by mixing hot solutions of oxamate of ammonia and sulphate of nickel. It is a greenish white, granular powder, soluble with difficulty in hot water.

Oxamate of Iron, $FeO + C_4H_2NO_5 + HO$, forms beautiful yellow microscopic crystals. Freshly precipitated ferric oxide, boiled with a solution of oxamic acid dissolves, forming an oxamate of ferric oxide, this exposed to direct sunlight slowly decomposes, evolving some gas, and a yellow precipitate falls, which, probably, contains the oxamate of ferrous oxide.

Oxamates of Mercury—By decomposing the ammonia salt with per- or proto-nitrate of mercury, the quicksilver salts are obtained as white, insoluble powders.

Reactions of Oxygen. — Schönbein continues (*Sitzungsb. der Königlich. bayer Akad. d. Wissenschaft*, July, 1860) his important investigations on the reactions of oxygen in its three modifications. Some of these (on Aniline) we have given already; at page 98 of this Volume. We now add some others.

Action of $\ominus$ *on Hæmatoxylin.*—White filtering paper dipped in a concentrated etherial solution of hæmatoxylin appears when dry of reddish yellow colour. Introduced into strongly ozonised air it quickly changes to a brown red, and at last becomes bleached, the paper then having a sour taste, and reddening moistened litmus paper. Powdered hæmatoxylin in a watch-glass exposed to the action of strongly ozonised air is soon coloured as red as peroxide of iron, then becomes moist and changes to a brown mass, which eventually turns to a colourless liquid that tastes strongly acid. An aqueous solution of this matter gives a white precipitate with lime water, which the author concludes is oxalate of lime. Combined $\ominus$ behaves in the same way. The paper, prepared as above and moistened with water, is soon coloured by the peroxides of manganese, nickel, &c. Dipped in the solution of a hypochlorite or permanganic acid it is instantly changed to a dark red.

Behaviour of $\oplus$.—Hæmatoxylin dissolves in water containing HO_2, giving a colourless solution; but for this experiment to succeed, not the least trace of an alkaline substance must be present. The mixture very gradually becomes violet. Turpentine containing $\oplus$ behaves like HO_2.

Behaviour of Neutral O.—A colourless aqueous solution of hæmatoxylin in contact with ordinary oxygen is slowly reddened, and after twenty-four hours becomes deeply coloured. The coloration is hastened by making the solution of hæmatoxylin hot. Schönbein has not yet succeeded in detecting HO_2 in the coloured liquid, but he believes some to be formed. A hundred grammes of the hæmatoxylin solution, containing one-thousandth part chromogen, shaken up in a two pint bottle, with five or six drops of a strong soda solution and atmospheric air, until the liquid becomes a deep cherry colour (which happens in the course of half-a-minute), and then immediately supersaturated with a little dilute SO_3, gives a yellowish mixture which possesses the following properties:—

1. Pure ether shaken up with an equal volume of the mixture and a drop of a dilute solution of chromic acid is coloured distinctly blue.
2. The mixture decolorises a solution of permanganate of potash, setting free O gas.
3. It turns a solution of indigo green, which only gradually changes to a yellow, but the addition of a drop of a dilute sulphate of iron solution quickly effects the change.
4. After shaking up with platinum black, peroxide of lead, &c., the mixture loses the power of producing the foregoing reactions.

These facts leave no doubt in the author's mind that a small quantity of HO_2 is formed during the oxidation of hæmatoxylin effected under the influence of soda, apparently by neutral O; and he believes, therefore, that the chemical polarization of neutral oxygen precedes the oxidation of hæmatoxylin, which is only effected by means of the $\ominus$.

MISCELLANEOUS.

The Electric Light.—The French Minister of Marine has contracted for the establishment of eight electric lights on the coast between Cape de la Heve, near Hâvre, to Treport, east of Dieppe. The object of these lights is to maintain a communication with ships within sight of land, and to transmit the news rapidly to the interior.

The Atlantic Cable.—The report of the Atlantic Telegraph Company states that in the cable recovered and brought home by Captain Kell, there was not the slightest symptom of deterioration or decay in the gutta-percha. It had been subjected to a severe electrical test, and a comparison between its present state of insulation, and the records of original tests of the most perfect portions of the cable, when it left the gutta-percha works, three years ago, showed that an actual improvement had taken place in its conditions since it was laid down.

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, April 23, at 3 o'clock, Professor Owen "On Fishes." Thursday, April 25, at 3 o'clock, Professor Tyndall "On Electricity." Friday, April 26, at 8 o'clock, Professor Owen "On the Scope and Appliances of a National Museum to Natural History." Saturday, April 27, at 3 o'clock, Max Müller, Esq., "On the Science of Language."

Bricks and Tiles made by Machinery.—We have been much pleased with an inspection of Eccles' Patent Brick and Tile Machine now on view at Messrs. Dray, Taylor, and Co.'s, London Bridge. The machine is about 12 feet long by 9 broad and is capable of producing 24,000 bricks in ten hours. The clay being taken direct from its native state, and made into a brick in a few minutes, without any previous expensive preparation. They are made perforated, which gives them many advantages over bricks of ordinary pattern. The following is the mode of employing this machine for making bricks:—The clay is taken from the embankment and placed into waggons, which run on a tram-road on a slight incline to the hopper, fixed at a height of about four feet six inches from the ground level, thus dispensing with the expense of fixing hoisting machinery, and power for working it. If the clay field is on an incline, the waggons may be run on to the hopper platform on a level. In the hopper are placed two sets of knives or agitators, working transversely, through which the clay passes and is cut up into small pieces, preparing it for the crushing rollers, of which there are three, placed at distances apart to suit quality of clay, thence passing into a horizontal cylinder fitted with the ordinary pug-mill shaft and blades, working on the screw principle, and is then forced by a side knife or wiper, through the dies on the tables fitted with rollers, where the mass is divided by wire cutters, (placed at equal distances apart) into the required size for Bricks. They are then removed on trays to the drying shed, where they remain until ready for the kiln. The drying sheds are heated by steam and hot air pipes, the former by using up the exhaust steam from the engine, and the latter by pipes inserted in the boiler flue, (not, however, interfering with the working of the boiler in any way, and no extra fuel being required.) The hot air is driven by a fan into the sheds through pipes, which when they reach the sheds are perforated, to allow the air to escape, and the heat to be equally diffused amongst the bricks. This plan of drying commends itself as the most economical extant.

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Numerous applications having been made for Monthly Parts of the CHEMICAL NEWS, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*** All *Editorial Communications* are to be addressed to Mr. CROOKES, and *Advertisements* and *Business Communications* to the PUBLISHERS, GRIFFIN, BOHN & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

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M. P. S.—You can easily concentrate the nitric acid by mixing it with oil of vitriol and distilling. If required to be quite free from sulphuric acid, it must be rectified a second time over nitrate of potash.

Reginald R.—Geranium oil may be detected in rose oil by mixing with concentrated sulphuric acid. If geranium oil be present, a peculiar unpleasant odour is produced, whilst the odour of pure rose oil is not affected by sulphuric acid. We do not know a quantitative means of estimating this adulteration. The strength of an essence may be roughly determined by mixing with three times its bulk of water and seeing how much oil is separated. This is, however, not very trustworthy.

T. C. Bell—Your idea is far from new. It has been proposed frequently, and Ransome's process is in effect the same thing. He uses silicate of soda and chloride of calcium, producing silicate of lime in the stone, and chloride of sodium which washes off. This is obviously better than your suggestion of using chloride of barium and sulphate of lime.

J. B.—Either sulphate of magnesia or molybdate of ammonia. We rather prefer the latter.

L. M. N.—It is now in type, and will be inserted as soon as we can find space.

Books Received.—"On the Application of Different Manures to Different Crops." By J. B. Lawes; and "On the Comparison of Oxen, Sheep and Pigs." By J. B. Lawes and Dr. J. H. Gilbert.

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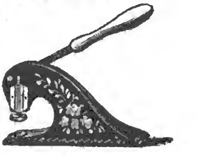

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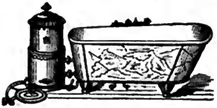

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THE
CHEMICAL NEWS
AND
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Edited by] [*Wm. Crookes, F.C.S.*

No. 73.] Saturday, April 27, 1861. {Price 4*d.* {Post Free 5*d.*

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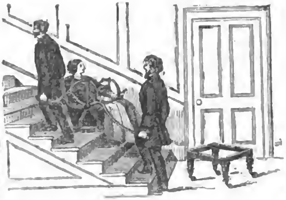

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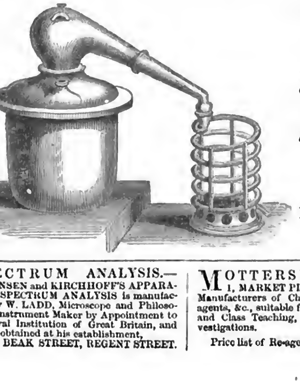

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THE CHEMICAL NEWS.

VOL. III. No. 73.—April 27, 1861.

THE NEW BLUE DYE, "AZULINE."

FOR some months there has been a blue colour in the market which has caused an endless series of inquiries as to its nature and origin. We have been at some pains to procure information about it, but, at present, to no purpose. One of our informants stated it to contain Prussian blue, but, as it leaves no residue on ignition, this is evidently incorrect. It differs from the aniline dyes in many particulars, not the least of which is its requiring the bath to be acidulated with sulphuric acid. This treatment with sulphuric acid has also to be resorted to after dyeing, to remove a reddish impurity which tenaciously adheres to the goods, and can only be removed by acids. Some chemists have supposed it to be prepared from chinoline, but this appears to us very unlikely; for, in the first place, chinoline blue "takes on" with wool or silk with the most extreme readiness, whereas azuline dyes wool with difficulty, but silk with tolerable ease. Azuline, moreover, resists the bleaching influence of light as well as indigo, whereas a piece of wool dyed with chinoline blue, and exposed to a brilliant light, soon begins to fade. In fact, azuline reminds one of indigo in more ways than one; its solubility in methylated spirit is, however, quite contrary to our notions of an indigo colour. Any person wishing to know the shade of colour of a solution of azuline may get an idea of it by boiling aniline in a test-tube with a little indigo. The hot aniline dissolves indigo, forming a solution having almost exactly the tint of the solution of azuline in methylated spirit. On cooling, the indigo crystallises out in beautiful coppery spangles. This method of crystallising indigo from a solution in boiling aniline does not appear to be generally known to chemists.

The smell of some of the solutions of azuline seems to betray a tarry origin; but, considering the number of operations which a colour undergoes before its final solution in spirit, such evidence loses most of its value. We are aware that naphthalamine can be converted into a blue dye; but our experience of it is, that it is fugitive, and in other respects unlike azuline. Nevertheless, there was one peculiarity in a specimen of blue which we prepared from naphthalamine, reminding us of azuline: *it bore the action of acids.* Another property of azuline, which seems to remove it from the aniline dyes, is its comparative insolubility in cold water, and the complexity of the process required for dyeing it properly on wool or silk. The annexed formula, which accompanied a specimen of azuline imported from Paris, will show this complexity very forcibly:—

Directions for Dyeing with Azuline on Silk.

"**Solution.**—The azuline is placed (in the cans, as delivered), for half-an-hour, in a water-bath, heated to about 120°, and is well agitated from time to time. It is then filtered through a fine linen fabric, and the clear fluid is ready for dyeing. Great care should be taken to ascertain that all the colouring-matter is either dissolved or in suspension. What remains in the filter should be dissolved in methylated spirit, filtered and used up. If any colour still remains, again dissolve and filter; for all, or nearly all, the colour will be dissolved in the fluid sent with it if due care is taken.

"**Dyeing.**—Dye in a bath slightly acidulated with sulphuric acid, and at natural temperature. Add the necessary quantity of azuline, in five or six successive portions, and after each addition of azuline work the silk quickly in the bath, and when the desired shade is obtained carry the bath to the boil by means of a jet of steam. Work the silk again well in the bath as it brightens the blue. The bath, being then of a dirty red, is thrown away, and a new bath made with boiling water, strongly acidulated with sulphuric acid; work the silk in this, passing it through seven times. The blue is again improved by losing its red tint. Wash the silk in cold water, and then pass it through a soap bath; wash, and pass it through a warm bath, slightly acidulated with sulphuric acid. The silk is then wrung out and dried.

"N.B.—The first bath should be cold to obtain uniformity; the boiling water with sulphuric acid gives brilliancy to the colour. It is of the greatest importance that the directions for entirely dissolving the colour be attended to. One gallon will dye about 32lbs. of silk medium shades, or at about 2s. per pound of silk."

It will be seen, by the directions for dyeing, that azuline is only recommended by the inventor for silk, and, indeed, it is very troublesome to apply to wool. This, again is entirely unlike the characteristics of aniline dyes generally.

We have written the above with the view of directing the attention of practical dyers and scientific men to azuline, chiefly with a hope that they will be able to throw some light upon its history. Although appearing to possess some valuable qualities, it is the least known of the many new dyes owing their existence to the recent progress of organic chemistry.

We need scarcely say that we shall be happy to receive communications upon the subject from any one who has real information to give.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On a New Colouring Matter (Rhamnoxanthine) extracted from the bark of the Buckthorn (Rhamnus frangula), by T. L. PHIPSON, *Ph.D., Member of the Chemical Society of Paris, &c.*

IN the year 1858 I discovered, by accident, in the bark of the buckthorn (*Rhamnus frangula*) a very remarkable colouring matter. Upon plunging into water, the little baskets in which the butter of Brittany is sold at Paris, I observed that the wood of which these baskets are constructed communicated to the water a vivid yellow tint, which finally became red, and then brown.

At first I was inclined to believe that the colouring matter came from the butter, which had probably been tinted with some well-known yellow dye in order to improve its appearance. I soon convinced myself, however, that this was not the case, and that the colour came from the wood of the baskets. The first thing to be done, therefore, was to ascertain with what species of wood the little butter baskets were constructed. It was not without much difficulty that in the course of three weeks I found that the wood in question was evidently that of the buckthorn (*Rhamnus frangula*).

In fact, when the branches of this shrub were placed in water, the liquid took the same yellow tint obtained with the fragments of basket, and those of the purgative buckthorn, *R. catharticus*, coloured the water in a similar manner.

In the next place I endeavoured to ascertain in what part of the tree this colouring matter was lodged, and I found that it occupied the cells of the layers of *liber* and the vessels of the *medullar tube*. In no other parts of the plant could I detect it. It is easily recognised with the naked eye in these two places upon the dried branches, but in the living shrub recourse must sometimes be had to re-agents in order to render its presence evident—which, however, never fails to be the case. Moreover, I found that if the branches be submitted to maceration for some days, their whole structure is imbibed with the colouring matter, which appears to prove that in the living plant the latter exists in solution. I then isolated the colouring principle in the manner hereafter described, and submitted it to a detailed examination.

Finally, it was necessary for me to ascertain if any other chemist had ever observed this same colouring principle, and, after having gone through a great many publications, I at length found that M. Buchner, a distinguished chemist of Munich, had extracted a yellow colouring matter from the bark of the root of *Rhamnus frangula*, to which substance he had given the name of rhamnoxanthine, and which was evidently the same substance I had extracted from the bark of *R. frangula* and *R. catharticus*. His observations were recorded in a short note published in the *Journal de Pharmacie et de Chimie*, for October, 1853. I published a slight account of this substance, under the name of Rhamnoxanthine, in the *Journal de Pharmacologie et de Médecine*, of Brussels, in 1858, and I afterwards found that rhamnoxanthine had also attracted the attention of M. Casselmann in the same year, who published a note upon it in the *Journal de Pharmacie et de Chimie*.

The results of the observations of these chemists have been generally confirmed and completed by my own, and will be reported in the present paper, with my experiments. M. Buchner observed that some bark taken from the root of *R. frangula* had communicated to the paper in which it was enveloped a bright yellow tint, and that the inner side of the bark itself was covered with a quantity of small brilliant crystals of a golden yellow. M. Buchner then extracted the colouring matter from the root by exhausting its bark with ether, or by treating with ether the alcoholic extract obtained from the bark of the root. In these circumstances, however, the colouring matter retains obstinately a little grease, which it is difficult to separate entirely by repeated crystallisation from ether.

I obtained rhamnoxanthine from the bark of *R. frangula* and *R. catharticus* in a somewhat different manner. The branches were plunged into sulphide or carbon, where I allowed them to remain for three of four days. The liquid, which at the end of that time had acquired a golden yellow colour, was evaporated to dryness at the ordinary temperature of the atmosphere, and the residue treated with alcohol, which dissolved the colouring matter and left behind a peculiar greasy substance of a brown colour. The alcoholic solution was evaporated to dryness, and the residue dissolved in ether, which, by spontaneous evaporation, left crystals of rhamnoxanthine. Thus obtained, rhamnoxanthine is insoluble in water and in diluted acids; it is soluble in ammonia and the alkalies, in alcohol, ether, and sulphide of carbon. Water precipitates it from these last three solvents; it can also be dissolved in fixed oils by means of heat, in benzin, and in essence of turpentine. With concentrated sulphuric acid it presents some curious phenomena, which I shall refer to presently. Nitric acid converts it into oxalic acid at a boiling heat, at the same time, according to M. Casselmann, a new acid is produced, to which he has given the name of nitrofrangulic acid, of the composition of which I shall speak further.

When heated quietly with a spirit-lamp rhamnoxanthine melts at about 250° C.; at the same temperature it sublimes, without decomposition, almost completely; but when heated in a tube, more or less is always carbonised, whilst the rest is converted into a vapour of an agreeable odour, which condenses itself in quadrangular microscopic lamellæ, or in oily drops, which crystallise on cooling. It can be thus obtained directly from the extract of the bark procured by sulphide of carbon, alcohol, or ether, in a sufficiently pure state. Upon analysis, rhamnoxanthine has given me the following results:—

	I.	II.	Theory.
Carbon . . .	56·97	56·88	57·15
Hydrogen . .	4·80	4·91	4·76
Oxygen : .	—	—	38·09
			100·00

whence we may deduce the formula, $C_{12}H_6O_6$, which corresponds also with the results obtained by M. Casselmann.

Ammonia dissolves rhamnoxanthine, giving a magnificent purple solution; potassa and soda have a similar action, but the colour they produce is not quite so delicate. With alkaline carbonates the solution obtained is of a reddish-brown. These combinations with the alkalies are destroyed by acids, and the primitive yellow substance is regenerated. By these reactions the colouring matter of which I speak resembles somewhat certain other yellow principles which have not been particularly studied. Moreover, I think I have detected the presence of rhamnoxanthine in several other plants, but have not yet completed my researches on this subject.

When concentrated sulphuric acid is poured upon rhamnoxanthine, this substance undergoes a very remarkable change. It immediately loses its golden yellow colour, and becomes of a bright emerald-green. I have observed the same striking phenomenon to take place with the yellow colour of leaves in autumn, and with the yellow colouring matter of the orange. If the action of concentrated sulphuric acid be allowed to continue, the fine green colour disappears, passing into a purple hue which dissolves in the acid. Water, and solutions of alkalies, appear to regenerate the primitive yellow. I have not examined particularly the combinations which take place with sulphuric acid, but the production of the green emerald colour in these circumstances appears liable to useful applications.

By eremacausis, or fermentation, rhamnoxanthine is decomposed, and transformed into a brown colouring matter, which I have not examined. The same change occurs when it is heated a little in concentrated acids. This brown colour, which is insoluble, is likewise obtained when rhamnoxanthine is treated with a mixture of caustic soda and protochloride of tin, when it is precipitated as a laque: it can also be formed by putting rhamnoxanthine in contact with nascent hydrogen. It is obtained invariably of the same fine brown tint by means of protochloride of tin and caustic soda, the liquid which contains the mixture being heated nearly to ebullition. When treated with hot nitric acid, rhamnoxanthine is oxidated and converted into oxalic acid. At the same time, a new acid is formed, which has been examined by M. Casselmann, who has called it nitro-frangulic acid.

This product, submitted to analysis, gives:—

Carbon . . .	39·0
Hydrogen . . .	1·9
Nitrogen . . .	11·4
Oxygen . . .	47·7
	100·0

which corresponds nearly with the formula:—

$$C_{12}H_4Az_2O_{12}.$$

This product is soluble in alcohol, from which it crystallises in yellow crystals, which easily detonate when heated; it is also soluble in boiling water and in alkalies, to which it gives a purple colour.

With oxides of magnesium, zinc, aluminium, tin, lead, &c., rhamnoxanthine forms laques, the tints of which may be varied to a great extent. Thus, if the branches of *R. frangula* be plunged into a weak solution of ammonia, the colouring-matter is dissolved, giving a red-purple liquid; if, then, the ammonia be saturated with citric acid and magnesia added, a beautiful violet-coloured laque is obtained. Again, on adding protochloride of tin to the yellow decoction obtained by macerating the branches in water, and precipitating with carbonate of ammonia, a brown-yellow laque is obtained, which sulphuric acid converts into a chocolate laque. Other brown, red, and yellow laques may be procured in a variety of manners.

When exposed to the damp, the wood of *R. frangula* and *R. catharticus* becomes gradually purple, which is probably owing to a formation of ammonia by putrefaction.

On attempting to dye stuffs with rhamnoxanthine, I found that this colouring-matter has a greater affinity for silk and wool than for cotton. But fine golden-yellow and purple dyes can be obtained by the use of mordants, and, in the hands of an experienced dyer, rhamnoxanthine may one day become a useful product.

In conclusion, I will state that this substance probably exists in other plants; it resembles very closely the chrysophanic acid obtained from the lichen *Parmelia parietina*, and which is supposed to exist in the root of rhubarb.

On Certain Reactions of Salts of Iron—Separation of Uranium and Iron, by M. F. Pisani.

Until the present time the property of impeding certain reactions has been attributed almost exclusively to non-volatile organic acids, such as tartaric acid; but it has lately been remarked that, in certain cases, oxalic acid possesses the same property. Thus, it is known by a recent experiment that its presence prevents the blue precipitation of salts of iron by ferrocyanide of potassium. The following are some analogous facts which I have observed:—

Salts of Iron.—If to salts of iron, as neutral as possible, acetic acid and an excess of oxalate of ammonia are added, the solution preserves its yellow colour, and does not become red, as is generally the case in the formation of acetate of iron. From this solution phosphate of soda does not precipitate phosphate of iron; but ammonia, as well as the ammoniacal sulphide, separate from it, as usual, all the iron. The liquid is not more coloured if, instead of acetic acid, acetate of soda is employed; but it then precipitates by phosphate of soda. To obtain these reactions, oxalate of ammonia must be added to the solution of iron, so that its colour changes from yellow to green-yellow.

Salts of Uranium.—In the presence of oxalate of ammonia nitrate of uranium does not give a red precipitate by ferrocyanide of potassium. In my paper on the estimation of uranium I stated that it could be precipitated by phosphate of soda in an acid liquid; but it must be remarked, that if oxalate of ammonia is first added to the solution, phosphate of soda gives no precipitate. If ammonia is added to it, all the uranium is separated in the state of phosphate; but if phosphate of soda has not been added, the precipitation by ammonia is incomplete. What remains can be precipitated only by the addition of phosphate of soda.

Salts of Aluminium.—When oxalate of ammonia is present in great excess, alumina is not immediately precipitated by ammonia and ammoniacal sulphide; but in a certain time, according to its proportion, alumina is precipitated, especially by the aid of heat. If we have to treat phosphate of alumina it is even possible to recognise phosphoric acid in the solution by a salt of magnesia; but this plan is not to be recommended, for it will probably precipitate as quickly as the alumina. Thus, for salts of aluminium oxalic acid fulfils but for a short time the purposes of tartaric acid.

Separation of Uranium and Iron.—When, as is generally the case, carbonate of ammonia is employed for this separation, it is well known that some iron is always dissolved with the uranium. The following is, however, a method for rendering the separation complete:—As oxide of uranium in solution in carbonate of ammonia does not precipitate by ammoniacal sulphide, we have only to add to the liquid, separated by filtration with oxide of iron, several drops of the latter re-agent to eliminate from it in the state of sulphide the small quantity of iron which has been dissolved. After filtering again, we get a liquid containing all the uranium without any trace of iron.—*Comptes-Rendus.*

On Certain Properties of the Platinum Metals, particularly Ruthenium, by Dr. C. Claus.

(*Continued from page 195.*)

The platiniferous metals constitute a well-marked group of analogous metals, the characteristics of which are based on the following properties:—

1. These metals have nearly the same atomic number. 2. Their characteristic double salts, allied in composition, are isomorphous. 3. These metals, particularly when in a pulverulent condition, exert a great attraction for

oxygen. 4. Their oxides are reducible by hydrogen at the ordinary temperature—a phenomenon always accompanied by a heat sufficient to make them red hot.

Besides these common properties, these metals differ greatly from each other. The properties of some are allied to those of the so-called "noble metals," and particularly to gold. Others, while preserving some points of similitude to the noble metals, are still more allied to the electro-positive metals, iron and manganese.

These metals may be arranged in two superimposed series, the *superior horizontal* of which I designate the *principal series* (*haupt reihe*), because the metals which constitute it predominate in the various platinum ores. This series is characterised equally by an elevated atomic number and by almost the same specific gravity, and by the circumstance that its isolated members are particularly apt to form with chlorine the combination MCl_2, which in its double salts with the chlorides of the alkaline metals maintains itself in the general type $MCl + MCl$, the crystalline form of which belongs to the tesseral system. The second *horizontal series*, which I call the *secondary series* (*neben reihe*), contains the remainder of the platiniferous metals, which also possess almost identical atomic and specific weights, but have in this respect but half the quantities of the principal series. These metals readily unite with chlorine according to the type MCl or M_2Cl_3; their double salts, with the alkaline metals, have the composition $M'Cl + MCl$, $2M'Cl + M_2Cl_3$ or $3M'Cl + M_2Cl_3$. Their crystalline form does not belong to the regular system, but to other systems with oblique axes.

If we divide the isolated members of these two series by vertical lines perpendicular to the horizontal line, we shall have a grouping of pairs of metals resembling each other, which, excepting the characteristics mentioned in the horizontal series, present in all other respects, decided analogies,—groups in which each pair consists of a principal metal (*haupt metall*) and a secondary metal (*neben metall*). In these pairs the resemblance is very evident, as I have already shown with respect to two pairs,—ruthenium and osmium, rhodium and iridium,—a resemblance which I shall make more apparent in the present article. The analogy of these metals, above all in their complicated combinations, extends so far, that not only are they alike in form, but their reactions are identical. Is not the newly-discovered volatile hyperruthenic acid another example in favour of this grouping? Palladium and platinum also constitute an exceedingly characteristic pair; in fact, they appear to be exactly alike in all their combinations, in their ammoniacal bases, in their cyanuretted combinations, &c.; and the fact of palladium in solution precipitated by cyanide of mercury is repeated by platinum. This fact is already known, but not the circumstances under which the precipitation occurs; these circumstances are the same as with palladium.

In fact, platinum, like palladium, ought to be found in the solution in the state of chloride; if, as has been observed, cyanide of mercury precipitates a platinum solution, treated by an organic substance, this is not the direct consequence of the presence of the organic substance, but rather because it determines the change from the state of platinic chloride to platinous chloride. Were it possible to prepare a solution of neutral chloride of palladium perfectly pure, and free from palladous chloride, it is very probable that cyanide of mercury would produce no precipitate from it. It has already been remarked that of all platiniferous metals, platinum itself is the one most closely allied to the precious metals, particularly to gold; palladium bearing a less striking resemblance to it, is allied to it through platinum, to which it bears so great an analogy; and this leads me to call this pair of metals, "the pair at the negative end of the horizontal series." The analogy is still more decided at the positive end of this series, that is to say, between osmium and ruthenium, as between iron and manganese, which on their side resemble, in their mutual relations, those of the pairs of the platiniferous metals. The solutions of a composition like that of ferrate of potash and osmite of potash, are so much alike that they are undistinguishable by their colour.

(To be continued.)

TECHNICAL CHEMISTRY.

Notes on Benzoic Acid and some Benzoates,
by John M. Maisch.

Benzoic Acid.—When benzoin is treated for benzoic acid by Mohr's process, it is necessary to gradually increase the heat, and if the process is continued until vapours cease to rise, the sublimate generally contains a variable quantity of empyreumatic matter, to which it owes its odour, and which likewise imparts colour to the product. The acid ought to be free of colour, and to obtain it thus a second sublimation is often found necessary. In order to avoid this, it is well to have two paper hoods ready made to fit the subliming vessel, and remove the first before it has been requisite to raise the temperature. At about 300° F. a quantity of benzoic acid may thus be obtained, which is perfectly white, of a high silky lustre, possessing but little of the empyreumatic odour, and, when dissolved in an alkali, affording a colourless solution; its appearance and chemical behaviour prove it to be far superior to the article usually met with in commerce.

If the sublimation is now continued, the acid obtained subsequently is found to contain empyreumatic colouring matter, as the process proceeds; it may be freed from it by a second sublimation. This increase of empyreumatic products in the sublimed acid is not owing merely to an increase of the temperature, but principally to the fac that the bibulous paper stretched over the vessel has become saturated with the same, and is unable to absorb and retain any more at the temperature to which it is exposed. If, during the process, the paper is replaced by a fresh piece, the acid sublimes as white as before, and with as little odour adhering to it. This empyreumatic aroma is frequently considered essential for the medicinal value of the acid, and such careful treatment of the balsam would therefore be inadmissible when the acid is to be used in medicine. But I believe that the product which has been obtained by a slow sublimation at a low temperature, will, by its beauty and purity, fully repay for the little trouble incidental with its collection.

Benzoate of Soda.—When benzoic acid is almost neutralised with carbonate of soda, a colourless solution results, which, when sufficiently concentrated, yields, on cooling, small, thin needles, in star-like groups; if somewhat further evaporated, the solution assumes a brownish colour, and may remain liquid for a day or two without separating any crystals. If still further evaporated, the crystals form very slowly in the syrupy liquid. They are very long, needle-shaped, and commence to become opaque while still in contact with the liquid, together with which they dry to an opaque mass without crystalline structure. When removed from the solution and rapidly pressed between bibulous paper, they preserve only in some measure their original shape. The brown, syrupy liquid, as obtained by evaporation, dries, to the last drop, to a white, opaque mass. Benzoate of soda is best obtained in crystals, by evaporating its solution only so far that on cooling it will just commence to crystallise, and to expose it gradually to a lower temperature. The crystals thus formed by cooling to 32° are to be removed from the thin liquid, and rapidly dried by pressing them between folds of bibulous paper.

Benzoate of Iron.—According to Trommsdorff and Berzelius, the ter-benzoate of iron is formed by dissolving recently-precipitated sesquioxide of iron in an aqueous solution of benzoic acid. The yellow needles are decomposed by water and alcohol, leaving a basic salt behind.

When a concentrated solution of benzoate of soda is added to the officinal solution of ter-nitrate of iron, a voluminous, orange-coloured precipitate is obtained, which is immediately thrown on a filter; when the liquid has nearly all passed, a small portion of water is added to displace most of the ferric solution, and the precipitate is then allowed to dry, spread out upon a brick tile. This was used for analysis.

Five grs. were heated in a porcelain crucible; a little water was first given off, then benzoic acid sublimed in small, white crystals. On increasing the heat, a brown liquid collected on the cover, which congealed into brown-coloured scales, having the empyreumatic odour and taste of benzoic acid. The residue was repeatedly moistened with nitric acid, and exposed to heat until vapours ceased to be given off; it then weighed 0·85 grs., which is equal to 17 per cent.

Ten grs. of the benzoate of iron was repeatedly treated with a large excess of ammonia, the sesquioxide of iron was well washed upon a filter, then washed into a tared capsule, and heated until it ceased to lose weight; its weight was 1·6 grs. = 16 per cent.

Into a glass tube, weighing 48·1 grs., 3·08 grs. benzoate of iron were introduced, and exposed to the heat of a water-bath for half-an-hour. The loss was only about 0·02. The tube was now introduced into a sand-bath of 260°, and kept at this temperature until aqueous vapours ceased to be condensed in the upper part, it now weighed 50·85 grs. The loss, therefore, amounted to 51·18—50·85, = 0·33 grs., or 10·71 per cent.

From these data, the following formula is calculated for this ter-benzoate of iron:—

Fe_2O_3	80	16·91	16·50
$3C_{14}H_5O_3$	339	71·67	71·44 (see below)
$6HO$	54	11·42	10·71
$Fe_2O_3, 3C_{14}H_5O_3+6HO$	473	100·00	

Ter-benzoate of iron, as thus obtained, is in a light powder, of a beautiful orange colour, permanent in the air, turning brown between 270° and 280°; it fuses and yields when carefully heated at first pure benzoic acid, afterwards an empyreumatic liquid, of an acid reaction, which appears to be principally the same acid, and congeals in scales. Water and alcohol dissolve a portion of it, probably an acid salt, and leave a basic salt behind.

Benzoic acid is scarcely used in medicine, except in paregoric, and occasionally in ointments; when taken internally, it is stated to act as a stimulant. Combined with iron, as in the above salt, it might perhaps prove useful in the hands of the physician. The salt has very little taste, not at all ferruginous, and would certainly be preferable to the acid or the alkaline salts, which possess a marked acrimony.

When the above ter-benzoate is desired, the directions above given for its preparation should be closely followed; the filtrate from the precipitate separates in the course of two days, another precipitate, which dries upon the filter in a yellowish powder, and in orange-brown scales, evidently a mixture of different compounds; and, after three weeks, more of the light powder is deposited. The drying is to be accomplished in thin layers upon a brick tile.

Bibenzoate of Ammonia.—In order to determine as near as possible the quantity of benzoic acid in the iron salt, the above filtrate from the treatment of the ferric benzoate with ammonia was evaporated, and kept at a temperature of below 190° F., until a dry mass remained behind and the loss of weight ceased. The residue weighed 8·25 grs.; it evolved ammonia when treated with an excess of caustic potassa; and Wackenroder's observation, which has been doubted already by Liebig, that on the spontaneous evaporation of a solution of benzoic acid in carbonate of ammonia, pure benzoic acid is separated in crystals, is thereby proven to be incorrect. Assuming the experiments of Berzelius, that a bibenzoate of ammonia is thus formed, to be correct, the above 71·67 per cent., or 7·167 grs. of $C_{14}H_5O_3$, would require of NH_4O 0·824 grs., and of HO 0·285 grs., making the required whole weight of the salt 7·167 + 0·824 + 0·285 = 8·276, with which the actual weight of 8·25 closely agreed. Accordingly, the formula for anhydrous bibenzoate of ammonia is calculated as follows:—

NH_4O	26	9·96
HO	9	3·45
$2C_{14}H_5O_3$	226	86·59
$NH_4O, HO, 2C_{14}H_5O_3$	261	100·00

and 8·25 grs. of bibenzoate of ammonia as found above contain $\frac{8\cdot25 \times 86\cdot59}{100} = 7\cdot143675$ grs. $C_{14}H_5O_3$ 71·437 per cent. of the above benzoate of iron.

This experiment verifies, on the one hand, the above formula for the neutral benzoate of iron, and tends to prove, on the other hand, that an anhydrous bibenzoate of ammonia remains behind on evaporating a solution of benzoic acid in ammonia, at a temperature below 190°. The influence of a higher heat had not been tried.—*American Journal of Pharmacy.*

Contributions to the History of Aniline: Production of a New Blue Dye-stuff (Paris Blue), by MM. Persoz, V. De Luynes, *and* Salvétat.

M. Hofmann,[1] while studying the action of bichloride of carbon on aniline, was led to the discovery of a new base, to which he assigned the formula $C_{38}H_{17}N_3$. To purify and separate it from the matters mixed with it, it suffices to wash it in cold alcohol, and crystallise it once or twice in boiling alcohol. Thus treated, the base crystallises, the alcohol retaining in solution a substance of a magnificent crimson colour.

The researches we have so long pursued on the colours derived from aniline, have led us to study the possible relations existing between the crimson substance discovered by M. Hofmann and the red dye-stuff discovered by MM. Renard and Franc of Lyons, to which the name of fuchsic acid has been given. We find that there is no analogy between these two substances; in fact, the Lyons red substance is completely soluble in alkalies, in connection with which it appears in the light of a true acid; it enters into combination with ammonia, potash, baryta, &c., and forms soluble combinations with these bases. These solutions, treated with acetic acid, serve to dye the purest shades. By grounding our process on the solubility of Lyons-red in alkalies, we have been enabled to extract this colouring matter from all the products employed in commerce under such various names.

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[1] *Comptes-Rendus*, September 20, 1858, Vol. xlvii. p. 492.

By strictly attending to the rules laid down by M. Hofmann, we have obtained, like this learned chemist, a viscous oil, gradually solidifying with a crystalline structure, and we have proved that the alcohol which had served to purify the base he investigated, remained crimsoned, the tint being more or less pure according to the circumstances under which the operation is conducted.

We have also ascertained that the product, insoluble in water, formed during the reaction, dissolves in hydrochloric acid, and that the hydrochloric solution yields with potash a dirty red precipitate, soluble in alcohol, to which it imparts a rich crimson colour. In M. Hofmann's opinion this colouring matter is of the same nature as that just mentioned; but we have been led to think that it is not so, and that it is formed by the mingling of two principles, one blue the other red.

This crimson substance, resisting the action of boiling alkalies, cannot be compared to fuchsic acid, and if, in M. Hofmann's experiment, this acid could be produced, it would only be found in alkaline solutions, in which it exists but in infinitesimal quantities; further, certain circumstances relating to bulk, temperature, or time are required to allow of the development or the preservation of this dye-stuff.

In fact, by heating for thirty hours, the same mixture which yielded the very decided crimson tints, either in the soluble portion or the residue, we have seen that this coloration does not extend to the products obtained at the temperature of 180°. There is nothing surprising in this result, since we have proved that a mixture of 3 parts of solid fuchsine and 10 parts bichloride of carbon, heated under the before-mentioned conditions, yields only clear yellow liquids; all red matter disappears.

Moreover, by moderating the temperature, the duration of the experiment, and the respective proportions of aniline and bichloride of carbon, we have produced substances decidedly richer in colouring principle than those obtained by M. Hofmann. That the fuchsine exists is unquestionable, but the point is to seize upon it at the moment of its production. It is, moreover, accompanied by Hofmann's red substance, which predominates, and is distinguished from it by its insolubility in potash.

We have naturally been led by these observations to know what should result on mixing, according to Hofmann's plan, anhydrous bichloride of tin and aniline, which forms Lyons red.

Nine grammes of bichloride of tin and 16 grammes of aniline, heated for 30 hours in a sealed tube, at about 180°, yields, instead of red or violet, a very bright and pure blue, which requires only to be treated with water to dye animal fibres in beautiful brilliant tints. This blue, which resists acids, is deepened by feeble alkalies, and assumes a gooseberry-purple with concentrated alkalies. As its tint and purity are not influenced by artificial light, manufacturers will not fail to take advantage of it. We give it the name of "Paris blue," and it is another addition to the very remarkable series of rich colours derived from aniline.—*Comptes-Rendus,* Vol. lii.

PHARMACY, TOXICOLOGY, &c.

Poisoning by Cyanide of Potassium,

by E. R. Southby.

The following is the history of a case as given me by Mr. P. Belcher, the medical man called in on March 23, 1861:—

"Ann W., aged 17, a domestic servant in a photographer's household, had been dead when seen by me about half-an-hour. Body getting cold, very livid, rigor mortis slightly perceptible in the legs; a slight smell of brandy from the mouth; tongue ecchymosed at the tip. The following account was given by eye-witnesses of the attack:—

"Deceased had been slightly indisposed last evening, refused supper, and went to bed; came down in the morning apparently well, took her breakfast, but refused dinner; did her household work as usual till four o'clock in the afternoon, was then sent by her mistress into the portrait-room (immediately adjoining the dark room in which the chemicals were kept) to light the fire; returned in a few minutes, and busied herself in getting the tea ready. Her mistress' attention was now attracted by her extraordinary attitude. She stood with a fixed stare, face twitching, and her hands hanging down from the wrists as if paralysed. Her mistress ran to her, caught her in her arms in the act of falling, and put her in an arm-chair. Now followed a severe attack of general convulsions, with froth from the mouth and nose, supposed by her and other witnesses to be an epileptic seizure. Some brandy was administered, of which she swallowed a little with difficulty, and appeared for some time to be rallying; the convulsions, however, returned; she suddenly stiffened her legs, gave a few gasps at long intervals, and died. The above attack seems to have lasted about twenty minutes, during all which time she neither spoke nor screamed. It appears, also, that the period from the time of her taking the poison—viz., when she was sent to the portrait-room till she was first seized—could not have exceeded ten minutes; so that the whole period from taking the poison till death ensued was between twenty-five and thirty minutes. Some additional evidence was elicited at the inquest, viz.—that she had had a quarrel with her lover the evening before, and that one of the lumps of cyanide in his stock bottle was observed by her master to have had a portion recently broken off."

Post mortem Examination, by Messrs. P. and S. Belcher.

"March 25.—Forty-one hours after death.—Rigor mortis continued. Integuments uncommonly fatty. Head: brain and membranes healthy; venous congestion. Thorax: Lungs healthy; heart enlarged; right side dilated, the walls thin, and those of the auricle translucent, except where crossed by the musculi pectinati; valves healthy. Left side of the heart healthy. Abdomen: stomach distended with gas; contained half-digested food, having only the usual smell. When removed from the body and opened, the mucous membrane was found to be in a state of intense inflammation, being generally very red, and easily peeled off with the nail; and in two places on the cardiac portion patches of a dusky hue, about four inches in circumference, were seen. Other viscera healthy; pelvic viscera, uterus and ovaries, healthy and unimpregnated."

Examination of the Contents of the Stomach.

The contents of the stomach, amounting to about four ounces, consisted of a grayish, viscid fluid, with portions of undigested food floating in it. Reaction slightly alkaline.

About two ounces were acidulated with sulphuric acid and distilled in a glass retort till about half had passed over. During the distillation the smell of prussic acid was perceptible, but a good deal masked by the stench.

About one drachm of the distillate was boiled with a little sulphide of ammonium till the smell of the latter had disappeared. On addition of a few drops of sesquichloride of iron, the deep red colour of the sesquisulphocyanide of iron appeared, this colour disappearing completely on addition of a solution of bichloride of mercury.

Another drachm of the distillate was rendered slightly alkaline by solution of potassa, a small crystal of protosulphate of iron added, and the whole boiled. A drop of sesquichloride of iron solution was then added, and the mixture acidulated with hydrochloric acid. A blue colour was produced, disappearing on addition of solution of potassa in excess, and re-appearing on acidulating with hydrochloric acid. On standing for twelve hours, the Prussian blue was deposited.

The residue left in the retort was evaporated to dryness, ignited till a greater portion of the carbon was driven off, and digested with hot distilled water, and the solution filtered. On addition of hydrochloric acid, alcohol, and bichloride of platinum, a yellow, crystalline precipitate, seen under the microscope to consist of the octahedra of the double chloride of platinum and potassium.

I therefore concluded that the poison taken was cyanide of potassium.

The above analysis was made about fifty-two hours after death. No smell of prussic acid was perceived, either immediately after death or during the *post mortem*.

PHYSICAL SCIENCE.

Early Researches on the Spectra of Artificial Light from Different Sources.

Some Experiments on Coloured Flames,[1] *by* H. F. Talbot, *Esq.*

Great progress has recently been made in investigating the properties of light, and yet many of them are still unexamined or imperfectly explained. Among these are the colours of flames, which not only appear very various to common observation, but are shown by the assistance of a prism to be entirely different in nature one from another; some being *homogeneous*, or only containing one kind of light, others consisting of an infinite variety of all possible shades of colour.

1. It was discovered by Dr. Brewster that the flame of alcohol, diluted with water, consists chiefly of homogeneous yellow rays. On this principle he proposed the construction of a monochromatic lamp, and pointed out its advantages for observations with the microscope. This must be considered a very valuable discovery. The light of such a lamp, however, is weak unless the alcohol flame is very large. I have, therefore, made several attempts to obtain a brighter light, and I think the following is the most convenient method. A cotton wick is soaked in a solution of salt, and when dried placed in a spirit-lamp. It gives an abundance of yellow light for a long time. A lamp with ten of these wicks gave a light little inferior to a wax candle; its effects upon all surrounding objects was very remarkable, especially upon such as were red, which became of different shades of brown and dull yellow. A scarlet poppy was changed to yellow, and the beautiful red colour of the *lobelia fulgens* appeared entirely black. The wicks were arranged in a line, in order to unite their effect for a microscope. A common blue glass has the property of absorbing the yellow light of this lamp, however brilliant, while it transmits the feeble violet rays. If these are also stopped by a pale yellow glass the lamp becomes absolutely invisible, though a candle is seen distinctly through the same glasses. But the most remarkable quality of this light is its homogeneity, which is perfect as far as I have been able to ascertain; I speak of the yellow rays, which form the mass of the light, and quite overpower the feeble effects of the blue and green. The origin of this homogeneous light appears to me difficult to explain. I have found that the same effect takes place whether the wick of the lamp is steeped in the muriate, sulphate, or carbonate of *soda*, while the nitrate, chlorate, sulphate, and carbonate of *potash* agree in giving a bluish-white tinge to the flame. Hence the yellow rays may indicate the presence of *soda*, but they nevertheless frequently appear where no soda can be supposed to be present.

2. Mr. Herschel discovered that sulphur when burning intensely gives a homogeneous yellow light. To examine it, I inflame a mixture of sulphur and nitre behind a screen having a narrow vertical slit through which the flame could be seen. This opening, examined with a prism, gave a spectrum in which there was a very bright yellow line, indicating the combustion of the sulphur. I thought it a point of considerable interest to determine whether this yellow ray was identical with that afforded by the flame of alcohol containing salt, and with that view I placed such a flame behind the other, their light passing through the same opening, so that if the rays were of a different nature two yellow lines should be seen in the spectrum; but if identical, then only one. I found upon trial that the rays coincided, and I obtained a further confirmation of this by inflaming the nitre and sulphur mixed up with a quantity of salt, the effect of which was not to produce a second yellow line in the spectrum, but to increase greatly the brilliancy of the original one. The result of this experiment points out a very singular optical analogy between soda and sulphur, bodies hitherto supposed by chemists to have nothing in common.

3. There are other means of procuring the same light, which I shall briefly mention. If a clean piece of platina foil is held in the blue or lower part of a gas-flame it produces no change in the flame; but if the platina has been touched by the hand it gives off a yellow light, which lasts a minute or more. If it has been slightly rubbed with soap, the light is much more abundant, while wax, on the contrary, produces none. Salt sprinkled on the platina gives yellow light while it decrepitates, and the effect may be renewed at pleasure by wetting it. This circumstance led me to suppose that the yellow light was owing to the water of crystallisation rather than to the soda; but then it is not easy to explain why the salts of potash, &c., should not produce it likewise. Wood, ivory, paper, &c., when placed in the gas-flame, give off, besides their bright flame, more or less of this yellow light, which I have always found

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[1] From Brewster's "Journal of Science," vol. v. 1826.

the same in its characters. The only principle which these various bodies have in common with the salts of soda is *water;* yet I think that the formation or presence of water cannot be the origin of this yellow light, because ignited sulphur produces *the very same,*—a substance with which water is supposed to have no analogy.[1] It is also remarkable that alcohol burnt in an open vessel, or in a lamp with a metallic wick, gives but little of the yellow light; while if the wick be of cotton, it gives a considerable quantity, and that for an unlimited time. (I have found other instances of a change of colour in flames owing to the *mere presence* of a substance which suffers *no diminution in consequence.* Thus, a particle of muriate of lime on the wick of a spirit-lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished.) The bright flame of a candle is surrounded by the same homogeneous yellow light which becomes visible when the flame itself is screened. The following experiment shows its nature more evidently:—If some oil be dropped on the wick of a spirit-lamp, the flame assumes the brilliancy of a candle, surrounded by an exterior yellow flame. This appearance only lasts until the oil is consumed.

4. The flame of sulphur and nitre contains a red ray, which appears to me of a remarkable nature. While examining the yellow line in the spectrum of this flame, I perceived another line, situated beyond the red end of the spectrum, from the termination of which it is separated by a wide interval of darkness. In colour it nevertheless differs but little from the rays which usually terminate the spectrum. It arises, I believe, from the combustion of the nitre, as the yellow ray does from that of the sulphur; for I have since observed it in the flame of a spirit-lamp whose wick had been soaked in nitre or chlorate of potash. It appeared that this ray was so distant from the rest that it might be less refrangible than any in solar light; and I have been since informed by Mr. Herschel that he had already observed it in a similar experiment, and was impressed with the same idea.

With the hope of establishing this, I admitted candle-light and that of the nitre lamp, which I have just mentioned, through the same aperture, and noticed how far this isolated red ray appeared beyond the spectrum of the candle. I then compared, in the same way, the light of the candle with that of the sun, and I found that the great intensity of the solar light lengthened the red end of the spectrum about as far; so that I was obliged to leave the question undecided, as the faintness of the lamp prevented my comparing it directly with the sun. This red ray appears to possess a definite refrangibility, and to be characteristic of the salts of potash, as the yellow ray is of the salts of soda, although, from its feeble illuminating power, it is only to be detected with a prism. If this should be admitted, I would further suggest that whenever the prism shows a *homogeneous* ray of any colour to exist in a flame, this ray indicates the formation or the presence of a *definite chemical compound.* An excellent prism is, however, requisite to determine the perfect homogeneity of a ray.

5. Phosphorus inflamed with nitre gives a very brilliant spectrum, in which no colour appears to be predominant or deficient. It, therefore, resembles the spectra of ignited lime, platina, and other solid bodies, and differs totally from the solar spectrum, in which there are now known to be innumerable interruptions of light. And it is worthy of remark that no light has been hitherto discovered at all resembling that of the sun (when analysed with a prism), except the light of the other celestial bodies.

6. The red fire of the theatres, examined in the same way, gave a most beautiful spectrum, with many white lines or maxima of light. In the red these lines were numerous and crowded with dark spaces between, besides an exterior ray, greatly separated from the rest, and probably the effect of the nitre in the composition. In the orange was one bright line, one in the yellow, three in the green, a very bright one in the blue, and several that were fainter. The bright line in the yellow is caused, without doubt, by the combustion of the sulphur, and the others may be attributed to the antimony, strontia, &c., which enter into this composition. For instance, the orange ray may be the effect of the strontia, since Mr. Herschel found in the flame of muriate of strontia a ray of that colour.[2] If this opinion should be correct, and applicable to the other definite rays, a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect.

London, March, 1826.

On a Method of Obtaining Homogeneous Light of Great Intensity,[3] *by* H. F. Talbot, *Esq.*

As it is a desideratum in optical science to procure *perfectly* homogeneous light of sufficient brightness for many important experiments, I am glad to be able to communicate a method which, in a satisfactory manner, supplies that deficiency. It is only requisite to place a lump of common salt upon the wick of a spirit-lamp and to direct a stream of oxygen gas from a blowpipe upon the salt. The light emitted is quite homogeneous and of dazzling brightness. If, instead of common salt, we use the various salts of strontium, barytes, &c., we obtain the well-known coloured flames which are characteristic of those substances with far more brilliancy than by any other method with which I am acquainted.

On the Flame of Lithia,[4] *by* H. F. Talbot, *Esq.*

Lithia and strontia are two bodies characterised by the fine red tint which they communicate to flame. The former of these is very rare, and I was indebted to my friend Mr. Faraday for the specimen which I subjected to prismatic analysis. Now, it is difficult to distinguish the lithia red from the strontia red by the unassisted eye; but the prism displays between them the most marked distinction that can be imagined. The strontia flame exhibits a great number of red rays, well separated from each other by dark intervals, not to mention an orange, and a very definite bright blue ray. The lithia exhibits one single red ray. *Hence, I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method.*

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[1] It may be worth remark, though probably accidental, that the specific gravity of sulphur is 1·99, or almost *exactly twice* that of water.

[2] *Edinburgh Transactions,* vol. ix. p. 456.

[3] *London and Edinburgh Philosophical Magazine and Journal of Science,* Third Series, 1833, vol. iii. p. 35.

[4] *London and Edinburgh Philosophical Magazine and Journal of Science,* Third Series, 1834, vol. iv. p. 114.

On the Flame of Cyanogen.

For the opportunity of examining the optical characters of this flame I am likewise obliged to the kindness of Mr. Faraday, who showed it both to Sir J. Herschel and myself at the Royal Institution. When viewed with a prism, this flame presents a very distinct and peculiar character, separating the violet end of the spectrum into three portions, with broad, dark intervals between. But the most remarkable fact is this, that the last of these portions is so widely separated from the others as to induce a suspicion that it may be more refracted than any rays in the solar spectrum,—a question which I should be glad to have the opportunity of deciding by direct experiment. This separate portion has a pale, undecided hue. I should hardly have called it violet, were it not situated at the violet end of the spectrum. To my eye it had a somewhat whitish or grayish appearance.

On Prismatic Spectra,[1] *by* H. F. Talbot, *Esq.*

It is much to be desired that an extensive course of experiments should be made on the spectra of chemical flames, accompanied with accurate measurements of the relative position of the bright and dark lines, or *maxima* and *minima* of light which are generally seen in them. The definite rays emitted by certain substances, as for example, the yellow rays of the salts of soda, possess a fixed and invariable character, which is analogous in some measure to the fixed proportion in which all bodies combine, according to the atomic theory. It may be expected, therefore, that optical researches, carefully conducted, may throw some additional light upon chemistry. Some experiments which I formerly made upon this subject will be found in "Brewster's Journal," for 1826, vol. v. p. 77. In addition to the substances there enumerated, as giving a peculiar optical character to flame, I have found that the salts of copper are exceedingly remarkable. They give spectra so covered throughout with dark lines as to resemble in that respect the solar spectrum. The flames of boracic acid and nitrate of barytes also possess somewhat of a similar character. The most convenient way of obtaining brilliant spectra of these substances is to deflagrate them with chlorate of potash; but this is attended with the inconvenience that the spectrum produced by the chlorate is seen in conjunction with the other, and an allowance must be made accordingly. Another good method is to sprinkle the substance in powder on the wick of a spirit-lamp, and direct a current of oxygen upon it. With regard to the *accurate* measurement of the lines, it requires the use of very superior apparatus. I have sometimes made approximate measurements by fixing a divided scale transversely to the linear aperture through which the light of the burning body was observed. This aperture was then expanded by the prism into a spectrum parallel to the scale, by means of which it could then be measured. An objection may, perhaps, occur to the reader, that the scale would thus be as much refracted as the light itself, and therefore could not serve to measure it; but this difficulty was avoided by a simple contrivance, viz., by illuminating the scale with homogeneous light.

Spectra of Various Galvanic Flames.—Silver leaf, deflagrated by galvanism, gave a spectrum with several definite rays, among which two green rays appeared to me to possess nearly the same tint, although differing in refrangibility. Gold leaf and copper leaf each afforded a fine spectrum, exhibiting peculiar definite rays. The effect of zinc was still more interesting; I observed in this instance a strong red ray, and three blue rays, besides several more of other colours. These experiments were made in the laboratory of the Royal Institution, in June, 1834.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting.

On the Nature of the Deep-sea Bed, and the Presence of Animal Life at vast Depths in the Ocean, by Dr. G. C. Wallich.

Our first clear glance at the floor of the ocean may be said to date from the period at which submarine telegraphy was first undertaken. For although the depth of the sea has been approximately ascertained over widely extended areas, in the course of the various surveys conducted under the auspices of the British, the United States, and the Dutch Governments, hardly any previous attempts have been made systematically to investigate the characters and composition of its bed. In the absence of any special object, such attempts would have been far too costly and difficult to be practicable. It has been ascertained, however, that the floor of the ocean is but the reflex, as it were, of the dry land; that it is in no place unfathomable; that along its deeper portions certain muddy deposits are to be met with, in many cases, made up more or less entirely of minute calcareous shells belonging to one of the most simple order of beings with which we are acquainted; and that together with these are also to be found, but in, comparatively speaking, small quantity, the minute flinty skeletons of other organisms derived both from the animal and vegetable kingdoms. But no conclusive evidence has been produced to show whether any or all of these organisms normally lived and perished, at the profound depths from whence they were obtained by the sounding lead; or whether, having inhabited distant, and perhaps shallower seas, their dead remains alone, after being transported by currents, or other agencies, had gradually subsided into the deep hollows of the ocean. Taking into consideration the very important part played by these organisms in the structure of the earth's crust, that vast strata have in ages gone by been built up of them, and that similar strata are at the present time being deposited along the beds of existing seas, the investigation of these questions becomes of the highest consequence, as bearing on the successful establishment of ocean telegraphy.

The distribution of animal life in the upper waters, of the sea is determined by climate, by the composition of its waters, the nature of its bed, and its depth in any given locality; the last of these items necessarily involving the relative degrees of temperature, light, aëration, and pressure, as compared with those to be met with near the surface. Of these conditions climate exercises a very powerful influence; for it is found as we advance from the equator towards the poles that a gradual diminution takes place, not only in the number of types met with, but of the varieties ranged under those types. It has been maintained, that in order to compensate for the diminution in the number of generic forms, the number of individuals of each species is much augmented. Although this law holds good as regards the higher orders, it can hardly be said to do so in the case of the lower; for the vast assemblages of these lower forms met with on the surface of the sea in the tropics, are in no wise less extensive than those met with in high latitudes. It will be found that the lower the grade of being, the more equally balanced will be its distribution, at the extremes of the globe; inasmuch as the greater range in depth commanded by these lower forms renders them less amenable to conditions which are variable from being dependent on atmospheric changes.

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[1] *London and Edinburgh Philosophical Magazine, and Journal of Science*, Third Series, 1836, vol. ix. p. 3.

The composition of the waters of the ocean is well known to become much more equable at great depths; and it therefore, exercises a far less marked influence on the presence of animal life than it does at the surface. The same causes which equalize the temperature in so remarkable a manner as the depth increases, are effective in equalizing the relative proportions of the various ingredients that enter into the composition of sea water, in all latitudes. For, whilst the surface stratum is subject to dilution with fresh water, from various sources, the greater the depths, the less subject can the waters be to this influence, and the less can it operate in modifying the distribution of the organisms that frequent them.

Oxygen is essential to the presence of animal life—without it animal life ceases. To air-breathing, as well as water-breathing creatures a due supply of this gas is indispensable; the function of respiration, no matter whether performed by lungs, as in man and the higher orders, or by a simple process of absorption and exudation through the general surface of the body, as in some of the lower forms, being, in every instance, essentially that process whereby oxygen is received into the system in exchange for carbonic acid which is given off. But although oxygen enters largely into the composition of both atmospheric air and water, the supply of this element is not obtained, in the case of creatures inhabiting the sea, under ordinary circumstances, from its decomposition, but from a certain portion of atmospheric air present in water in a state of solution. Most gases are absorbed by water. Under pressure, the quantity absorbed is much increased, as is seen in the familiar case of soda-water. It should be borne in mind, however, when the fact is applied to the occurrence of animal life at great depths in the sea, that in order to produce the absorption of atmospheric air, its contact or mixing together at the surface by the action of wind and wave is necessary, and the effect of this operation can only extend to a limited depth, unless, as has been assumed by some of our highest authorities, the lower strata of sea water, being subject to increased pressure, become capable of holding in solution a greater quantity of oxygen; and, by robbing the superincumbent strata of that which they contain, gradually become saturated with it. Should this view be correct, there must be a point at which the maximum amount of oxygen, which sea water can absorb, is permanently present in it. But, inasmuch as the vegetable cell, simple though it be in structure, can eliminate carbon from the medium in which it lives, it is not unreasonable to assume that the lowest forms of animal life, even where no specialized organs are traceable, may, in like manner, be able to eliminate oxygen directly from the water around them.

The temperature of the sea is materially influenced by the climatic conditions of different latitudes; and, of course, exercises a powerful effect both on the distribution and abundance of the higher orders of living beings present in its waters. But, as has been shown, this influence is not manifest, or at all events, not so manifest in the lower orders, for at great depths the variability of the temperatue is reduced within very narrow limits in all latitudes. Now the higher orders of oceanic creatures inhabit only the surface waters, never sinking down to extreme depths. In the case of some of the lower forms, on the other hand, a very extended pathymetrical range exists, putting out of the question those which constantly dwell on the sea bed itself, of which I shall presently have to speak.

In like manner, Light, or rather the absence of it, can hardly be said to determine, in any important degree, the distribution and limitation of the lower forms of animal life. Light is not essential even in the case of some of the higher orders. A large class of creatures, both terrestrial and marine, possess no true organs of vision, although there is good reason for believing that they do possess some special sensory apparatus, susceptible to the influence of light; whilst certain creatures, whose habitation is in subterranean caves or lakes, as in the Magdalena caves, near Adelsberg, and the Great Mammoth caves in Kentucky, either possess no organs of vision, or possess them in so rudimentary a state, as to prove clearly that the absence or imperfect development of this sense may be compensated for by the higher development of other senses.

It is impossible at present to say to what depth light penetrates in the sea. The photographic art will, no doubt one day solve the problem. But it is almost certain that a limit is attained, and that moreover long before the deep recesses gauged by the sounding machine are reached, where the light-giving portion of the ray cannot penetrate even in its most attenuated condition; and yet, as shall hereafter be shown, creatures have been found down in those profound and dark abysses, whose colouring is as delicate and varied as if they had passed their existence under the bright influence of a summer sun!

Pressure is the last condition which has to be noticed. Although undoubtedly a highly important one, I hope to be able to prove that it is not of essential value as has heretofore been laid down, in determining the final limit of animal life in the sea.

It is almost needless to state that at the sea level there exists a pressure of 15 lbs. on every square inch of surface, due to the weight of the atmospheric column resting upon it; and that the pressure on the successive strata of water in the sea, as the depth increases, is infinitely in excess of this, inasmuch as a column of water only 33 feet in height is capable of counterbalancing the entire atmospheric column, which extends to a height of about 45 miles. Accordingly, for every 33 feet of descent in the sea, putting out of consideration the effect of the superincumbent column in actually diminshing the bulk of the portions beneath by augumenting their density, there is an additional 15 lbs. At great depths, therefore, the aggregate pressure becomes stupendous. As is well known, pieces of light wood let down to a depth 1500 or 2000 fathoms, become so compressed and surcharged with water as to be too heavy to float. But there is a fallacy in this experiment; for the contraction of the woody fibres and cells is a necessary consequence of their submission to an amount of pressure so enormously in excess of that under which they originated. With organisms which have been developed, from first to last, under the full operation of any given amount of pressure, the result would not be of this nature; for the equalization of the pressure within and without their entire structure, although it might possibly exercise some definite effect in determining their shape, size, or even functions, cannot, I submit, operate in causing the creatures living under it to experience any more detrimental results than we experience from the 15 lbs. on every square inch, or about 14 tons, on the general surface of our bodies near the sea level.

It can scarcely be wondered at that under such apparently extraordinary conditions, the maintenance of life, even in its least developed aspects, should have been deemed absolutely impossible at extreme depths; and that it should have been almost unanimously recognized as an axiom, that at a depth of 400, or, at most, 500 fathoms, life, whether animal or vegetable, must be extinct. The fact is unquestionable that as we descend beyond the first hundred fathoms, the traces of life become more and more remote; and it is probably owing to this gradual diminution in the number of animal forms, as the depth exceeds this limit that it has been assumed, rather as a matter of theory than of observation, that a point is speedily reached at which all the conditions essential to life are extinguished.

This view has also derived support from the idea that "animal life depends on the previous existence of vegetable life." In the case of the higher orders of the animal kingdom, the law, no doubt, holds good. Not so, however, in the case of the lower. The conditions essential to the perpetuation of the one are not essential to the perpetuation of the other. Thus, light is indispensable for the healthy respiration and growth of the vegetable. The animal can, on the other hand, respire as freely in the blackest darkness as in the broad glare of day. And this is, no doubt, the reason why vegetable life in the ocean attains its final limit in depth so much sooner than animal life. And yet, considering how very unexpectedly animal life has been proved to exist deep down in the ocean—as I shall immediately show far removed beyond those conditions which had hitherto been considered indispensable—we ought perhaps to pause before we assert that the same plastic skill which has so constituted certain creatures as to admit of their inhabiting the deep abysses of the ocean, may not, in like manner, have so constituted some of the vegetable organisms as to be capable of living under similar conditions.

The *Foraminifera* are the organisms to which reference has been made as performing so very important a part in the formation of certain strata on the earth's crust. They occur abundantly in all existing seas. They are to be met with in a fossil state, not only in chalk, but in almost all marine sedimentary strata; as for instance, in the hard limestones and marbles. The recent Foraminifera may therefore be looked upon as the oldest living representatives of any known class of organisms.

In the mud or "ooze," as it has been termed, which is brought up from great depths in many parts of the open sea, immense assemblages of Foraminifera are to be met with, chiefly belonging to one species however. In the absence of examinations conducted immediately on their being brought up to the surface by the sounding machine, it is not surprising that the question as to their occurrence in a living, or only in a dead state, should have remained undecided. Most of the authorities who have written on the subject being of opinion that they do not live at great depths, but that their shells and remains have drifted to the positions in which they were found from shallower waters, or have subsided from the upper strata of the ocean. Professor Huxley was one of the very few who leant to the more correct opinion; he having declared, that although far from regarding it as proved that the *Globigerina* (the species referred to) live at these depths, the balance of probabilities seemed to him to incline in that direction. Other writers have offered surmises on the subject; but these, in the absence of anything like substantial proofs, were, of course, only estimated at what they were worth.

The difficulty is how to determine the point conclusively. For it seems legitimate to infer, that if these organisms are specially adapted to exist under conditions differing so widely from those present at or near the surface, the very circumstance of removing them from one set of conditions to the other, would inevitably destroy their vitality, and perhaps their normal structure, before it could become practicable to subject them to microscopic analysis. Nor is the difficulty an imaginary one. For, taking into consideration the entirely altered circumstances in which these creatures must find themselves placed when brought to the surface, locomotion, or even the protrusion of their filamentary appendages, could hardly be expected. The mere existence of the fleshy parts within their shells, and that too in an apparently recent condition, affords no proof, inasmuch as the great quantity of saline matter present in sea-water, and especially at great depths, would of itself alone account for their perfect state of preservation.

During the recent survey of the North Atlantic, I found that in certain localities, where the Globigerina deposit was of the purest kind and in the greatest plenty, the specimens from the immediate surface stratum of the sea-bed alone retained their normal appearance, both as regards the perfect state of the sarcodic contents of the shells and the presence of the pseudopodia. The latter organs were never seen by me in an extended condition; but in the specimens alluded to, and in those only, occurred as minute bosses, resembling in shape the rounded rivet-heads on boilers, closely appressed to the external surface of the shell; whereas, in specimens from the substratum, the colour was much duskier, and these bosses were absent. And further, in these pure deposits the shells were to be found in every gradation, from the single chamber, of microscopic minuteness, hyaline transparency, and extreme thinness, to the dense Zeolite-like structure of the many-chambered mature shells, which are large enough to be readily distinguished by the naked eye. These facts, when taken in conjunction with the entire absence of the varied remains of other organized structures found in localities where the Globigerinæ are only scantily represented, afford, as I conceive, all but the direct proof, which can only be arrived at on witnessing locomotion, or the protrusion and retraction of the pseudopodia of the organisms in question.

Most fortunately, as it happened, this collateral evidence was rendered doubly conclusive by other proofs of a most unexpected and interesting kind. Before entering on these, I may state that the substratum, spoken of as differing in aspect from the immediate surface-layer, is nevertheless identical in composition; the difference in colour arising simply from decay. It contains no living Foraminifera; for the minute particles of matter becoming gradually condensed and aggregated together by molecular affinity, and the enormous superincumbent pressure exerting itself only in one direction, that is, vertically, its permeability by fluids is thus completely destroyed, and it is compacted into a dense mass of far too unyielding a nature to admit of its being traversed by living creatures of any kind. As the Foraminifera die off, their shells and decaying contents, together with the minute particles of amorphous matter associated with them, go to build up the calcareous strata of the earth's crust. I would mention, that in order to determine whether the *Globigerinæ* live as free floating forms in the mid strata of water, I attached a small open-mouthed bag, at about 200 fathoms from the extreme end of the sounding line, in a locality where the species was most abundant in the deposit, and brought it up through nearly 5,000 feet of water without securing a single shell.

But by far the most important and interesting discovery remains to be noticed, namely, the detection of a high order of radiate animal, in a living state, at a depth of a mile and a-half below the surface of the sea.

When we take into consideration the low position of the Rhizopod in the scale of being, and the obvious probability, pointed out by Professor Huxley, that a class of creatures proved to extend so far back in time—that is, in a fossil state—must be able to maintain existence under extraordinary and variable conditions as regards light, temperature, and pressure, the sentiment engendered is rather one of wonder, that their vitality at great depths should have been so long and so stoutly maintained, than that it should now be so fully proved. But few persons were bold enough to suspect that creatures of a far higher type, namely, Radiata, could exist under similar conditions; and I freely admit, that nothing short of the most incontrovertible proof ought to be accepted in support of such a view. Fortunately, I am in a position to afford that proof.

In sounding midway, in the direct line between Cape Farewell, the southern point of Greenland, and the north-west coast of Ireland, in lat. 59° 27′ N., and long. 26° 41′ W., the depth being 1260 fathoms (or 2520 yards), whilst the sounding apparatus itself brought up a considerable quantity of minute granular particles, looking like a fine oolite, but which was, in reality, a nearly perfectly pure

Globigerina deposit, 13 star-fishes, from 2 to 5 inches in diameter from tip to tip of rays belonging to a genus plentifully represented on our own coasts, came up adhering to the extreme 50 fathoms of sounding line. These *Ophiocomæ* were not only alive on being brought up out of the water, but some of them continued for fully a quarter of an hour to move about their long spinous arms. To render intelligible the significancy of the entire circumstances, I must mention, that in order to ensure accuracy, it is always necessary, when sounding in deep water, to ascertain the depth by one sort of apparatus, and to bring up the sample of bottom by another. In the present case, the ascertained depth was 1260 fathoms, and 50 fathoms was accordingly "paid out" in the second operation of bringing up bottom, in order to make sure that the more complicated and unmanageable apparatus required for this purpose fairly rested on the bottom.

Now, supposing it possible that these star-fishes were drifting about in some intermediate stratum of water, between the bottom and surface, it is evident that they would have attached themselves indiscriminately to any portion of the entire 1260 fathoms of line; unless, indeed, they chanced to have been directing their course in a closely compacted column, which was traversed by the last *extra* 50 fathoms of line at the precise moment of their crossing it. Whether it be possible that they were drifting in such a column, or floating on a bed of seaweed or other substance, is immaterial, inasmuch as they could only have attached themselves as they did to the portion of line referred to under this one condition. But the very act of attachment would, I maintain, be impossible in the case of creatures whose movements are so sluggish, when the object which they had to grasp was moving upwards at the rate of two miles per hour (as it does when hauled up by the steam-engine), and without a moment's intermission. But even assuming it to be possible that they had drifted to the position in which they were captured, from distant and less profound depths, the fact of their vitality and vigorously healthy condition would be scarcely less extraordinary; for the distance from the nearest point of land, which is a rock off Iceland, is 250 miles; whilst the next nearest land, Greenland, is distant no less than 500 miles. But it must be obvious to every one who is at all conversant with the structure of the *Ophiocomæ* and Echinoderms generally, that they are essentially creeping and crawling creatures, and of far too great specific gravity to float at all, under any circumstances.

Taking into consideration, then, the circumstances under which these *Ophiocomæ* were taken, the extreme improbability of their having drifted to the locality in which they were found, from distant and shallower waters; and lastly, the peculiarities of structure, which render them wholly unfit to float or swim for even a brief period, we should have been fully warranted, I think, in believing that they existed in a living state at the bottom. In order to obtain some clue to the solution of the question, I very carefully dissected and analysed the contents of the digestive cavity of a specimen, immediately on its being brought up; and was most amply repaid by the detection of numerous *Globigerinæ* in every stage of comminution, and with the contained sarcodic matter in greater or lesser quantity. Whilst, therefore, the detection of these organisms in the digestive cavities of the *Ophiocomæ*, afforded a most conclusive proof that the Foraminifera were living on the sea bed at the profound depth from which they were obtained; the fact of the star-fishes being captured with the fresh remains of the Foraminifera in their digestive cavities, proves that their normal habitation is at the same great depth, inasmuch as it has been sufficiently established that the Globigerina are present *only* at the bottom. I may mention that, within the past few days, in examining a sample of the Globigerina deposit brought up by a previous sounding on the same spot, I detected some Echinoderm spines, which at once struck me as being identical with those on the Ophiocomæ; and that, on comparison, my surmise proved to be quite correct: a further and very striking proof of the vitality of the Ophiocomæ at the bottom being thus afforded.

ROYAL INSTITUTION OF GREAT BRITAIN.

WILLIAM ROBERT GROVE, *Esq., Q.C., F.R.S., Vice-President, in the Chair.*

On the Action of Gases and Vapours on Radiant Heat, by JOHN TYNDALL, *Esq., F.R.S., Professor of Natural Philosophy, Royal Institution.*

THE discourse commenced by a reference to the researches of Leslie, Forbes, and Knoblauch; but more especially to the admirable investigations of Melloni on radiant heat. These eminent men had left the gaseous form of matter practically untouched, and to extend our knowledge into this wide region was the object of the investigation on which the present discourse was founded.

The apparatus made use of, and which was applied in the experiments of the evening, consists of the following parts:—

1. A copper cube, C, containing water kept constantly boiling, and one of whose faces, coated with lamp black, forms the source of radiant heat.

2. A brass tube, 2·4 inches in diameter, which is divided into two portions, α and β.

α. The portion of the tube intended to receive the gases and vapours; it is stopped air-tight at its two ends by plates of rock-salt, and is attached to a good air-pump, by which it can be exhausted at pleasure. The length is 4 feet.

β. An air-tight chamber between the tube α and the cube C. It is kept constantly exhausted, and the calorific rays therefore pass from the radiating plate through a vacuum into the tube, thus retaining the quality which belonged to them at the moment of emission.

To prevent the transmission of heat by conduction from the cube C to the tube α, the chamber β is partly embraced by an annular space, in which cold water continually circulates.

3. A thermo-electric pile, furnished with two conical reflectors, and connected with an excellent galvanometer. One of the faces of the pile receives the rays which have passed through the tube α.

4. A second copper cube C′, also filled with boiling water, and whose rays fall upon the second face of the thermo-electric pile. The two cubes C and C′, thus radiating upon the opposite faces of the pile, tend, of course, to neutralise each other.

Between the cube C′ and the adjacent face of the pile a screen S is introduced, being attached to an apparatus of Ruhmkorff's, capable of extremely fine motion; by the partial advance or withdrawal of this screen the two sources of heat can be caused to neutralise each other perfectly.

The tube α and the chamber β being both exhausted, the needle of the galvanometer is brought exactly to zero by means of the screen S. The gas or vapour to be experimented with is now admitted into the tube α, and if it possesses any sensible absorbing power, it will destroy the previously existing equilibrium. The consequent deflection of the galvanometer, properly reduced, is the measure of the absorption. In this way the action of eight gases and thirteen vapours have been examined, and also the action of atmospheric air.

Oxygen, hydrogen, nitrogen, and atmospheric air, respectively absorb about 0·3 per cent. of the calorific rays; this is the feeblest action which has been observed.

The most energetic action is that of olefiant gas, which at the tension of one atmosphere absorbs 81 per cent. of the calorific rays. Between those extremes stand carbonic oxide, carbonic acid, nitrous oxide, and sulphuretted hydrogen.

Below a certain tension, which varies for different gases, the amount of heat absorbed is exactly proportional to the density of the gas. Above this tension the rays on which the principal absorptive energy is exerted become gradually exerted, so that every augmentation of density produces a diminished effect.

In the case of olefiant gas, for example, where a unit measure $\frac{1}{50}$th of a cubic inch in capacity was made use of; for a series of fifteen such measures, the absorption was exactly proportional to the quantity of gas; subsequently, the ratios of the successive absorptions approached gradually to equality. The absorption produced by a single measure of olefiant gas of the above volume moved the index of the galvanometer through an angle of 2·2 deg.; the tension of the gas being only $\frac{1}{11000}$th of an atmosphere.

In the case of vapours, the most energetic is that of sulphuric ether; the least energetic is that of bisulphide of carbon. Comparing small volumes and equal tensions, the absorptive energy of sulphuric ether vapour is ten times that of olefiant gas, and ten thousand times that of oxygen, hydrogen, nitrogen, or atmospheric air.

On a fair November day the aqueous vapour in the atmosphere produced fifteen times the absorption of the true air itself. It is on rays emanating from a source of comparatively low temperature that this great absorbtive energy is exerted. Hence the aqueous vapour of the atmosphere must act powerfully in intercepting terrestrial radiation; its changes in quantity would produce corresponding changes of climate. Subsequent researches must decide whether this *vera causa* is competent to account for the climatal changes which geologic researches reveal.

Oxygen obtained from the electrolysis of water exerted four times the absorptive energy of the same substance when caused to pass through iodide of potassium, the greater action being due to the presence of ozone.

The radiative power of gases was examined by causing them to pass over a heated sphere of metal, and ascend in a column in front of the thermo-electric pile. Various precautions were taken to secure accuracy in the results. It was found that the order of radiation was exactly that of absorption; that any atom or molecule which is capable of accepting motion from agitated ether is capable in precisely the same degree of imparting motion to still ether. Films of gas on surfaces of polished metal were found to act like coats of varnish.

The speaker also investigates the physical connection of radiation, absorption, and conduction. In the foregoing experiments free atoms and molecules were dealt with, and upon them individually was fixed the responsibility of the effects observed. These effects are thus detached from considerations of cohesion and aggregation, which suggest themselves in the case of liquids and solids.

The reciprocity of absorption and radiation is a simple mechanical consequence of the theory of an ether.

But why is one molecule competent to stop or generate a calorific flux so much more powerfully than another? The experiments prompt the following reply:—The elementary gases which have been examined all exhibit extremely feeble powers both of absorption and radiation, in comparison with the compound ones. In the former case we have oscillating atoms, in the latter oscillating systems of atoms. Uniting the atomic theory with the conception of an ether, it follows that the compound molecule which furnishes *points d'appui* to the ether must be capable of accepting and generating motion in a far greater degree than the single atom, which we may figure to our minds as an oscillating sphere. Thus oxygen and hydrogen, which taken separately, or united mechanically, produce a scarcely sensible effect, when united chemically to form oscillating systems as in aqueous vapour, produce a powerful effect. Thus, also, nitrogen and hydrogen, which, when separate or mixed, produce but little action, when combined to form ammonia, produce a great action. So also, nitrogen and oxygen, which when mixed as in air, are feeble absorbers and radiators, when united to oscillating systems, as in nitrous oxide, are very powerful in both capacities. Comparing small volumes and equal tensions, the action of nitrous oxide is 250 times that of air; a fact which perhaps furnishes a stronger presumption than any previously existing, that air is a mixture, and not a compound. Carbonic oxide is about 100 times as powerful as its constituent oxygen; carbonic acid is 150 times as powerful, while olefiant gas, as already remarked, is 1000 times as powerful as its constituent hydrogen. In the case of the hydro-carbon vapours, where the atomic groups attain a higher degree of complexity, the action is even greater than that of olefiant gas.

The speaker also refers to the experiments and observations of Niepce, Angstrom, and Foucault; but more especially to the admirable researches of Kirchhoff and Bunsen, as regards the influence of the period of oscillation on the rate of absorption. He points out how the grouping of atoms to systems in a resisting medium must tend to make their period of oscillations longer, and thus bring them into isochronism with the periods of the obscure radiations made use of in the experiments.

With regard to conduction, the speaker would illustrate his views by reference to two substances—rock-salt and alum. He was once surprised to observe the great length of time required by a heated mass of rock-salt to cool; but this was explained by the experiments of Mr. Balfour Stewart, who shows that rock-salt is an exceedingly feeble radiator. The meaning of this is that the molecules of the salt glide through the ether with small loss of *vis viva*. But the ease of motion which they are thus proved to enjoy must facilitate their mutual collision. The motion of the molecules, instead of being expended on the ether between them, and then communicated in part to the ether external to the mass, is transferred freely from particle to particle; or, in other words, is freely conducted. This *à priori* conclusion is completely verified by the author's experiments, which prove rock-salt to be an excellent conductor. It is quite the reverse with alum. Mr. Balfour Stewart's experiments prove it to be an excellent radiator, and the author's experiments show it to be an extremely bad conductor. Thus it imparts with ease its motion to the ether, and for this very reason finds difficulty in transferring it from particle to particle; its molecules are in fact so constituted that when one of them approaches its neighbour, a swell is produced in the intervening ether; this motion is immediately communicated to the ether outside, and is thus lost for the purposes of conduction. The lateral waste prevents the motion from penetrating the alum to any great extent, and hence it is pronounced a bad conductor. These considerations seem to reduce the phenomena of absorption, radiation, and conduction to the simplest mechanical principles.

CORRESPONDENCE.

Magenta Dye.

To the Editor of the Chemical News.

Sir,—In your Number for April 13, I observe a letter from "Magenta," stating that he has made on a new principle a Magenta powder soluble in water and which will dye either silk, wool, or cotton without any mordant, the powder only requiring hot water poured upon it and filtering to separate insoluble matter.

Now, my experience almost daily both in the manufacture and consumption of the above colour warrants me in asking "Magenta" to excuse me telling him that he is mistaken in having made colouring matter of the above character that can be practicably applied in the dyeing of cotton goods without a mordant having been first applied to the cotton, and without which mordant, cotton will not imbibe more of the colour than a mere tint; indeed cotton has no attraction for the colour until it has received a mordant.

With respect to the silk and wool, both those substances have a powerful attraction for the above colouring without any mordant, the silk in particular; both substances are dyed in quantities in a few minutes without any mordant: by the simple addition of the colour to hot water, both silk and wool will even attract the colour from a weak solution, when cold, in a few hours. I am not aware of another colour that will combine so freely with animal substance when the solution is cold.

In reference to "Magenta's" colour being soluble in water, I may tell him that all colours of this character are soluble in boiling water, no matter which of the metallic salts are used in the manufacture of the colour, water being always used to extract the colour from its carbonaceous impurities, after which it is filtered to separate them: although boiling water holds a large quantity of colour in solution, most of it will precipitate when the aqueous solution becomes cold, the precipitate being of a beautiful green bronze colour. The mere cost of methylated spirit requisite to hold the colour in solution is not worth a consideration when the advantages are compared with the disadvantages. Some of the salts make the colour much more soluble in water than others.

I have tried several of the French Magenta powders and paste, parts of which I have, but have found none that dye silk Magenta, but would also dye wool the same colour; but I fear that attempts to make the colour in England have been more numerous than successful, judging from the amount of patents and the inconsistency of the majority of them, as it would be quite impossible either to produce the very bright colour, or to produce it at a price that would sell it, by simply adhering to the patents.

I am, &c. JAMES SHARP.

Leeds.

Adulteration of Food.

To the Editor of the CHEMICAL NEWS.

SIR,—One day last week some very fine-looking pickled girkins were placed on my dinner table; having partaken of them, a peculiar metallic taste I fancied they possessed induced me to leave a steel fork in the vinegar (from them) all night; the next morning it was found thickly coated with metallic copper. This, I believe, indicates the existence of the metal in poisonous proportions. The bottle from which they were taken bore the label of one of the oldest manufacturers in London.—I am, &c.

CU.

London.

Natural Oxide of Silver.

To the Editor of the CHEMICAL NEWS.

SIR,—Having perused the letters which you have republished from the *Times* and *London Review* on the subject of Oxide of Silver in Cornish Gozzans, and it having come to my knowledge that the fortunate possessor (or possessors) of the gozzans have at the present moment (*i. e.*, say they have) 800*l.* worth of silver in a lot of ore, or gozzan, ready to be operated on by the plan of the equally fortunate gentleman who has discovered the process of obtaining in bulk four times as much precious metal as can be found by the existing old-fashioned method of assay,—I would respectfully suggest that the party who has formed the above estimate will inform your readers whether it is founded on the new or old system of assay. As it follows, if calculated on the new, the ore cannot be worth more than 200*l.*, if sold in the market, while if valued on the old method of assay, it must be worth 3200*l.* by the new system of reduction, if we are to believe that this system yields at least four times as much as those hitherto in use.

An answer to this question before the reduction, with a report of the yield after, will throw more light on this curious question than pages of theoretical discussion. In questions of this nature, an ounce of fact is worth a ton of theory,—and if this matter will bear enquiry, the discoverer of this new process, and the owner or owners of the gozzans, will not fail to adopt the above suggestion, as a simple and expeditious means of putting themselves in a true position with the public.—I am, &c.

ARGENTUM.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

The Polysulphides of the Heavy Metals.—It is generally supposed that alkaline polysulphides give the same precipitates with the heavy metals as a solution of sulphuretted hydrogen. It is not so, however. Berzelius observed that the salts of lead give a red precipitate with the pentasulphide of potassium, and that after a little time the red sulphide blackened, and sulphur separated. He observed a similar phenomenon with the salts of copper. The investigation has been extended by Schiff to all the heavy metals. who gives (*Annalen der Chemie und Pharm.*, bd. cxv. s. 68) the following as the results of his experiments. He precipitated dilute solutions of various salts with an excess of liver of sulphur, or yellow sulphide of ammonium, in an equally dilute solution:—

Salts of Lead.—A deep red precipitate; very unstable, whether in water or alcohol.

Salts of Copper.—A dirty-yellow or yellowish-brown precipitate, which loses sulphur without changing colour much. In alcohol it deposits easily, and may be separated from the greater part of the excess of sulphur. It does not appear to be obtained of a constant composition.

Salts of Silver.—A light yellow precipitate, which becomes first green and then black after standing sufficiently long.

Mercuric Salts.—A light yellow precipitate, soon becoming deeper, the containing liquid becoming opaline.

Mercurous Salts.—A dirty-yellow precipitate, which rapidly turns black.

Salts of Nickel, Cobalt, and Protoxide of Iron.—Black precipitates.

Salts of Peroxide of Iron.—A violet precipitate, becoming instantly yellow, and afterwards taking a dirty tint. Perchloride of iron added, drop by drop, to a solution of liver of sulphur produces a beautiful deep blue colouration, which lasts nearly an hour. A yellow precipitate falls at the same time, which first turns green and then blue. The blue colouration is a little disguised by the sulphide of iron which is separated.

Chromic Salts.—A yellow precipitate, which is immediately decomposed, with the separation of sulphur and the disengagement of HS. The precipitate which remains consists principally of hydrated oxide of chrome.

Salts of the Protoxide of Manganese.—A yellow precipitate, which soon changes into ordinary sulphide of manganese. Heat produces the change immediately.

Bichloride of Tin.—A yellow precipitate, from which sulphur separates immediately, but without change of colour.

Protochloride of Tin.—A liver-coloured precipitate, which in time changes into bisulphide of tin.

Chlorides of Gold and Platinum.—Yellow precipitates, soluble with heat in an excess of liver of sulphur; the reddish-brown solution is precipitated yellow by acids.

Subnitrate of Bismuth.—Liver of sulphur colours it a dark yellow, and, after some time, black.

Salts of Antimony.—A bright yellow precipitate, from which sulphur soon separates. The change into the pentasulphide only takes place slowly in the cold, but rapidly when heated. The precipitate is at first soluble in an excess of liver of sulphur. Acids separate the orange-yellow pentasulphide.

Arsenic and Arsenious Compounds.—In a neutral liquid a yellow precipitate is produced, which is composed of sulphur and pentasulphide of arsenic, and soluble in an excess of the precipitant.

Salts of Zinc.—A white precipitate, sometimes a little yellowish, which, pressed between folds of blotting-paper and dried over sulphuric acid, contains no water, and is of a straw-yellow colour. It dissolves in acids, giving off HS and separating sulphur. When heated, sulphur is disengaged, and there remains 43·2 per cent. of the monosulphide of zinc, mixed with a very small quantity of sulphate. It follows that the precipitate is ZnS_5.

Salts of Cadmium.—A precipitate exactly like the monosulphide when dried, but with a slightly different shade in water. It loses sulphur when heated, and leaves 53·4 per cent. of monosulphide; hence the precipitate is CdS_5.

The tersulphide of potassium gives precipitates analogous to those produced by the pentasulphides.

The Tungstates.—Scheibler writes (*Journal für prakt. Chemie*, bd. lxxx. s. 204) that the tungstates which chemists have hitherto regarded as *bi*tungstates are really salts, whose composition may be expressed by the general formula,—

$$3RO\ 7WO_3 + x\ HO.$$

The author has analysed the tungstate of soda, and found that, when crystallised in the cold, this salt contains $3NaO\ 7WO_3 + 16HO$; when crystallised from a hot solution it has only 14 HO. After calcination, water splits up this salt into two others,—one soluble and alkaline, $2NaO\ 3WO_3$; the other insoluble and crystalline, $NaO\ 4WO_3$.

Tungstate of Potash ($3KO\ 7WO_3 + 6HO$).—Slightly soluble; lammellar crystals.

Tungstate of Ammonia ($3NH_4O\ 7WO_3$).—Crystallises in the cold with 6 molecules of water, and in a hot solution with only 3.

Tungstate of Lithia ($3LiO\ 7WO_3 + [16HO?]$).—Is obtained in beautiful oblique, rhomboidal prisms, which are very soluble.

Metatungstates.—Those salts are produced when a strong acid is added to a soluble tungstate, as long as the tungstic acid is re-dissolved; or better when an alkaline tungstate is boiled for a long time with an excess of tungstic hydrate. The metatungstates are very soluble.

Metatungstate of Ammonia ($NH_4O\ 4WO_3 + 9HO$), according to Margueritte, crystallises in beautiful octahedra. It may be obtained by keeping the dry tungstate of ammonia at 250° to 300°, as long as ammonia is given off, and then re-dissolving in water.

Metatungstates of Potash and Soda have a similar composition to the preceding salt. All three give, by double decomposition with chloride of barium, a salt crystallisable in beautiful octahedra with a square base, $BaO\ 4WO_3 + 9HO$. This salt is decomposable by cold water into an insoluble barytic salt and free metatungstic acid, which will combine again on boiling.

Metatungstic Acid is obtained by precipitating the metatungstate of baryta with sulphuric acid. It has a taste at once strongly acid and bitter. Evaporated in a vacuum over sulphuric acid, it deposits small crystals, which appear to be octahedra, with a square base, and contain $2HO\ WO_3 + 7HO$. Metatungstic acid may be evaporated over a water-bath to a syrupy consistence; but when the concentration by heat is pushed too far, it suddenly changes into yellow, insoluble tungstic acid.

Metatungstic acid and the metatungstates completely precipitate the organic alkaloids. A solution containing only $\frac{1}{200000}$th of quinine or strychnine is rendered cloudy by these re-agents. Other metatungstates may be easily prepared by double decomposition with the metatungstate of baryta. The author has obtained metatungstic ether by the action of iodide of ethyl on crystallised metatungstate of silver in a sealed tube.

The blue, crystallised oxide of tungsten may be prepared by calcining alkaline metatungstates.

II. ORGANIC CHEMISTRY.

New Hydroselenocyanides.—Kypke and Neger describe (*Annalen der Chem. und Pharm.*, bd. cxv., s. 207) two new compounds which they have obtained by passing slowly a current of chlorine through a sufficiently strong solution of pure selenocyanide of potassium. A red precipitate separates at first, which is soluble in an excess of the liquor, but, by degrees, the precipitate becomes permanent and the liquor acid. The precipitate is composed of minute red prisms. By continuing the action of the chlorine they turn yellow. The action is accompanied by the disengagement of an intense heat, and (more particularly during the latter change) chloride of cyanogen is evolved. The red and yellow bodies are not chlorinated compounds; for they are also produced by the action of nitric and hyponitric acids, and even the oxygen of the air on the selenocyanide of potassium. They are very unstable in moist air.

The red body is deliquescent; dissolves in water and hydrochloric acid, with the deposition of selenium. Sulphuric acid sets free hydrocyanic acid, and nitric acid gives rise to the formation of the yellow body. Caustic soda, ammonia, and cyanide of potassium dissolve it, and the solution gives the reactions of hydroselenocyanic acid. Ether and a diluted alcohol dissolve it, and the new body crystallises from these solutions in beautiful red prisms.

The yellow body is an oxidised product of the red. When dry it forms a yellow powder, which turns red or blackens in moist air. Water and hydrochloric acid separate selenium from it. Concentrated sulphuric and nitric acids dissolve it. Alcohol and ether dissolve it, leaving a red powder, which rapidly blackens, and the solutions deposit yellow crystals.

A concentrated etherial solution changes, when heated, into a red crystalline mass, identical with the red body above described, hydrocyanic acid being evolved. Heated to 130°, out of contact with the air, the yellow body fuses and evolves white vapours, which condense in white crystalline plates, soluble in water, alcohol, ether, potash, and nitric acid, and which are perhaps selenide of carbon.

The authors do not give the composition of the red and yellow bodies, but remark that they appear to correspond to the bodies called pseudo-sulphocyanogen and perhydrosulphocyanic acid.

MISCELLANEOUS.

A New Alloy of Copper.—A substance resembling gold, and known as "oreide of gold," is extensively manufactured in the United States under the licence of the French patentees, MM. Mourier and Vallant. It is composed of 100 parts (by weight) of pure copper, 17 of zinc, 6 of common magnesia, 3·60 sal ammoniac, 1·80 quick lime, and 9 of crude tartar. The copper is first melted in a crucible, then the magnesia added, then the sal ammoniac, lime, and tartar separately, and in powder. These are kept from contact with the air, and are well stirred for about twenty minutes, until they are incorporated together. The zinc is now added in strips, which are thrust below the scum formed on the top of the crucible. The mass is now stirred, the lid put on the crucible, and its contents kept fused for about twenty-five minutes; after which the crucible is opened, the slag skimmed carefully from the surface, when the molten alloy is poured out into ingot moulds if it is required to be rolled, or into iron moulds if designed for casting. When designed for works of art, however, it is best to cast it into ingot form first, then melt it in a furnace and cast it. This alloy is very ductile, and may be rolled into very thin leaf, but it is nearly as easily tarnished as common brass.

Chemical Society.—At the next meeting on May 2nd, the following papers will be read:—"On the Absorption of Poisons by Plants," by Dr. Daubeny. "On the Amount of Water displaced from the Hydrates of Potash, Soda, and Baryta, by Boracic and Silicic Acids," by Prof. Bloxam. "On the Graphite of Cast Iron," by Prof. Crace Calvert. "On the Action of Dibromide of Ethylene on Pyridine," by J. Davidson, Esq. "On Bromide of Carbon," by A. W. Lennox, Esq.

Improved Mode of Extracting Phosphorus from Bones.—*Le Génie Industriel* describes a process recently patented by M. Cari Mantrand, of Paris, for extracting phosphorus from bones more economically than by the processes heretofore employed. The calcined bones, reduced to a fine powder, are mingled with a sufficient quantity of pulverized charcoal to combine, as carbonic oxide, with all the oxygen of the phosphate. The mixture is placed in an earthenware cylinder varnished on the inside, filling the cylinder to three-fourths of its capacity. The cylinder is then heated red hot, and a current of hydrochloric acid gas is blown into it. The phosphate of lime is immediately decomposed, forming chloride of calcium and carbonic oxide, while the liberated phosphorus is evaporated and driven through a copper tube, which leads into a vessel of cold water, where the phosphorus is condensed. The chloride of calcium, disembarrassed of the charcoal, in contact with sulphuric acid, regenerates hydrochloric acid for a new operation. The labour of pulverizing the bones may be saved by digesting them with a solution of hydrochloric acid; using for this purpose the water of the condenser from the preceding operation.

The Cause of Fermentation.—In a recent communication to the Academy of Sciences, M. Pasteur, whose labours on fermentation are highly appreciated, gives an account of a curious microscopic insect, which is not merely produced during the process; but which is actually the cause of it. In lactic fermentation various substances are produced, viz., lactic acid, gum, mannite, butyric acid, alcohol, carbonic acid, and hydrogen. M. Pasteur having found that the vegetable ferment which transforms sugar into lactic acid, differs from those which produce gum, and which in their turn do not produce lactic acid, soon ascertained that none of those produced butyric acid. The existence of a special butyric ferment was therefore highly probable, and after many experiments during which M. Pasteur vainly endeavoured to get rid of the infusoria which infested his preparations, he was at length struck with this curious pertinacity, and at length came to the conclusion that these infusoria were themselves the ferment which he was in search of! This view was speedily confirmed by a great many experiments, which showed that the transformation of sugar, mannite, and lactic acid into butyric acid was exclusively due to these animalculæ. Their form is that of small cylindrical rods rounded off at their extremities, generally straight and connected together in chains of two, three, or four links each, sometimes more. Their length does not exceed the fifteen-thousandth part of a millimètre! These diminutive creatures advance with a creeping motion, their body remaining nearly rigid, or at best experiencing but slight undulations. They are often bent at one or both of their extremities, but this seldom happens in their youth. They are fissiparous, that is, they multiply by separation, one link after another dropping off from the parent animal, and becoming a parent of others itself. These vibrios may be regularly sown in any medium which affords them appropriate nourishment; but the most singular circumstance connected with them is, that they can live and multiply without the assistance of the smallest particle of air or free oxygen; nay, air and oxygen kill them, while a current of carbonic acid, a most deleterious gas, as we know, may be made to pass through the liquid in which they are, without affecting them in the slightest degree. This is the first instance on record of animal life being supported without free oxygen.

ANSWERS TO CORRESPONDENTS.

Numerous applications having been made for Monthly Parts of the Chemical News, we have determined upon issuing a Part with the Magazines each Month, strongly sewn in a wrapper. The price of each Part will be 1s. 5d., post free 1s. 7d.; or when consisting of five numbers, 1s. 9d. and 1s. 11d. Part III. of the present Volume is now ready.

*** In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

*** All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

Vol. II. of the Chemical News, containing a copious Index, is now ready, price 12s., by post, 12s. 8d., handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1s. 6d. Subscribers may have their copies bound for 2s. if sent to our Office, or, if accompanied by a cloth case, for 6d. A few copies of Vol. I. can stilll be had, price 10s. 6d., by post 11s. 2d. Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

E. R. S.—The numbers are now out of print, but will be shortly reprinted. Thanks for the report.

J. W. W.—*Silvering Glass.*—We have a communication for this correspondent: where shall it be sent?

R. C. C.—Apply to Messrs. Munn and Co., publishers of the *Scientific American*, New York.

Ignotus.—What you ask is impossible, only *small* pictures can be so treated, and then it takes a long time. The ordinary process is the best.

J. Goold.—1. The silver or acid evidently contained some impurity. 2. It is the old name for the salt. 3. The alkali metal forms with absolute alcohol what is called "sodium alcohol."

W. R. R.—Received with thanks: they shall be inserted. We have heard something of the case you mention, and should like to hear further particulars. Can you favour us with them, not necessarily for publication?

Parchment-paper is made by immersing unsized paper in sulphuric acid, made by diluting the strong acid with half its bulk of water and allowing it to cool. Keep the paper in for about five seconds, and then remove to a large vessel of water. Wash well till all the acid is removed, and dry. The paper will have to be rolled to remove the crumpled appearance.

W. R.—No good description has yet been published: one will shortly appear in our pages.

T. E. D.—Clean the plates by rubbing them with very fine emery, and when bright platinise by immersion in a dilute solution of bichloride of platinum, connecting them at the same time with a battery.

Silver Ores.—A correspondent writes as follows:—"The copper mine which presents itself to my notice for experimental purposes is one in which the ore is boiled in muriatic acid to extract the copper, which is then precipitated by the addition of iron. 1st, therefore, Do you suppose that in either of those stages, or from the muriate of iron residue, the silver could be extracted; and how? 2nd. Suppose the case of the waste ore—too weak in copper to be worth the above process,—how would you advise me to proceed in order to 'globulate' the silver from its oxide, in however minute quantity it may be present?"

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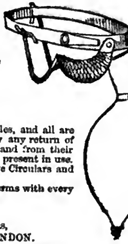

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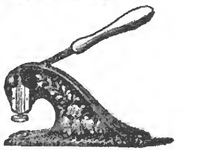

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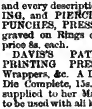

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AND
Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

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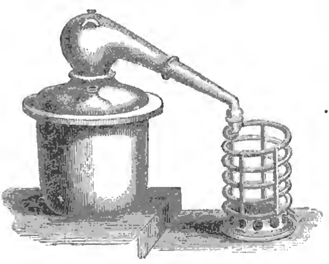

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THE CHEMICAL NEWS.

Vol. III. No. 75.—May 11, 1861.

WATER ANALYSIS.

STATEMENT OF THE RESULTS.

No branch of analytical chemistry is, perhaps, better defined or less liable to error than the methods usually employed to detect the presence, and determine the amounts respectively, of the several bases and acids likely to occur in the examination of a sample of spring water; but no sooner is an accurate estimate of the individual constituents arrived at, than difficulties are experienced in drawing up a statement of the results which shall correctly represent the mode of combination and arrangement of the several saline constituents held together in solution by the water. Much diversity of opinion appears yet to exist with regard to the system to be adopted in these cases; some Analysts following the recommendation of Fresenius, whilst others, working according to systems peculiarly their own, have arrived certainly at very anomalous conclusions in reference to the co-existence of salts in solution.

We remember to have heard, many years since, an anecdote bearing so directly on the subject in hand that we may be excused for mentioning it here. It is related of one of our early but distinguished Chemists, who, on reporting the analysis of an ordinary spring water sent to him for examination, had combined the sulphuric acid with the magnesia, sodium with chlorine, lime with carbonic acid, &c., and thus represented the water to contain not only common salt and chalk, but also a large proportion of Epsom salt; whereupon his client became alarmed at the highly medicinal character of the spring, which, in his neighbourhood, had acquired some celebrity as an agreeable drinking water. This led to a re-consideration of the analytical results, and it was found that by substituting *chloride* of magnesium for the objectionable sulphate, and gypsum for chalk, that the amended report became then completely satisfactory to all parties.

A recent example of discrepancy in stating the results of the chemical examination of samples of water from the same locality is presented in the April issue of the *Quarterly Journal of the Chemical Society*. The saline water of Purton, near Swindon, has been analysed independently by Dr. Noad and by Dr. Voelcker, of Cirencester. Their results were communicated to the Chemical Society on the same evening, and the tabulated statements are brought together on contiguous pages of the Society's *Journal* and in the columns of the Chemical News.[1] It will be seen that there are many points of general agreement in these analyses, but there is also a notable discrepancy as regards the state of combination of the lime. Dr. Noad advances experiments to prove that nearly half the amount of lime is present in the water as carbonate, since this proportion is precipitated on boiling. Dr. Voelcker maintains, on the contrary, that *potash* is present as carbonate, and the lime *all* as sulphate. This latter supposition may not, perhaps, be directly contradicted by the known reaction of carbonate of potash upon sulphate of lime, but involves at least the necessity of assuming this to be the more probable state of combination. In regard also to that frequent constituent of saline waters, chloride of magnesium, Dr. Noad's analysis exhibits a fair proportion, but in Dr. Voelcker's Table this item is altogether unrepresented.

What has been stated above in reference to the satisfactory nature of the tests for inorganic constituents in water does not, unfortunately, equally apply to the determination of organic matter. It were much to be desired that better methods could be found for determining with greater accuracy the amount of dissolved organic impurities contained in samples of water. The process usually followed is difficult of execution, and takes no account of the particular *kind* of organic matter which may be present. Whether peaty or nitrogenous in its nature, harmless or injurious, all come under the general category "organic matter." Neither can it be said that the division into *crenic* and *apocrenic* acids has, since the first introduction of these names, served to convey any clearer idea of the nature of these organic bodies.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Chemical Researches on Combustible Minerals, by M. E. Fremy.

My long-pursued studies on vegetable tissues, of which the Academy already knows the principal results, have naturally led me to wish to determine the chemical characteristics of the combustible minerals, and to try to discover whether their constituent materials present any analogy with those forming unaltered vegetable tissues.

Admitting, as do all geologists, that turf, lignite, coal, and anthracite, are formed under different circumstances, and that they appertain to the strata of different epochs, my desire is to discover in these combustibles the degree of alteration of the organic tissue.

In studying turf, I have discovered no really new fact. By the side of the unaltered elementary organs, found in such large quantities in fibrous turf, I have found, according to the degree of alteration the combustible has undergone, various proportions of those brown compounds, neutral or acid, nitrogenised or not, which, in our ignorance, we designate by the general term of ulmic compounds. The presence of these bodies, which M. Payen has already investigated, establishes a very clear distinction between turfs and unaltered organic tissues.

The chemical examination of lignites was likely to prove more interesting. In this examination I have taken care to distinguish those specimens still presenting a ligneous organisation from those which have the appearance and compactness of coal. The former constitute the xyloid lignite, or fossil-wood; the latter, compact and perfect lignite. Regarding their chemical characteristics, all the varieties of lignite I have examined are included in the two preceding species.

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[1] See Chemical News, Vol. iii. p. 77.

Although the fossil-wood lignite has often the tenacity and appearance of ordinary wood, I have come to the conclusion that in this combustible the ligneous tissue undergoes a great change; trituration reduces it to fine powder; submitted to the action of a weak solution of potash, it yields to the alkali a considerable quantity of ulmic acid.

The following two reactions establish a very marked difference between ordinary wood and fossil-wood lignite:—

When nitric acid, by the aid of heat, reacts on wood, it dissolves a portion only of its fibres and medullar rays, and leaves untouched the cellulose matter, which dissolves without colouration in concentrated sulphuric acid, and possesses all the properties ascribed to it by M. Payen.

Under the same circumstances, fossil-wood lignite is attacked with great energy and completely transformed into a yellow resin, soluble in alkalies and in excess of nitric acid.

By submitting wood and fossil-wood lignite comparatively to the action of hypochlorites there is a marked difference in their effects on the two substances.

Hypochlorites re-act on wood in a manner which may be compared to that of nitric acid. They dissolve rapidly a portion of the fibres and medullary rays, leaving the cellulose untouched.

Alkaline hyhochlorites attack fossil-wood lignite, almost entirely dissolving it, leaving only imponderable colourless traces of the fibres and medullary rays.

It results, then, from the preceding facts, that when ligneous tissues arrive at that state of modification constituting fossil-wood lignite, and while preserving the appearance of wood, their substance has undergone a very material modification, and then contains new proximate principles, characterised by their perfect solubility in nitric acid and in hypochlorites.

After determining the chemical characteristics of fossil-wood lignite, it becomes interesting to ascertain whether the compact lignite, in which the texture of the ligneous tissues is no longer observable, and which is black and brilliant like coal, to which it bears so much analogy as to mystify the most experienced, preserves the chemical characteristics of fossil-wood lignite or whether it resembles coal.

In a geological point of view, the comparative study of fossil-wood lignite, of compact lignite, and of coal appears to me to be also of great importance. If, in fact, there is a positive connection between the state of alteration of combustible minerals and the age of the strata containing them, it becomes apparent how much geology would benefit by possessing a chemical character, by means of which it would be possible to determine accurately the extent of the modification of an organic body, and of determining the age of the stratum by the state of alteration of the combustible mineral found therein. Therefore, I have applied myself to find a series of chemical re-agents acting differently on combustibles, minerals, and to arrange their varieties conformably to the degree of their modifying action and the chemical characters they would thus present. The re-agents I have employed are potash, hypochlorites, sulphuric and nitric acids. I also take into account the excellent characteristic given by M. Cordier.

I have previously shown that it is impossible to confound ligneous tissue with fossil-wood lignite, the latter being soluble in hypochlorites and nitric acid. Compact lignite presenting no longer the appearance of organisation could be confounded only with certain varieties of coal. The mode of combustion, the reaction of the volatile products on litmus, and the colour of the dust, constitute already distinctive and very important characteristics. To this subject chemical re-agents will give a yet greater degree of certainty. When compact lignite is submitted to the action of concentrated potash, the liquid sometimes becomes coloured brown, dissolving a small quantity of ulmic acid; but alkaline liquid does not generally re-act on the combustible. Thus a distinction is immediately established between fossil-wood and compact lignite.

I have always observed that the lignites which resist the action of potash are those which, by their bearing, approach nearest to the coal series.

Compact lignites, black and brilliant like coal, dissolve entirely in alkaline hypochlorites, are rapidly attacked by nitric acid, and produce the yellow resin which I have already mentioned while treating of fossil-wood lignite.

The two characters just indicated prevent, then, the confounding lignites with coals. Coal, in fact, is not dissolved by hypochlorites, and is attacked but feebly by nitric acid. I have submitted to the test of the hypochlorites almost all the important coals belonging to different strata, and have found that these combustibles always resist the action of these chemical re-agents. To me this characteristic appears so valuable that I think were a sample of coal met with slightly attackable by hypochlorites, it would be advisable to examine whether the combustible possessing this exceptional property were really coal; for it may well be imagined that in coal-fields there may exist vegetable matters unequally decomposed.

Coal and anthracite which resist the action of alkaline and hypochlorite solutions dissolve completely in a mixture of monohydrated sulphuric acid and nitric acid; the liquid takes a very deep brown colour, and holds in solution an ulmic compound, which water completely precipitates.

It is not my intention in this paper to consider the influences by which organic tissues are transformed into combustible minerals. I ought, however, here to put on record what seems to me an interesting observation. I have ascertained that ligneous tissue, exposed for several days to a temperature of 200°, undergoes successive modifications, and yields bodies very similar to those found in lignites. The first are soluble in alkalies, and correspond to fossil-wood lignite; the second are insoluble in alkalies, but dissolve entirely in hypochlorites, like compact lignite. These are the new facts which it is my wish to submit to the Academy. Their evident object is to introduce chemical characteristics into the study of combustible minerals, and they appear to me to lead to the following deductions:—

1. By treating combustible minerals by the above-mentioned re-agents, it is ascertained that in proportion to the age of the lignite so the chemical characteristics of the tissues gradually disappear, and the organic matter more resembles graphite the older the stratum whence it is derived. One exception I make in respect of the strata which have been modified under the influence of metamorphism. My researches accord completely with

those of M. Regnault, who has already arrived at the same conclusions as myself in his important researches on combustible minerals.

2. The first degree of modification of ligneous tissues represented by turf is characterised by the presence of ulmic acid, and also by the ligneous tissues or cellules of the medullary rays, which can be extracted and purified in considerable quantities by means of nitric acid or of hypochlorites.

2. The second degree of modification corresponds to fossil-wood, or xyloid lignite. It is partly soluble in alkali, like the preceding body; but it undergoes a greater alteration, for it dissolves almost entirely in nitric acid and hypochlorites.

4. The third state of modification is represented by compact or perfect lignite. Re-agents indicate in this substance a transition from organic matter to coal: thus, alkaline solutions have generally no effect on perfect lignite; this combustible is characterised by its complete solubility in hypochlorites and nitric acid.

5. The fourth degree of modification corresponds to coal, which is insoluble in alkaline solutions and in hypochlorites.

6. The fifth degree of alteration is anthracite, which is obviously allied to graphite, which resists the re-agents capable of modifying the preceding combustibles, and which is but very feebly attacked by nitric acid.

It is, then, evident that chemical reactions confirm the classification of the combustible minerals admitted by geologists. I am, however, far from thinking that lignite, coal, and anthracite, which are now characterised by their elementary composition and chemical reactions, constitute the only modifications which organic matters undergo while changing into combustible minerals. There doubtless are intermediate transformations of the organic tissues corresponding to the differences established by commercial practice between the different species of coal and lignites. But are the re-agents sufficiently sensitive to be capable of characterising these different varieties of the same combustible mineral in glance-coal or smith-coal, or even in the different layers of the same coal-bed? I shall investigate this question in another communication.—*Comptes-Rendus.*

On Some Applications of the Ferrocyanide of Potassium in Chemical Analysis, by EDMUND W. DAVY, *A.B., M.B., M.R.I.A., Professor of Agriculture and Agricultural Chemistry to the Royal Dublin Society.*[1]

(Continued from page 276.)

Having made two standard solutions, the first containing 21·122 grammes of ferrocyanide of potassium in a litre of the solution, and the second 14·759 grammes of bichromate of potash in the same quantity of solution (weights which are to each other as their atomic equivalents), I made several estimations of chloride of lime with them, adopting the method I have just described, and found that it gave the most consistent results, and which agreed very closely with those obtained by Gay-Lussac's and Otto's methods when the latter were performed with the greatest care, the only difference being that the results obtained by my method indicated a few hundredths of a part more of chlorine than either of those methods did, which may be accounted for by the unavoidable loss of a minute quantity of chlorine which takes place in those processes.

In order to simplify the process and render the calculation as short as possible, I would recommend for commercial valuations the following way of carrying out this principle:—Having obtained a flat-bottomed flask or bottle, which will contain 10,000 grains of distilled water when filled up to a certain mark in the neck, make two standard solutions, the first by placing in the flask or bottle 1190 (or exactly 1189·97[2]) grains of the purest crystallised ferrocyanide of potassium (yellow prussiate of potash), reduced to powder, adding distilled water to dissolve the salt, and when this is effected, filling up with water to the mark; and having mixed the solution thoroughly, place it in a well-stoppered bottle. The second standard solution is made in the same manner, substituting for the ferrocyanide 138·6 (or exactly 138·58) grains of bichromate of potash which has been purified by re-crystallisation and fused in a crucible at as low a heat as possible. Both these solutions will keep unchanged, and will answer for a number of determinations if they are preserved in well-stoppered bottles, and the ferrocyanide solution be kept, when not in use, excluded from the light. Get a burette or alkalimeter capable of holding or delivering 1000 grains of distilled water, and divided into 100 equal divisions; also two small bottles, one capable of delivering 1000 grains, and the other 500 grains of distilled water when filled up to a certain mark on the neck of each,[3] which may both be readily made by filling them with water, emptying them, and, after they have drained for a minute or two, weighing into each the above weights of distilled water; or, what will be sufficiently accurate for most purposes, pour from the burette into one 100 divisions of distilled water, and into the other 50, and mark with a file where the fluid stands in the neck of each bottle. Having these all ready, take an average specimen of chloride of lime, and weigh out 100 grains of it, and make in the usual way a solution of it by trituration in a mortar with some water; pour it into the flask which was used in preparing the two standard solutions, and, having filled up with water to the mark in the neck, mix the solution thoroughly; and before each time that any of the chloride of lime is taken out, shake well the contents of the flask.

Measure out into a beaker-glass, by means of the two little bottles, 100 divisions of the chloride of lime solution, and 50 of the standard solution of ferrocyanide; and having mixed them well together, add some hydrochloric acid to dissolve the precipitate formed and acidify the mixture strongly; and having mixed the whole well, pour from the burette slowly the standard solution of bichromate, stirring well all the while, till a drop taken from the mixture and brought in contact with a drop of a very weak solution of perchloride of iron produces a yellowish-brown colour, as already noticed. Then read off the number of divisions of the standard solution of bichromate which was necessary to produce this effect, and this being deducted from 50, gives the per-centage by weight of chlorine.

For the standard solution of ferrocyanide having been made so that the 10,000-grain measures should be equivalent to 100 grains of chlorine, and as every division of the burette equals 10 grains, each of these divisions of the ferrocyanide solution converted into ferridcyanide will indicate 0·1 grain of chlorine. Again, the 100 divisions of the solution of chloride of lime represent 10 grains of that substance, and we want to know how many divisions of the ferrocyanide solution its chlorine has converted into ferridcyanide. This is readily ascertained by the bichromate solution, which has been so graduated that each division represents a division of the ferrocyanide solution. So that to determine the percentage of chlorine, we have only to deduct, as before stated, the number of divisions of the bichromate solution employed from the 50 of the ferrocyanide solution, and the difference gives us the per-centage of chlorine by weight in the sample; thus, in four experiments 50 divisions of the ferrocyanide solution, mixed with 100 divisions of the solution of chloride of lime, required 18·5 divisions of the bichromate solution to convert the whole of the ferrocyanide employed into ferridcyanide; this number, taken from 50, leaves 31·5 divisions of ferrocyanide, which were converted into ferridcyanide by the chlorine of the chloride of lime; and as each division represents 0·1 grain of chlorine, 31·5 will be equivalent to 3·15 grains of chlorine, which is the amount contained in 10 grains of the sample; consequently, 100 grains will contain 31·5 grains of chlorine, which is the same amount as is obtained by simply deducting the number of divisions of bichromate solution employed from 50 of ferrocyanide used in the estimation.

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[1] From the *Philosophical Magazine*, being part of a paper read before the Royal Dublin Society, December 17, 1860.

[2] The above numbers are obtained as follows:—35·5 parts of chlorine are capable, as before stated, of converting 422·44 parts of the crystallised ferrocyanide of potassium into ferridcyanide; therefore, 100 parts of the former will convert 1189·97 parts of the latter into the same compound. Again, as before observed, 1267·32 parts of the crystallised ferrocyanide require 147·59 parts of the bichromate of potash to convert them into the ferridcyanide; 1189·97 parts, therefore, will take 138·58 parts of that salt to produce the same effect.

[3] Two small pipettes capable of delivering the above quantities would be found still more convenient.

Though this process appears a long one, from the details which are necessary to explain its principle, yet in practice it is very expeditious, and requires only a very few minutes for its performance, and is much quicker than either Gay-Lussac's or Otto's method.

Though I have as yet chiefly confined my attention to the use of the ferrocyanide of potassium in the estimation of chlorine in bleaching powder, I have no doubt that it may be advantageously employed in many other useful determinations by carrying out the principles already explained: thus, for example, it may be used as a means of determining the amount of bichromate of potash present in a sample of that salt, or the quantity of chromic acid that exists under different circumstances. Again, the same salt may be used in different determinations where a certain amount of chlorine is liberated, which represents a proportional quantity of some other substance: thus, for example, in the estimation of manganese ores for commercial purposes, where they are heated with hydrochloric acid, the quantity of chlorine disengaged will indicate a certain amount of peroxide of manganese in the ore, on the presence of which its commercial value almost entirely depends; and the chlorine evolved may be estimated by absorbing the gas in a dilute solution of caustic potash, and then determining the amount of chlorine in it by precisely the same process as that I have recommended in the valuation of chloride of lime. To test the accuracy of this method, I heated in a small flask a given quantity of pure bichromate of potash with an excess of strong hydrochloric acid, and collected the evolved chlorine by means of a dilute solution of caustic potash, employing the bulbed retort and curved dropping-tube, as recommended by Bunsen in the "Analysis of the Chromates" (see the last edition of Fresenius's "Quantitative Analysis," page 234), and ascertained afterwards, by the use of the ferrocyanide of potassium, the amount of chlorine evolved, which corresponded almost exactly with the calculated amount of that substance which should have been obtained by the action of the quantity of bichromate used on the hydrochloric acid. Again, a standard solution of ferrocyanide of potassium may be used, as E. de Haen has shown, to determine the strength of the permanganate of potash in the analysis of the ferrocyanide and ferridcyanide of potassium, as an acidified solution of the ferrocyanide, as before stated, rapidly decolorises a solution of permanganate of potash, whereas the ferridcyanide has no action on that salt; and this reaction might be taken advantage of, in the valuation of chloride of lime, to determine the excess of ferrocyanide used in my process, but from my experiments I found that more precise and accurate results were obtained by the use of the bichromate of potash.

The reaction of the bichromate of potash on the ferrocyanide might be employed in the valuation of the ferrocyanide of potassium and other ferrocyanides, having previously, in the case of those which were insoluble, converted them into the ferrocyanide of potassium by boiling them with caustic potash, and separating the insoluble oxides by filtration.

It might also be employed for the valuation of the commercial red prussiate of potash, which is now to some extent employed as a bleaching agent in calico printing, and which consists of varying quantities of ferro- and ferridcyanide of potassium together with chloride of potassium. By ascertaining first how much a given quantity of the sample requires of a standard solution of bichromate of potash to convert the ferrocyanide present into ferridcyanide, the per-centage of that substance would be known; and then by taking another portion of the sample and converting the ferridcyanide it contained, by reducing agents, such as the sulphites of soda and potash, &c., into the ferrocyanide, and finally determining the amount of bichromate necessary to bring the whole of the ferrocyanide then present into the state of ferridcyanide, the difference in the two results would indicate the proportion of ferridcyanide originally present in the sample.

The last application of ferrocyanide of potassium which I shall notice in the present communication is its employment as a reducing agent. It has long been known that the cyanide of potassium possesses most powerful reducing properties, and has been very usefully employed for that purpose in the reduction of different metallic salts under various circumstances; but I am not aware that the ferrocyanide of potassium has been proposed or used for similar purposes; at least, I have referred to a great number of analytical and general chemical works, and in none of them is this salt recommended as a reducing agent, though the cyanide is so much extolled for that purpose. According to my experiments, the ferrocyanide is a far more convenient reducing agent than the cyanide, and may be substituted for it in many cases of reduction with the best results, as it possesses many unquestionable advantages over that salt for this purpose. Thus, the ferrocyanide does not deliquesce and decompose when exposed to the air, whereas the cyanide rapidly absorbs moisture, and, unless kept in very well-stoppered bottles, becomes quite wet, and in this state quickly decomposes; and this deliquescence on the part of the cyanide is often a source of much inconvenience in its use as a reducing agent, owing to the almost unavoidable absorption of more or less moisture which takes place in mixing it with the substance to be reduced, and during the introduction of the mixture into the reducing-tube. The ferrocyanide, on the other hand, in a thoroughly dried and finely-powdered state, can be intimately mixed with the substance without any appreciable absorption of moisture.

I made the following comparative experiment to ascertain the relative absorptive properties for moisture of the two salts under the same circumstances:—Having thoroughly dried in a water-oven, till it ceased to vary in weight, some finely-powdered ferrocyanide, I placed 50 grains of it in a counterpoised watch-glass, and powdering in a warm mortar some fresh cyanide of potassium, I placed the same quantity of it in a similar counterpoised watch-glass, and left them both exposed to the air. On examining them after four hours' exposure, I found that the former had only gained $\frac{6}{100}$th parts of a grain of moisture, whereas the latter had taken up 3·6 grains, or sixty times as much moisture under the same circumstances. After two days' exposure I found that nearly all the cyanide had passed into the liquid condition, having taken up 46 grains of water; whereas the ferrocyanide appeared perfectly dry, and had only absorbed 1·4 grain.

The great fusibility of the cyanide is sometimes rather a disadvantage, which has to be lessened by mixing it with a certain proportion of dried carbonate of soda; but the ferrocyanide not fusing at so low a temperature, does not require in most cases this admixture to lessen its fusibility. Again, the ferrocyanide is not a poisonous salt, whereas the cyanide is highly so, and must be used with great caution; and, lastly, the former salt is little more than half the price of the latter. Combined with the above advantages, I find that the ferrocyanide is equally effective in reducing metallic oxides and sulphurets, and is especially convenient for the reduction of different combinations of arsenic and mercury, which are reduced by it with the greatest ease.

I made several comparative experiments with the dried ferrocyanide and with the cyanide as reducing agents for the sulphuret of arsenic and arsenious acid, employing the same quantity of arsenical compound with each salt, under similar circumstances; and in almost every case, particularly where the quantities operated on were minute, I obtained more satisfactory results with the dried ferrocyanide than with the cyanide.

The following were amongst my experiments:—I mixed the $\frac{1}{10}$th of a grain of sulphuret of arsenic with 3 grains of the dried ferrocyanide, and made a similar experiment, substituting the same quantity of cyanide; and, on heating the mixtures in similar glass tubes, obtained almost identically fine and characteristic rings of metallic arsenic.

I then intimately mixed the same quantity of sulphuret of arsenic with 49·9 grains of very finely-powdered glass, and taking 5 grains of this mixture, containing the $\frac{1}{100}$th part ot a grain of the sulphuret, mixed it with 5 grains of the dried ferrocyanide, and made a comparative experiment with another 5 grains of the mixture, substituting the same quantity of cyanide: on heating both these mixtures in small reduction-tubes, I got the characteristic metallic rings in both, but better defined in the case of the ferrocyanide.

I finally took 2·5 grains of the mixture of sulphuret and glass, containing about $\frac{5}{1000}$th parts of a grain of sulphuret of arsenic, and treated them in the same manner, using in one case 2·5 grains of ferrocyanide, and in the other 2·5 grains of cyanide, and obtained in each case a minute metallic ring, which, however, was much more distinct and satisfactory where the ferrocyanide had been used as the reducing agent.

The same comparative experiments were made with arsenious acid, when results similar to those in the case of the sulphuret of arsenic were obtained.

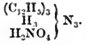
The ferrocyanide, therefore, is a most delicate reducing agent in the case of arsenical compounds, and where very minute quantities have to be detected, appears from my experiments to give more satisfactory results than the cyanide.

Whether the addition of dried carbonate of soda would improve the ferrocyanide for some cases of reduction, I am not at present able to say; but in one experiment which I made with the sulphuret of arsenic, I obtained as good results, using the ferrocyanide alone, as where it was mixed previously with its own weight of dried carbonate of soda. In many cases the ferrocyanide may be used as a reducing agent in a state of powder without separating its water of crystallisation; but, in most cases, it will be rendered a far better reducing agent by being previously dried at 212° in a water-bath or oven; and in this dried condition it may be kept for any length of time in a good-stoppered or well-corked bottle.

Though as yet my experiments have been chiefly confined to the reduction of different compounds of arsenic and mercury, I entertain no doubt that the ferrocyanide of potassium will be found an equally effective reducing agent in the case of the combinations of other metals, and that it may with great advantage be substituted for the cyanide of potassium in many cases where the latter salt is used as a reducing agent.

On Aniline Red, by M. E. Kopp.

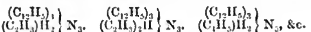
I FIND that aniline red, produced by the action of nitric acid or of nitrates of an easily-reducible base, is a triamine, representing mononitro-trianiline,—



$$\left.\begin{matrix}(C_{12}H_5)_3\\ H_3\\ H_2NO_4\end{matrix}\right\} N_3.$$

The existence of binitro-trianiline seems to me almost certain, as, by treating aniline with a mixture of sulphuric and nitric acid, we obtain a yellowish-brown body, possessing in the highest degree the properties of a nitro compound; but I have not yet succeeded in purifying it sufficiently to make the analysis coincide exactly with the formula.

In trianilines a portion of the hydrogen may be substituted by methyl, ethyl, and amyl, along with nitrous gas. These are very probably the compounds:—



$$\left.\begin{matrix}(C_{12}H_5)_3\\ (C_2H_3)H_2\\ (NO_4)H_2\end{matrix}\right\} N_3. \quad \left.\begin{matrix}(C_{12}H_5)_3\\ (C_2H_3)_2H\\ (NO_4)H_2\end{matrix}\right\} N_3. \quad \left.\begin{matrix}(C_{12}H_5)_3\\ (C_4H_5)H_2\\ (NO_4)H_2\end{matrix}\right\} N_3, \text{\&c.}$$

I specified in my paper these substances only as hypothesis, but they really exist. I have already obtained several of them, and it is very remarkable that the red tint disappears, becoming from violet more and more blue, according as the hydrogen is replaced by the hydrocarbons.

The latter are not yet sufficiently pure for analysis, though very beautiful as shades of colour. They are generally almost insoluble in water, but easily soluble in alcohol or acetic acid. When the trianiline, instead of being nitrated, is chlorated, bromated, or iodised, the compound is equally a red matter, but presenting slight differences of tint.

I venture to hope that my work will serve to throw some light on the mode of formation, the composition, and the constitution of the colouring matters, so remarkable and so important, which are derived from aniline.—*Comptes-Rendus.*

On the Properties of Liquid Carbonic Acid, by George Gore, *Esq.*

In this communication the author shows how a small quantity of liquid carbonic acid may be readily and safely prepared in glass tubes closed by stoppers of gutta percha, and be brought in a pure state into contact with any solid substance upon which it may be desired to ascertain its chemical or solvent action, or be submitted to the action of electricity by means of wires introduced through the stoppers.

By immersing about fifty substances in the liquid acid for various periods of time, he has found that it is comparatively a chemically inert substance, and not deoxidised by any ordinary deoxidising agent except the alkali metals. Its solvent power is extremely limited. It dissolves camphor freely, iodine sparingly, and a few other bodies in small quantities. It does not dissolve oxygen salts, and it does not redden solid extract of litmus. It penetrates gutta percha, dissolves out the dark brown colouring matter, and leaves the gutta percha undissolved, and much more white. It also acts in a singular and somewhat similar manner upon india rubber. The india rubber while in the liquid acid exhibits no change, but immediately on being taken out it swells to at least six or eight times its original dimensions, and then slowly contracts to its original volume, evidently from expansion and liberation of absorbed carbonic acid, and it is found to be perfectly white throughout its substance. These effects upon gutta percha and india rubber may prove useful for practical purposes.

The liquid acid is a strong insulator of electricity; sparks (from a Ruhmkorff's coil) which would pass readily through $\frac{8}{32}$nds of an inch of cold air, would with difficulty pass through about $\frac{1}{10}$th of an inch of the liquid acid.

In its general properties it is somewhat analogous to bisulphide of carbon, but it possesses much less solvent power over fatty substances.—*Proceedings of the Royal Society.*

TECHNICAL CHEMISTRY.

Cyanogen Steel.

It is curious to observe how frequently several inventors are engaged at the same time upon similar discoveries, although each of them may be pursuing his labours altogether independently of the others, and although the discovery may be unquestionably new. At the time when the pneumatic process of refining iron was creating so much attention, it will be remembered that both Mr. Martien and Mr. Bessemer had lodged provisional specifications within three months of each other, consequently the provisional protection accorded to each was running simultaneously, and before the public or any other person but themselves had had an opportunity of ascertaining the details of the respective inventions. About the same time the presence of nitrogen in steel was positively declared by Mr. Christopher Binks, and his project for employing cyanogen compounds in the manufacture of that metal was brought prominently forward. The question, however, of improving the mode of manufacturing steel had at that period been discussed to repletion, and Mr. Binks' assertion was regarded with suspicion, or, at least, as the project of an enthusiastic patent seller. After long remaining in abeyance (the fact of the discovery being only kept from sinking into oblivion by the occasional lodging of a specification, in which the use of substances capable of yielding cyanogen was incidentally mentioned), the subject has been revived, and is now restored to more than its pristine vigour, by the publication of M. Fremy's communication to the French Academy of Sciences; and it seems probable that cyanogen compounds will ere long be acknowledged to be as necessary to the manufacture of steel as carbon has hitherto been.

In Mr. Fleury's process of manufacturing steel with electricity cyanogen is regarded as a necessary constituent of steel; and several other processes, which have in practice proved to give excellent results, may owe their success to the unintentional introduction of cyanogen compounds. Amongst the specifications of patents issued during the past week, is one relating to an invention by Messrs. Duhesme, De Ruolz, and De Fontenay, for improvements in the manufacture of cast-steel, and in the re-manufacture of old steel, in which the use of cyanogen is again the principal ingredient employed. In making cast-steel according to this invention, certain mixtures of wrought-iron, cast-iron, oxide of iron, old steel, and ferrocyanide of potassium, or ferricyanide of potassium, or some of them are to be employed. The ferrocyanide of potassium is commonly known as yellow prussiate of potash, while the ferricyanide of potassium is commonly known as red prussiate of potash. The corresponding sodium salts may be substituted for the potassium salts. Were the sodium salts used, the process would be almost identical with that of Mr. Charles Sanderson, of Sheffield, also made public several years since. The papers read before the Society of Arts by Mr. Binks, advocating cyanogen, and by Mr. Sanderson, for employing substances capable of yielding cyanogen, for improving the quality of iron and the allied metals, having been presented within a short time of each other. But whether the salts of sodium or those of potassium be used, it is evident that Messrs. Duhesme, De Ruolz, and De Fontenay have been long anticipated, the cyanogen being incontestably the ingredient upon which they rely for success. In the manufacture of cast-steel, they continue, one mixture employed consists of—Wrought-iron, 700 parts by weight; white cast-iron, 200 parts; oxide of iron, 100 parts; red prussiate of potash, 8 parts. A second mixture consists of—Old steel (old files, tools, or springs), 500 parts by weight; wrought-iron, 500 parts; red prussiate of potash, 14 parts. A third mixture consists of—Old steel, 350 parts by weight; wrought-iron, 350 parts; white cast-iron, 200 parts; oxide of iron, 100 parts; red prussiate of potash, 7 parts. A fourth mixture consists of—Wrought-iron, 1000 parts by weight; oxide of iron, 75 parts; red prussiate of potash, 28 parts. From the small proportion of cyanogen added in the several instances, the astonishment that its existence did not long previously attract attention will at once disappear, the quantity of ferricyanide being only such as would cause a very small proportion of cyanogen to remain in the iron at the completion of the process.

If the ferrocyanide be employed instead of the ferricyanide of potash in the mixtures described, double the quantity indicated for the red prussiate of potash is used; but the latter is preferred as giving a more decided reaction, and also on account of its containing no water of crystallisation. Refined iron, or fine metal, may be advantageously substituted for the cast-iron in the first and third mixtures, in which case the quantity of fine metal employed must be double that indicated for the cast iron, without altering the proportions of red prussiate of potash. The cast-iron employed should be white, and of at least two qualities, one of which should contain manganese. It is to be first broken up into small fragments. The wrought-iron should be in the smallest possible fragments, and it should be avoided to use granular with fibrous iron. Granular iron of a good quality is preferred. The old steel should be of a good quality, and introduced into the mixture in small pieces. For the re-manufacture of old steel one mixture employed consists of old steel, such as old files, tools, or springs, 1000 parts by weight; red prussiate of potash, 10 parts. Another mixture consists of old steel, 1000 parts by weight; yellow prussiate of potash, 20 parts. In carrying out the manufacture of cast-steel, as also the re-manufacture of old steel, according to this invention, the above mixtures have to be melted down in crucibles, the ingredients having been previously mixed as completely as possible. The old steel, as also the wrought and cast-iron, should first be broken into small pieces, and the prussiate of potash should be well distributed throughout the mass. The operations take place in a common air furnace, for which a very pure coke is employed, mixed with a tenth part of wood charcoal. The crucibles must be kept hermetically closed until complete fusion of the metals ensue, and the fire must be managed with the usual precautions. The same description of crucible is employed as is generally used for the smelting of steel. Those of plumbago are preferred; but, if these are used, it is well to increase slightly the proportion of prussiate of potash after the crucibles have been employed for two successive meltings.—*Mining Journal.*

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Ten Lectures on Inorganic Chemistry, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

Lecture VII.

Nitrogen — Preparation and Properties — Compounds of Nitrogen with Oxygen and with Hydrogen.—Carbon—its Properties in the Form of Diamond, Graphite, and Charcoal.

We have to-day to consider an element which, in many respects, presents striking contrasts to the two elements we have hitherto studied. This element is nitrogen, a body which is rather distinguished for the sluggishness of its affinities than for the facility with which it unites with other bodies. On this account, nitrogen exists in nature chiefly in the free or elementary condition. It is contained in this state in the atmosphere, and constitutes about four-fifths of the total volume of atmospheric air, and more than three-fourths of the weight of the atmosphere. It exists, however, also in combination, although in comparison with the quantity contained in the atmosphere this amount in combination should be regarded almost as a vanishing quantity. It exists, however, in the tissues of plants and animals, and is in fact essential to the existence of animal and vegetable life. It is found also in the remains of plant life of former ages; consequently it is a constituent of our coal formations, and exists there to the extent of from one to one and a-half, and even, in some cases, up to two per cent. It is also a constituent of nearly all those highly poisonous vegetable substances, such as strychnine, brucine, narcotine, and bodies of that kind. It is also an essential element in hydrocyanic acid, that very poisonous prussic acid, as it is sometimes termed, with the properties of which we are all acquainted. There is scarcely any vegetable poisonous substance which does not contain nitrogen. That singular vegetable extract with which the Indians poison their arrows, is, so far as we know at the present time, entirely free from nitrogen. That and the poison of *cocculus Indicus* are nearly the sole exceptions to this general rule that these poisonous vegetable substances invariably contain nitrogen.

We can prepare this element either by abstracting it from the atmosphere, or, in other words, by removing the element with which it is associated in the air, namely, oxygen; or we can extract it from some of its compounds. The first is the most simple mode of proceeding. As nitrogen is remarkable for its want of affinity, and as oxygen is distinguished for its powerful affinities, there are many means by which we can withdraw the oxygen from the nitrogen in the atmosphere, thus leaving the nitrogen in a free state. A common candle will do this, to a certain extent, by burning it in a closed vessel filled with atmospheric air. Under such circumstances, after a certain time, the candle goes out. Now, it goes out when it no longer finds a sufficient quantity of oxygen to continue its combustion; but, unfortunately, a candle will not continue to burn until all the oxygen has been removed. Besides this, it also produces, at first, carbonic acid, and when the candle is about to be extinguished for want of more oxygen it produces carbonic oxide. From the carbonic oxide it is very difficult afterwards to free the nitrogen; and consequently this process of burning a candle in the air would not answer our purpose for procuring free nitrogen. Phosphorus, however, is a substance which is more combustible than a candle is, and which does not generate, on its combination with oxygen, any volatile matter—any gaseous substance—which we should have difficulty in subsequently removing from the nitrogen. Further, the phosphorus is capable of continuing to burn until every trace of oxygen has been removed from the air, and phosphorus, consequently, is well adapted for the preparation of nitrogen, leaving it in a state of comparative purity.

Here is the apparatus by which we effect this. We place a little piece of phosphorus on a small capsule floating on water, and then ignite it by means of a heated wire, and invert over it a bell-jar filled with air. At first we have an expansion of the air contained in the jar, owing to the heat generated by the combustion of the phosphorus, but this expansion will soon cease, and give place to a contraction as the combustion goes on; and we shall notice, I think, when the phosphorus has become extinguished that the gas remaining in the receiver will be considerably less than the bulk of air which the vessel contained at first. Those of you who can see over the edge of this vessel will perceive that even at the present moment the water is gradually rising in the interior. This rising of the water will increase as the temperature of the enclosed nitrogen diminishes. You see the phosphorus burns more and more dimly, and it will finally be extinguished. At the moment of its extinction, the last particle of oxygen will have been removed from the atmospheric air. Now, as we shall subsequently find, atmospheric air consists of particles of oxygen and nitrogen, the nitrogen only remaining after the extinction of the phosphorus. The gas which remains may be regarded as pure nitrogen, or, at all events, nitrogen very nearly pure.

That is one mode of preparing the element. Another plan consists in causing two compounds of nitrogen to decompose each other. If we mix together a compound of nitrogen and hydrogen, which we term ammonia, and a compound of nitrogen and oxygen, which we call hyponitrous acid, we have a solution containing just a sufficient amount of oxygen in the one compound to combine with the hydrogen in the other compound so as to produce water, leaving the two quantities of nitrogen that were contained in those two compounds in the elementary form. Now, nitrogen, prepared by either of these processes, possesses the following properties: It is a perfectly colourless and transparent gas. Here we have a jar of it, and just as we saw in the case of the oxygen and hydrogen, so here with nitrogen; you cannot distinguish a vessel filled with this gas from another vessel filled with atmospheric air. Here [in the bell-jar in which the phosphorus had just been burnt], at present, we have a quantity of white fumes mixed with our nitrogen. These white fumes consist of phosphoric acid produced by the combustion of phosphorus at the expense of the atmospheric oxygen. This phosphoric acid will gradually subside and dissolve, leaving the gas, after a few minutes, as clear and transparent as that which we have in the other jar, and which was prepared, in fact, by this process. Nitrogen possesses nearly the same specific gravity as atmospheric air. Taking atmospheric air as unity it is slightly more than ·97. It is very slightly soluble in water—far less so than oxygen. One hundred volumes of water dissolve only about one and a-half volumes of nitrogen, whilst about double this quantity of oxygen is dissolved. This greater solubility of oxygen than atmospheric nitrogen in water is a most important point connected with the existence of animal life beneath the surface of water. Fishes live in water by means of the atmospheric oxygen which is dissolved in that liquid. They have no power whatever of decomposing the water, and separating the oxygen from the hydrogen; but they require for their respiration free oxygen, just in the same way as animals living in the air; and they receive this oxygen from the quantity of air dissolved in water. If you deprive water of air by placing it under the exhausted receiver of an air-pump for a few moments, fishes placed in that water will very soon die, because they cannot obtain the supply of oxygen which is necessary to continue their respiratory functions. In consequence of water dissolving more oxygen than nitrogen, these animals have, so to speak, a dissolved atmosphere, far richer in oxygen than our ordinary air; and, therefore, by a comparatively much smaller bulk of air than land animals require they can continue their respiration, and obtain quite a sufficient amount of oxygen for their wants.

In ordinary language we generally describe nitrogen as being incombustible in itself, and incapable of supporting combustion. If we take a jar of it—let us take this jar here, for instance—and plunge into it a lighted taper, we shall see, on the one hand, that when the paper comes in contact with the gas the latter will not inflame, and, on the other hand, that the paper is extinguished when it is lowered into the jar; so that we gather from this experiment that nitrogen is incombustible; but these matters are not to be decided upon the dictum of a single experiment of this kind. It is quite possible that nitrogen may be very combustible at higher temperatures, and indeed we shall find that it is combustible to a certain extent at all events. Different combustibles require very different degrees of heat to set them a burning—to "ignite" them, as we call it. I have here a substance which is so easily ignited that the moment it comes in contact with the air, even at ordinary temperatures, it bursts into flame. I will expel some of it out of this tube—it is a liquid body—into the dish there; and you see this substance, as soon as it comes in contact with the air, ignites and burns brilliantly. Now phosphorus is another combustible body which will furnish us with a second link in this chain. The body which we have just expelled from the tube has its igniting point below the temperature of the atmosphere. The igniting point of phosphorus is higher than the ordinary temperature of the atmosphere, and if we bring a piece of this phosphorus into the air, you see it does not take fire; but it will take fire at a temperature not very much higher than that of the atmosphere, and if we float it in a copper capsule upon hot water, bring it in contact with the heated surface, our phosphorus will probably then ignite. [A piece of phosphorus was ignited in the manner described.] There you see at that temperature our phosphorous ignites and enters into combustion. Then, again, we have a substance, the bisulphide of carbon, which has an igniting point a little higher than phosphorus, but still far below an ordinary flame. We will warm this glass rod. I need not have it so hot that I cannot bear to draw it through my fingers. With this warm rod I dare say I shall be able to ignite this bisulphide of carbon. Yes; you see it ignites at this temperature, far below any of our usual igniting points. The rod is sufficiently hot, but I can draw it in this way through my fingers, although it possesses power to ignite this inflammable substance. Now, proceeding upwards, the ordinary gas flame which we burn has an igniting point still higher than this bisulphide of carbon, but lower, again, than some other bodies. The ordinary coal gas, if exposed to very bright red heat, ignites; but if it be exposed to a temperature below this very bright redness, that is, if you take an iron bar heated only to dull redness, it will fail to ignite this coal gas. Now, we have here another material which is still less combustible than coal gas; this material is a compound of nitrogen which I shall presently have to explain to you. It is called ammonia, or ammoniacal gas. We are generating it here in this vessel, and passing it through this iron tube which is heated to redness by a gas furnace. I dare say by the aid of that external heat applied to the ammonia we shall get it to burn at the extremity of the tube; but the ammoniacal gas, as we shall presently see, is not ordinarily combustible, but only when previously heated in this way can it continue its combustion. If we allow a jet of ammoniacal gas to impinge on a candle-flame it will burn around the flame, but will not continue its combustion when the flame has been removed; so that if we were to pass this ammoniacal gas simply through a cold tube and endeavour to ignite it at the end we should get no flame from it. Probably, our tube will now be heated sufficiently high. [A light was applied to the end of the tube from which the gas was issuing.] Yes, there our ammoniacal gas burns at the extremity of the tube quite readily. Now we will turn off the gas of the apparatus by which the tube is heated, and you will see that soon after that gas has been extinguished the flame of the ammoniacal gas will gradually withdraw itself from the end of the tube and finally be extinguished. It can no longer retain its ignition, because the temperature produced by the combustion of ammonia is by no means so high as its igniting point. We can only keep the temperature at the igniting point by previously passing the gas through this hot tube. It thus produces a higher temperature, just as you get a higher temperature in a furnace fed by heated air than you do in a furnace fed by ordinary air; and the difference between these five or six hundred degrees of heat is sufficient to raise the temperature of the burning ammoniacal gas beyond its igniting point, and therefore to render it capable of burning in the air. The element nitrogen, to which all these experiments lead us up, has an igniting point still higher than that of any of these substances. It is higher even than that of ammoniacal gas; and we require to expose it to a still higher temperature in order to bring about its combustion with oxygen. Further, like ammoniacal gas, nitrogen on its combustion with oxygen, that is, when you get it to burn by heating it to the proper temperature to render it combustible, produces, in burning, a less amount of heat —I mean to say a lower temperature is produced—than is equal to its igniting point. Its igniting point is higher than the temperature produced on its combustion. On this account the combustion cannot be propagated from particle to particle, or from mass to mass of the mixture of oxygen and nitrogen gases. If this were not the case it is obvious that all the nitrogen in the air would long ago have been burnt up. The first time that a philosopher obtained the heat of the oxyhydrogen blow-pipe he would inevitably have ignited the atmospheric nitrogen and burnt up the whole of the atmospheric oxygen. The combustion would have been propagated from particle to particle, had not the igniting point been higher than the temperature produced by its combustion. The union of the two gases which compose the air must have resulted from this ignition at a single point; but as the igniting point is higher than the temperature produced by the combustion of the nitrogen, the combustion of atmospheric air cannot be communicated from one point to another, but can only go on where the temperature is sufficiently high. The temperature of an ordinary fire is not sufficiently high for this purpose. It does not exceed about 4000° Fahr. The temperature of our ordinary oil, gas, and candle flames does not exceed about 3500° Fahr. The igniting point of nitrogen is considerably above that. It is impossible to measure these high temperatures, but I have calculated from an experiment with a mixture of oxygen and nitrogen, that a temperature of about 5400° is necessary to ignite nitrogen. Now, you see the temperature we have in a common fire, and that which we have in common gas and candle flames, are not sufficient for the ignition of nitrogen, and therefore we get no combustion of nitrogen in these flames—a circumstance which, as we shall presently find, is of great value to us, and contributes considerably to the comfort of our lives. We should have exceedingly noxious effects produced in the atmosphere by the ignition and combustion of nitrogen with oxygen. We have, however, some sources of heat sufficient to ignite nitrogen. One of these sources is the combustion of oxygen and hydrogen gases in the proportions to form water, or nearly so. If we mix these gases with nitrogen, and then ignite them, and add at the same time a certain excess of oxygen, the nitrogen burns, —to a limited extent it is true,—but it undergoes very definite combustion.

The temperature of the electric spark is also high enough to ignite nitrogen, being above the igniting point of that gas; and consequently if we pass a stream of electric sparks through a jar of atmospheric air, such as we have here, we shall have an ignition of the nitrogen which it contains, and its combustion at the expense of the atmospheric oxygen. You see at the present moment [exhibiting a glass flask containing air] this is quite colourless. If the nitrogen burns on the passage of the electric sparks, it will produce ruddy vapours in the flask. You will see, in the first place, a yellowish colour, and, finally, a reddish colour, produced by the action of the stream of electric sparks, as they pass between these two points. [A series of electric sparks from a Ruhmkorff's coil was then passed through the flask of atmospheric air.] We shall only get our colour after some time; but it is an experiment which is worthy our seeing, we shall therefore wait. Oxidation of the nitrogen is caused by the passage of the electric sparks through the jar, and consequently such an oxidation must take place every time a flash of lightning passes through the atmosphere.

Nitrogen in thus uniting with oxygen, forms an extensive series of compounds. Here we have a list of them in this table:—

Compounds of Nitrogen with Oxygen.

Protoxide of Nitrogen	NO
Binoxide of Nitrogen	NO_2
Hyponitrous Acid	NO_3
Nitrous Acid	NO_4
Nitric Acid	NO_5

In any proportions in which the oxygen does not exceed two equivalents to one of nitrogen, the compound obtained is neutral; when there is a larger proportion of oxygen than this, the compound becomes acid.

We have already a very perceptible yellow colouring produced in that flask [through which the electric sparks were still being passed]; and in the course of a minute or two more, I dare say it will be more perceptible. The compounds produced here are the three last mentioned in the table to which I have just directed your attention. They consist partly of hyponitrous acid, partly of nitrous acid, and partly of nitric acid. All these three substances are liable to be formed in this way; and in the action of lightning in thunderstorms these three compounds are produced, but the two compounds containing the smaller proportions of oxygen gradually pass into the last containing the highest proportion of oxygen; and therefore it is that we usually find present in the rain that descends after a thunder-storm a certain proportion of nitric acid.

We have now a very decided brown [again referring to the flask through which the sparks had been passing]. The operation is a very slow one, but the formation of these oxides of nitrogen continues to increase.

This is one source, then, of the fifth combination of nitrogen with oxygen—nitric acid, which we have next to consider. It is produced by the combustion of atmospheric nitrogen, and it is also found in a state of combination with bases. It forms nitrates with bases. Some chemists are of opinion that all the nitric acid which we meet with in combination with potash or soda, in the so-called "nitre districts," is due entirely to the putrefaction and decay of animal and vegetable substances; while others contend that it is due to the combustion of nitrogen by atmospheric electricity. Probably the proportion of nitric acid generated in the atmosphere, and present in the rain in this way, is comparatively small; and we find our chief supplies of this substance from the oxidation of nitrogen contained in animal and vegetable substances, but especially in animal substances. Now, nitrogen which is already combined is capable of undergoing this kind of combustion at the ordinary temperature of the atmosphere. It is going on around us, in fact, in the soil of London. Permeated as it is by the sewerage, we have there animal matters containing nitrogen undergoing this process of combustion with oxygen without any elevation of temperature whatever, producing immense quantities of this substance, nitric acid, which may be detected in all the shallow wells in and around London. It is especially abundant in the neighbourhood of burial grounds, where animal matter is oxidizing to a greater extent than it is in other localities; and the wells in those neighbourhoods have been found to contain a large proportion of the salts of nitric acid.

Nitric acid may be made by a process such as we have proceeding here. It is here being obtained by displacing it from its compounds with bases by means of sulphuric acid. We have added a quantity of sulphuric acid to this nitrate of potash, or common nitre, which is one of the best known salts of this body. Sulphuric acid is capable, under the influence of heat, of displacing the nitric acid. After the reaction we shall have left sulphate of potash in the retort, whilst nitric acid distils over.

Nitric acid thus obtained is a colourless, fuming, limpid, highly corrossive liquid, and contains one equivalent of nitrogen and five equivalents of oxygen, which we have represented on the diagram by the symbol NO_5, but our nitric acid contains also one equivalent of water. It is distinguished for its powerful oxidizing properties. It oxidizes metals with very great facility, converting them, first, into oxides, and, subsequently, into nitrates. Here is some of this acid which is used commercially on a large scale, being formed by this decomposition which we have going on here. The operation is performed in large cylinders which are lined with fire bricks in order to protect them from the action of the acid. If nitric acid is brought into contact with copper a very powerful oxidation of the copper takes place, with the production of these red fumes. These red fumes are the same as those which we have been forming in this flask, only you see here they have a greater intensity. Here is a greater amount of this red compound produced by the deoxidation of this nitric acid. It is from this deoxidation of the final compound of nitrogen and oxygen that we usually obtain our supplies of the other compounds of nitrogen and oxygen. We might perform the same experiment, also, with iron, which is oxidized in the same way and gives rise to the same red fumes. Nitric acid, however, is incapable of oxidizing all the metals in this way. If we bring it in contact with gold, for instance, no such oxidation occurs. [A piece of gold leaf was placed in a glass vessel containing nitric acid.] The gold floats about in the nitric acid with its brilliancy unaffected. It is also incapable of oxidizing platinum and a few other metals; but the great majority of metallic substances are capable of being oxidized by it. These oxides, being taken up by the remaining nitric acid present, form nitrates, and thus we produce our nitrate of copper, nitrate of iron, nitrate of zinc, and nitrates of other metals which we wish to obtain.

Nitric acid may be very readily detected by this very process that we have been performing here,—namely, by deoxidizing it by means of copper turnings. If we first set the acid free from any combination in which it may previously have existed, by the addition of sulphuric acid, as we have done here, and then bring copper turnings into contact with the liquid in which the nitric acid is thus set free, we have these red fumes produced which are quite characteristic of the presence of nitric acid. When the quantity operated upon is small, we may not, perhaps, perceive any of these fumes: in this case we can detect the presence of the nitric acid by those papers we employed on the last occasion for another purpose. Here are some papers which have been immersed in a mixture of starch and iodide of potassium, and you see they are powerfully acted upon by this gas; the action is, indeed, too powerful for you to see it properly. They are browned very strongly, but with a smaller quantity the paper is rendered blue. You see this bluing action better when the paper is moistened a little.

Now, you will remember that this is precisely the test which is used for the detection of ozone in the atmosphere. We mentioned it in the last Lecture, as being used to show the presence of ozone in the atmosphere, and we have seen that every electric spark that passes through the air produces this acid. How, then, can this be a proper test for the presence of ozone in the atmosphere? If we know that this nitrous acid produces the same effect upon the papers as was produced by what is supposed to be ozone, how can such papers be said to be a test for ozone? It is obviously quite unreliable. The effect would be produced, and I think probably is produced, by the nitrous acid derived from the passage of electric discharges through the air, and not by ozone at all.

From nitric acid we can obtain, by graduated deoxidation, the remaining compounds of nitrogen with oxygen. The first compound resulting from the gradual deoxidation, is nitrous acid, and the next is hyponitrous acid, which contains three equivalents of oxygen and one of nitrogen; the next compound which we shall come to, will be the so-called "binoxide of nitrogen," which contains two equivalents of oxygen and one equivalent of nitrogen. This is the substance which is produced when nitric acid is acted upon by copper turnings; in fact, we had it produced in the first experiment. In order to obtain this substance, NO_2, it is necessary to exclude the air; and here is an apparatus by which we can perform that experiment. The operation shall be going on while we are endeavouring to make ourselves acquainted with the properties of this gas. Here we have a vessel containing copper turnings. We pour a portion of nitric acid through this funnel-shaped tube, which passes nearly to the bottom of the jar. The gas, on being evolved, will pass out through this flexible tube, and be collected here, [referring to a jar inverted over the pneumatic trough.]

Binoxide of nitrogen is a perfectly colourless and transparent gas, like those we are already acquainted with. Here is a jar of it, for example, which has been already collected, and in which we shall be able to see some of its properties. It possesses a specific gravity only slightly greater than that of air, very slightly indeed, it being 1·04. It is slightly soluble in water, water absorbing about 5 per cent. of this gas; but still we can collect it over water without any material loss; that is, in fact, the mode in which this gas is generally collected. One of its most distinguishing characteristics is its strong affinity for a further amount of oxygen; it is, indeed, a chemical compound in a very unstable position, and it has a great desire for a further quantity of the element oxygen. If we pass some of this gas up into a jar containing oxygen, we shall find that it will at once produce those ruddy fumes that we had formed here, owing to the transformation of the binoxide into the next two compounds below it on the diagram—hyponitrous acid and nitrous acid. Here, for instance, is a jar half filled with oxygen, [the jar of binoxide of nitrogen was discharged into the jar of oxygen standing in the pneumatic trough.] You see where it comes in contact with the oxygen, these dense red fumes are produced, and you will notice after a very short time, that there is a considerable absorption of the red compound in the interior of the jar. You see how the water is rising in the interior of the jar taking the place of the gas which it absorbs. These two compounds, hyponitrous acid and nitrous acid, which were produced by the addition of the oxygen to the binoxide of nitrogen, are both of them very soluble in water, and are, therefore, very rapidly removed from the remaining oxygen present in the jar.

Binoxide of nitrogen presents a rather remarkable character in regard to combustibles. It is difficult to say whether it is a supporter of combustion or not. It is not inflammable, at all events; we can affirm that of it positively; and if we plunge a taper into this gas, you see the light is immediately extinguished, so that we should be rather inclined to conclude from that experiment that binoxide of nitrogen does not support combustion; and perhaps we should be quite right in that conclusion. The fact is, that unless the burning substance we place in the binoxide has sufficient heat in it to decompose the gas, transforming it into its elements, oxygen and nitrogen, the combustion is not supported. If, however, the combustible can decompose the gas, it will continue to burn in the mixture of nitrogen and oxygen which is formed. Phosphorus, for instance, is a substance of this kind. If we put a fragment of phosphorus into this jar of binoxide of nitrogen, if it is only just ignited the temperature of the phosphorus is not enough to decompose the gas; but if actively in combustion it continues to burn in the gas with very considerable brilliancy, especially after the phosphorus vapourises, and thus comes into extensive contact with the binoxide. But it is not in the binoxide itself that the phosphorus burns, because if we introduce it in only a langnidly ignited state, it is extinguished. Another substance which is capable of burning in this gas is the bisulphide of carbon, which we have already used in a former experiment. Here is our jar of binoxide of nitrogen; here is our bisulphide of carbon. We will pour a little bisulphide of carbon into the jar and agitate it a little, so that the vapour may mix pretty well with the gas. [A light was applied to the mouth of the jar, and a slight explosion, accompanied with a bright flash, took place.] This substance, you see, undergoes very brilliant combustion in the binoxide. The temperature of its combustion is sufficient to decompose the gas, and then the vapour of the bisulphide burns in the mixture, which contains an equal quantity of oxygen and nitrogen, and is, therefore, much richer in oxygen than atmospheric air. The bright light contains also a large amount of what have been called the "chemical rays," and therefore it has been used in photography. It enables you—in the dark, I was going to say—at all events, without sunlight, to take photographic pictures.

Now, the remaining compound of nitrogen with oxygen—namely, the protoxide—is obtained by a still further reduction of nitric acid, by which we remove a further quantity of its oxygen. This protoxide is a compound in itself unimportant, but it is rather singular in its effects. It is remarkable for its peculiar effect upon animals, and on this account it is called "laughing gas." If I could exhibit some of its effects before you, I should then consider myself justified in speaking longer about it; but as we cannot very well do that, I think we had better pass it by simply with the remark that it resembles the binoxide with regard to combustibles. Some substances will not burn in it. Sulphur, for instance, is at once extinguished; therefore we can say of all these gases that they do not support combustion when the oxygen is in combination; but if they are decomposed by the heat of the combustible, the products of their decomposition, nitrogen and oxygen, do exert a supporting effect upon combustion.

We now come to the compounds of nitrogen with hydrogen,—namely, ammonium and ammonia,—the respective compositions of which are represented here on the diagram:—

Compounds of Nitrogen with Hydrogen.

Ammonia, or ammoniacal gas .	NH_3
Ammonium	NH_4

These substances can neither of them be formed by direct combination with their elements. It requires indirect means in order to bring about these combinations. Both of these bodies are produced by the gradual decay or putrefaction of organic substances. Ammonia uniting with acids, or even with water, give rise to ammonium. But this ammonium is a somewhat hypothetical body. We have never as yet been able to get it in a free state. It is contained in chloride of ammonium, which is obtained by neutralising hydrochloric acid by ammonia. Chloride of ammonium contains one equivalent of ammonium united with one equivalent of chlorine. We might go a little farther than that, and form a kind of alloy, or amalgam, with this hypothetical substance, ammonium, by making it combine with mercury. Such an amalgam is produced when we act upon a solution of chloride of ammonium in water by an amalgam of sodium and mercury. The two metals, potassium and sodium, are capable of extracting the chlorine and leaving the ammonium disengaged from the chlorine. But the ammonium does not appear in a free state. It combines, upon the moment of its liberation, with mercury, and forms a kind of pasty amalgam, which reminds us of the amalgams formed by dissolving tin, zinc, lead, and other metals, in mercury. Here we have some amalgam formed with sodium and mercury, and I will pour some of it into this vessel [a shallow white dish], which contains a solution of chloride of ammonium. Now, you see this mass which I poured into that was not very large, but it is growing very considerably. It has nearly filled the lower part of the dish, and it has now changed its character. It is so light that it is floating upon the surface of the liquid here; and you see it forms a kind of butter-like mass which is capable of being moulded into any form, and taking a very high polish. Here we have some of this body forming in a tall jar, and you may there see it grow more clearly than in this basin. You see from that comparatively small portion of material what an immense mass of this butteraceous substance we have produced.

The other body, ammonia, is readily produced from chloride of ammonium, which we obtain from the destructive distillation of animal and vegetable substances. Gas liquor, obtained in the manufacture of coal gas, and bone liquor, furnish us with a great bulk of the ammonia contained in this chloride of ammonium. If we mix chloride of ammonium with quick lime or, better, with hydrate of lime—slaked lime—we remove from it the chlorine, and also one equivalent of the hydrogen which it contains, and we have then evolved, in the form of gas, ammonia or ammoniacal gas as it has been called with greater propriety. Here is some of it coming off; we will collect it at once, into this vessel containing water where it will be absorbed, and we shall obtain a solution of it. We have some of this gas already collected here in these jars; here is a jar of it, so that from this we can ascertain sufficient of its properties for our present purposes. It possesses a specific weight much less than that of the atmosphere; it is, bulk for bulk, about half the weight of atmospheric air, its specific gravity being about ·539. It cannot be respired in its concentrated form; if diluted with air and then respired it has rather exhilarating and pleasant effects than otherwise. It is the volatile gaseous matter which comes out of those Preston salts bottles which are pretty generally known, and which are found to be sometimes refreshing. Under these circumstances it is of course highly diluted with air. Ammonia is slightly inflammable, but you saw that it was necessary, in the experiment where we burnt ammonia in the previous part of the Lecture, first to heat it above its igniting point in order to have a continuous combustion. It is one of those gases which can be rendered liquid by pressure. It is a highly alkaline substance, and this is at once rendered evident if we dissolve some of it in a reddened solution of litmus. Here, for instance, is a solution of reddened litmus in water, by means of which we will absorb this jar of ammoniacal gas. [The mouth of the jar containing the ammonia was placed in the reddened solution, and upon the removal of the stopper beneath the surface of the liquid the water rushed up and filled the jar.] You see how rapidly it is absorbed by water and how the red colour of the litmus solution, which we employed for the purpose of showing that ammonia is alkaline, was transformed into blue when the liquid came into contact with the gas. Water in this way is capable of absorbing no less than 780 times its volume of this gas.

We must now rapidly pass on to commence, at all events, the history of another element, namely, Carbon, which exists in three forms in nature; namely in the form of diamond with which we are all acquainted; of graphite, which we know in those lead pencils used for marking upon paper; and in the form of charcoal as it is obtained by heating animal and vegetable substances to redness. These substances are all of them essentially the same element, and they produce on their combustion precisely the same compound. We will endeavour to burn three specimens of these substances, namely, diamond, graphite, and charcoal, and ascertain whether the substance produced is really the same in each case, and by that means establish the identity of the three substances. We will first endeavour to burn a diamond. This small piece of diamond is embedded in plaster of Paris in order to hold it conveniently. In order to burn it it is necessary to heat it to a very high temperature, and as its igniting point is very high, I shall require to light it in a jet of hydrogen in contect with oxygen. You see we have here a jar of oxygen, and when we have ignited the diamond to a sufficient temperature, we will plunge it into this jar, and then I hope it will continue its combustion. It will certainly do so unless we have not heated it sufficiently; but the experiment is rather difficult, unless we take a large diamond, and then it becomes rather a costly experiment. [The diamond was ignited by means of the oxy-hydrogen blow-pipe.] There, our diamond has got up to a white heat. [The diamond was then introduced into the jar of gas.] There is our diamond, burning as a bright point in the jar of oxygen, and when it has burned a little longer so as to give us an appreciable quantity of carbonic acid—for it burns very slowly—we will test whether we have really produced carbonic acid by this combustion of the diamond. Now, we will take, in the same way, a piece of graphite; in fact, a piece of a Mordan's pencil, and burn it in another similar jar of oxygen. In the meantime we will make sure that this is the jar in which the diamond was burned. Now, I think we shall not find so much difficulty with our graphite in getting it up to the proper temperature. You see that burns more readily than the diamond does; we have not to heat it to so high a temperature, and now it is introduced into the oxygen combustion goes on much more rapidly. The combustion will be very much sooner extinguished, although we have a quantity of graphite, five or six times the weight we had of the diamond. Sufficient carbonic acid has now been produced to make its presence manifest. Finally, we will take a piece of charcoal, which will not require to be heated much. [A piece of charcoal was burned in a jar of oxygen.] You see what a wonderful effect cohesion has upon this chemical process. There we had such difficulty in igniting our hard and compact diamond; we had less difficulty in igniting our graphite, which was less dense than the diamond; and, finally, we had no difficulty in igniting the charcoal. Now, let us see whether we have precisely the same product produced in all these instances. Here is some lime-water which we shall find to be a most delicate test for carbonic acid, and I will pour a little of it into each of these jars. [A quantity of lime-water was poured into the three jars in which the diamond, graphite, and charcoal had been burned.] There, you see, we have produced a quantity of chalk; we have formed carbonic acid from the combustion of that diamond; the carbonic acid has united with the lime, forming the carbonate of lime or chalk. Here is the result of the combustion of the graphite which is identically the same; and in the jar in which the charcoal was burned we have also carbonate of lime precipitated, showing the production of carbonic acid in that jar; so that here we have precisely the same result in all three cases.

NOTICES OF BOOKS.

On the Composition of Oxen, Sheep, and Pigs, and of their Increase whilst Fattening. By J. B. Lawes, F.R.S., and Dr. J. H. Gilbert, F.R.S.

The pamphlet which has recently appeared under this title sets forth the results of an extended series of analytical experiments in continuation of those already communicated by the same authors to the Royal Agricultural Society. The object has been, in the present instance, to investigate the nature of the increase, and the relative development of the different organs or parts of fattening animals under various conditions of feeding. As a process of "meat manufacture," therefore, where the value of raw materials and the proportionate yield of saleable carcass have to be considered, the question directly addresses itself to the agriculturist, constituting, indeed, a highly important branch of the business of the farmer; and to the chemist opens up a wide field for investigation in reference to the disposal in the animal economy of the several constituents of food,—to what extent they contribute directly to the formation of flesh,—and what proportion is necessarily lost in the form of offal and waste, or as products of inferior commercial value.

A diligent study of the numerous Tables in the original treatise is requisite to place the inquirer in the full possession of the details of an investigation so laborious as that undertaken by Messrs. Lawes and Gilbert. The experimental results are drawn up for comparison, and exhibited in twenty-one tabular statements. In these columns the quantity and nature of the food consumed, with the gross increase resulting therefrom,—the proportion subsisting between the several parts of the animal,—the distinctions of offal and carcass,—and (in pigs) the relation of fat to lean, are conspicuously stated. Then, in regard to the chemical composition of the parts of the animal, the distinctions of fatty, nitrogenous, and mineral constituents and water, are deduced from means of several individuals in each class. And, lastly, the increase of the animal whilst fattening is referred, in like manner, to the several constituents of the food consumed.

The main conclusions from the whole inquiry may be briefly enumerated as follows:—

I. *Food and Increase.*

Fattening oxen fed liberally upon good food, composed of a moderate proportion of "cake" or corn, some hay or straw chaff, with roots, or other succulent food, and, well managed, will, on the average, consume from 12lbs. to 13lbs. of the dry substance of such mixed food per 100lbs. live-weight per week; and should give 1lb. of increase for 12lbs. to 13lbs. of dry substance so consumed.

Sheep fattening, under somewhat similar circumstances, but with a less proportion of hay or straw, will consume about 15lbs. of the dry substance of the mixed foods per 100lbs. live-weight per week, and should yield, over a considerable period of time, 1 part of increase in live-weight for about 9 parts of the dry substance of their food. If the food be of good quality, oxen and sheep may give a maximum amount of increase for a given amount of total dry substance of food, even provided the latter contain as much as 5 parts of total non-nitrogenous to 1 of nitrogenous compounds.

Pigs fed liberally upon food composed chiefly of corn, will consume from 26lbs. to 30lbs. per 100lbs. live-weight per week of the dry substance of such food. They should yield 1 part of increase in live-weight for 4 to 5 parts of the dry substance of the food. They may give a maximum amount of increase for a given amount of dry substance of such food, if it contain as much as 5 or even 6 parts of total non-nitrogenous to 1 of nitrogenous compounds.

Taking into consideration the cost of the foods and the higher value of the manure from those which are rich in nitrogen, it is frequently the most profitable for the farmer to employ,—even up to the end of the feeding process,—a higher proportion of nitrogenous constituents in his stock-foods than is necessary to yield the maximum proportion of increase in live-weight for a given amount of dry substance of food.

II. *Proportion of Parts.*

In proportion to their weight, sheep yield rather more internal loose fat than oxen, and pigs very much less than either.

As oxen, sheep, and pigs mature and fatten, the internal organs (offal parts) increase in actual weight, but they diminish in proportion to the weight of the animal.

Well-bred and moderately-fattened oxen should yield about 60 per cent. carcass in fasted live-weight, and sheep in similar condition a like amount. Excessively fat oxen may yield from 65 to 70 per cent. carcass.

When the fattening food of oxen, sheep, and pigs contains less than about 5 parts of non-nitrogenous to 1 of nitrogenous compounds, the proportion of gross increase for a given amount of dry substance of the food will not increase with the increased proportion of nitrogenous compounds; the proportion of carcass to the live-weight will probably be somewhat less, and the carcasses themselves will be somewhat more bony and fleshy, and less fat.

III. *Chemical Composition of the Animals.*

Of dry fat the entire body of a fat calf contained about

14½ per cent.; of a fat ox, 30 per cent.; of a fat sheep, 35½ per cent.; and of a moderately fat pig, 42 per cent.

In the fattened condition, the entire bodies of calves and oxen will probably contain about 4 per cent. of mineral matter; those of lambs and sheep, under 3 per cent.; and those of pigs, on an average, 1½ per cent.

This mineral matter may generally be reckoned to contain nearly 40 per cent. of phosphoric acid, and about 6 per cent. of potash.

The mean composition of six animals analysed in a condition fit for the butcher shows about 3 per cent. mineral matter, 13 per cent. nitrogenous compounds, and 33 per cent. fat; in all, about 49 per cent. total dry substance, and 51 per cent. water.

The proportion of mineral matter in the bodies of oxen, sheep, and pigs rises and falls with that of the nitrogenous compounds.

IV. *Composition of Increase.*

The increase of liberally-fed oxen over the last six months of the fattening period will probably consist of 70 to 75 per cent. total dry substance, of which 60 to 65 parts will be fat, 7 to 8 parts nitrogenous compounds, and about 1½ part mineral matter.

V. *Relation of Constituents in Increase to Constituents Consumed.*

Sheep fattening for the butcher on a good mixed diet will seldom carry off more than 3 per cent. of the consumed mineral matter. They will probably carry off less than 5 per cent. of the consumed nitrogen, if the food be comparatively rich, and more than 5 per cent. if it be comparatively poor in nitrogen. They should store up about 10 parts of fat for every 100 parts of non-nitrogenous substance consumed.

Pigs liberally fed on fattening food should store up 20 parts, or more, of fat for every 100 parts of non-nitrogenous substance consumed. These animals were found to store up four or five times as much fat as was supplied ready formed in their food.

If the produced fat were formed from starch, about 2½ parts would be required for the formation of 1 part of fat. In the sense here supposed, if the fat were so formed, about two-thirds of the dry substance of the food would be expired, perspired, or voided, without directly contributing to increase.

The comparative values of our current fattening food-stuffs, as a source of saleable animal increase depend more on their amount of digestible and assimilable non-nitrogenous than on that of the nitrogenous constituents; but, as a source of manure, their value will be the greater the higher their proportion of nitrogenous compounds.

Such are the main points which the authors deduce as the result of their observations. In reviewing these, we are willing to allow that many of the conclusions appear to be fully borne out by the experimental and numerical data urged in their support, and are, moreover, in general accordance with popular opinion. The relation of parts in the animal, and their average rate of increase, would, doubtless, be considered as established on sufficient basis. There are some points, however, to which we should be disposed to take exception, partly by reason of what may be termed the superfluous manner of employing and stating the results. Thus, in Table V., in the last column, are given the absolute weights of the carcass and the several internal organs of the sheep, calculated as a mean of 249 individuals; yet to furnish this mean, or average, 5 sheep in the lean (store) condition are included with 45 others excessively fat; and we find, in consequence, that the item "caul fat," which is shown to vary in the sheep from 2¾lbs. to nearly 10lbs., according to the condition of the animal at the time of slaughter, is brought out, nevertheless, and made to give a general mean in the last column. The expediency of taking so many as 100 sheep to furnish an average for the "moderately fattened" is, we think, a little questionable.

In reference to the chemical composition of the animal bodies, in their entirety and as separate parts, it is to be regretted that no particulars are afforded explanatory of the analytical methods employed and of the general system of operating. We are not informed whether, for instance, the whole organ (and frequently no less would be satisfactory) was consumed in an examination for mineral constituents; in what manner the relative proportions of nitrogenous and non-nitrogenous matters were estimated. These and other questions suggest themselves as difficulties in the working of chemical processes on a scale sufficiently extensive to ensure trustworthy results. That the authors are themselves fully prepared to admit the difficulty of an investigation such as that attempted is evident, among numerous references, from the paragraph with which we conclude this notice:—

"The results must, indeed, be looked upon as only approximations, though we believe the data now supplied constitute the most reliable basis for estimates of this kind at present at command."

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Iron and Nitride of Iron.—By passing a galvanic current from a Daniell's battery through a pure solution of chloride of iron H. Kramer obtained (*Chemisches Central-blatt*, No. 18, April 24, 1861), on the kathode a thin coating of metal, which easily took a bright polish. On increasing the strength of the current, so that hydrogen was freely evolved, he procured the reduced iron in a spongy mass so soft and porous that it could be flattened to a thin plate by the pressure of the thumb-nail.

The mass reduced by Bottger's method from a mixed solution of protosulphate of iron and sal ammoniac is very different. It is as hard as steel and as brittle as glass, and on examination is found to be not pure iron but nitride of iron. If this mass be rubbed to powder, which its brittleness easily permits, and then is washed until the washings no longer give any precipitate with a silver solution, it still yields when heated with soda-lime, an alkaline gas, which gives a white precipitate when passed into a solution of chloride of mercury. One gramme of the mass gave 1·420, and in a second experiment 1·421, of peroxide of iron, which makes the proportion of nitrogen contained 1·49 per cent. The author thinks that this proof of the ease with which nitrogen combines with iron is confirmatory of Fremy's views of the chemical constitution of steel, and also Schafhault's on the composition of English white iron.

A Double Salt of Chloride of Calcium and Carbonate of Lime.—In making a solution of commercial chloride of calcium, Fritzsche observed (*Bulletin de St. Petersb.*, tome iii. p. 285) that a small quantity of sandy powder remained undissolved. As long as the solution of the chloride was concentrated, this powder had a yellowish appearance, but on diluting the solution it became white. On searching for the cause of this change in appearance, the author found that the powder was composed of crystals of a double compound of chloride of calcium and carbonate of lime, and that the alteration in appearance was owing to a decomposition effected by the water. Some of the crystals placed on a glass plate with a little water, and examined immediately by the microscope, were seen at first to be perfectly transparent; but soon the surfaces became cloudy, and a granular separation was apparent. In consequence of the solution of the chloride of calcium they soon lost all their transparency, and after a time there only remained a skeleton of carbonate of lime of the size and shape of the original crystals. Some of the sandy powder separated very carefully from the chloride of calcium was found on analysis to have the composition $Ca\ Cl + 2CaO\ CO_2 + 6HO$.

II. ORGANIC CHEMISTRY.

A New Acid by the Oxidation of Nitro-Benzine.—When nitro-benzine is oxidised by boiling either with a solution of permanganate of potash or a mixture of nitric acid and bichromate of potash, there is found among the products a peculiar acid. The oxidation is most conveniently effected by the last-mentioned means, but care must be taken to have an excess of nitro-benzine. The new acid is soluble in hot nitro-benzine, but separates on cooling in small white crystals, which remain suspended in the solvent. This is separated by decantation, and is then shaken up with an excess of ammonia, which dissolves the new acid and another, forming a deep yellow salt much like picric acid in appearance. The solution is treated with hydrochloric acid, which causes the new acid to deposit. It is then washed with distilled water to remove chloride of ammonium and also some yellow acid which accompanies it. So purified, it has the following properties:—It is colourless, has a sharp, slightly bitter taste, and presents itself in slender needles irregularly grouped together. It fuses at a temperature little above the ordinary, and completely volatilizes, condensing in well-formed brilliant needles. Almost insoluble in cold water, and but little soluble in boiling water, it dissolves freely in alcohol, ether, and nitro-benzine. It dissolves, also, in hot acetic acid, and crystallises out again on cooling. Several analyses of specimens prepared by different methods led the authors (MM. Cloez and Guignet, *Bulletin de la Société Chimique de Paris*, No. 1, p. 3) to the following as the formula of the new acid:—$C_{18}H_7(NO_4)O_6$, and induce them to regard it as a product of the oxidation of nitro-cinnamic acid $C_{18}H_7(NO_4)O_4$. In conclusion, they remark that they have experimented with commercial nitro-benzine, and the new acid may come of the oxidation of some foreign body in that article.

Preparation of Aniline Red by Arsenic Acid.—We may add the following (*Polytechn. Centralblatt*, 1861, s. 493) to the account we gave of this process at p. 292, vol. ii. It seems that if an excess of aniline be employed no result is obtained until this excess has distilled over. If, on separating this colour afterwards, it is found to be contaminated with tarry matter, it is best removed by mixing the whole with sand, and then adding a little hot water, and filtering. Sometimes it happens that the dyer fails in using aniline red, because the fuchsine has got combined with ammonia. In such a case the addition of a little acetic or any other acid will make the bath again available. Gerber-Keller (*Ibid*, s. 398) has repeated Dr. Hofmann's experiments on the action of bichloride of carbon on aniline (See Chemical News, vol. ii. p. 292) with exactly the same results. He obtained an oily or doughy mass, which in time changed into a hard, brittle product, according to the temperature and strength of the action. Between 180° and 195° the author obtained in every experiment with Hofmann's process a magnificent crimson-red dye, which is without doubt identical with the Fuchsine of MM. Verguin and Rénard, Brothers. The chloride of carbon acts on aniline, but very slowly,—even at high temperatures,—while the production of azaleine with certain easily-reducible oxygen salts takes place in a few hours. Dollfus-Galline confirms the foregoing, but remarks that the colour prepared by Hofmann's method has a yellowish tone. In general, he says, the aniline reds made with chlorine compounds have a vermilion tint, while those prepared with oxygen salts have a violet. Gerber prepares his azaleine at a temperature which never exceeds 109° C. In Hofmann's method 200° must be employed, but this need be no obstacle to its technical application. The author thinks it probable that a purer red may be obtained by this than by any other process.

MISCELLANEOUS.

SACRIFICIAL METALS.

A curious statement made in the House of Commons, a few days ago, induces us to offer a few remarks on what may be called, appropriately enough, "the sacrificial function in metals." It was stated, our readers will remember, that the leaden envelope with which the Armstrong shell is surrounded, and which is absolutely necessary as a constituent part of that missile, might be caused to adhere securely, either by "a sort of mortise" or by the intermediation of zinc. The second expedient is that indeed adopted; with what amount of success the recent practice of these guns in China would disclose, were the Government not so pertinaciously bent on holding back the communications which have reached them on this matter.

It is a fact perfectly well known to chemists, but one which mechanicians have not heeded nearly so much as it deserves, that when two metals are retained in contact, and conjointly exposed to chemical influences during long periods of time, one metal sacrifices itself to ensure the other's preservation. The history of mechanical construction furnishes numerous illustrations of this chemical fact, as will be presently seen, when we come to cite a few examples; the marvel, then, is, that Sir William Armstrong—or, rather, the laboratory branch of the War Department, before whose cognizance the proposition of zinc soldering (thus to designate it) must have come—could ever have sanctioned an expedient which must necessarily fail in the end, like every other expedient which violates a law of nature. The history of ships' copper sheathing shall be taken as our first illustration of the sacrificial metallic function. Sir Humphry Davy, as is well known, devised a method of checking, or altogether obviating, the destruction of ships' copper sheathing. He accomplished this by attaching to the ship's bottom, at suitable intervals, slabs of the metal zinc, and which he called protectors. Protectors they were, in the following manner:—The zinc rapidly corroded, and was lost; but, so long as any zinc remained for the sea-water to act upon, the copper remained untouched. So far as copper preservation is concerned, the method adopted must be pronounced efficient. Practically, it failed, indeed, to secure the advantages described, but not for any reason that concerns us here. It was found that when copper no longer slowly dissolved, it ceased to be a poisonous metal. Barnacles and sea-weed attached themselves, just as they might have done to an uncoppered wooden bottom; and, moreover, owing to a galvanic action set up, the ship's copper was rapidly fouled by a deposition of magnesia and lime, precipitated from the magnesian and calcareous soluble salts always present in sea-water. Failing, then, to achieve what was intended of it, the copper-protecting process of Davy ceased to be employed; but the failure of it is that which alone concerns us here, as illustrating what we would wish to convey by the words "sacrificial metal."

Take, as the next example, the illustration afforded by the setting of an iron paling-rail in a bed of lead. The most casual observation, as one passes along the streets of this city, may be made more pregnant with fact bearing upon this matter than the longest homily. For a time, varying as to length with locality, external influences, and—perhaps, in some degree—on the varying quality of the iron and lead brought into contact, both metals remain sound. But, after a time, decay inevitably sets in, and, when once commenced, marches to the issue of final destruction with wonderful rapidity. The remarkable fact is, that both metals do not decay simultaneously; it is the iron which corrodes, whilst lead—the softer metal—

remains intact. Let the mechanician do what he will, this result cannot be prevented—a law of nature having decreed the sacrifice, it must and will be achieved.

Our next illustration shall be ordinary tin plate—iron, coated with tin, as is well known. Well, what sort of destruction is set up when tin plate has been exposed for a long duration of time to the atmosphere? Is the tin destroyed—does it tarnish even? Never. Iron is the sacrificial metal here; and, as surely as the iron is anywhere exposed, it perishes, crumbles, and dissolves away, with destructive rapidity. The rate of destruction of iron totally unprotected is slow, by comparison with that which ensues when, owing to a flaw or disintegration of the covering tin envelope, atmospheric agencies come into play upon the underlying metal.

But the case we most particularly wish to direct attention to, is the result of bringing zinc into contact with iron, and retaining the two metals together over long periods, as may be seen in the so-called galvanized iron. Under this latter disposition, zinc becomes the sacrificial metal. Not one particle of iron decays, so long as atmospheric destructive agents can wreak their dissolution—so to speak—on the protective zinc. Nor does this protection altogether depend on a complete covering of the iron. Flaws and imperfections there are, and necessarily must be, through which, quite down to the iron, destructive agents, always present in the atmosphere, must penetrate. They do not act upon the iron, nevertheless, so long as a sufficient expanse of protective zinc surface remains; and this simply because a fundamental law of nature forbids their doing so.

Now, if our remarks be true—and we are content to appeal to chemical science for a verdict—what becomes of the efficiency claimed for the Armstrong shell-envelope, because of the attachment of lead by a process of zinc soldering? Sound, to the eye, these projectiles may at first seem; but a disruptive force will be acting upon them, nevertheless, from the very beginning; and in total ruin and destruction this force will eventuate at last; whatsoever Sir William Armstrong's advocates may assert, notwithstanding—whether at Elswick or at Woolwich, whether in Parliament or out of it.—*Mechanics' Magazine.*

Royal Institution.—The following is the arrangement of Lectures for the ensuing week:—Tuesday, May 14, at 3 o'clock, Mr. John Hullah, "On the History of Modern Music." Thursday, May 16, 3 o'clock, Mr. W. Pengelly, "On the Devonian Age of the World." Friday, May 17, 8 o'clock, Professor J. Clerk Maxwell, "On the Theory of Three Primary Colours." Saturday May 18, 3 o'clock, Professor Max Müller, "On the Science of Language."

Chemical Society.—The next meeting of this Society will be held on May 16, when there will be a discourse delivered by W. H. Perkin, Esq., "On the Colouring Matters obtained from Coal-tar."

Royal Institution of Great Britain.—At the General Monthly Meeting, held on Monday, May 6, 1861, Sir Henry Holland, Bart., M.D., F.R.S., in the Chair, the Hon. Lieut.-Gen. Thomas Ashburnham, C.B., Lieut.-Colonel W. MacGeorge, and William Reid, Esq., were elected Members of the Royal Institution; the Duke of Chartres, the Count of Paris, Stephen Goodfellow, Esq., M.A., and Alexander Mackintosh, Esq., were admitted Members of the Royal Institution. The following Professors were re-elected:—William Thomas Brande, Esq., D.C.L., F.R.S., Hon. Professor of Chemistry., John Tyndall, Esq., F.R.S., as Professor of Natural Philosophy. The presents received since the last meeting were laid on the table, and the thanks of the Members returned for the same.

To Unite Stone and Glass.—M. Bru, Curator of the Museum at Narbonne, has discovered that silicate of potash possesses, in the highest degree, the property of uniting surfaces of stone, glass, and pottery. It is applied with a brush to the surfaces which it is desired to bring into contact, and in a few days acquires a great solidity. It appears that the same material can also be successfully used in joinery, and for all the purposes to which common glue is applied. This discovery, which promises to be of considerable importance, was announced by M. Bru to M. Figuier, the Editor of *L'Année Scientifique et Industrielle*, the volume of which work for the present year contains, at p. 481, a paper on the subject, and a letter from M. Bru to the editor.—*Builder.*

Treating Iron in Japan.—A literary friend sends me the following:—"I have just stumbled on the following in 'Mandelslo's Travels in the Indies,' at p. 190, 'They (the Japanese) have, amongst others, a particular invention for the melting of iron without the using of any fire, casting it into a tun, done about on the inside with about half a foot of earth, where they kept it hot with continual blowing, and take it out by ladelsfull to give it what form they please, much better and more artificially than the inhabitants of Liège are able to do.'" This would really seem to describe, in an imperfect manner, the fact that Bessemer's discovery has been old knowledge for ages in the "far Cathay," and that, like printing and gunpowder, the knowledge of them has only been communicated to, or re-discovered by, his younger brother Japhet. When a boy I myself recollect well a country blacksmith, in the North of Ireland, who used char peat as fuel, showing me, as a curious experiment, that, by taking a tolerably large flat bar of iron from the fire, when at that temperature above the welding heat when its angles liquefy and begin to scintillate and burn off, and then at once blowing upon the bar strongly with a pair of hand or house bellows, the whole mass, for a few seconds, burnt and flew about like a firework, doing so until the stream of air cooled it down too low. I had little notion what fruitful truths lay hid in that little exhibition. These notices of the "heatings in the dark," as Bacon calls such heraldings of every great discovery, may be of interest thus to record for some future historian of iron metallurgy, by whom Bessemer's name will be undoubtedly honoured. —*Robert Mallet.*

Hermised India-rubber.—India-rubber has been known about a hundred years, and only within the last twenty years has it been found of much practical use. In the town of Beverly, Mass., for some years past, there was a manufacturing company than used a devulcanizing process, taking old rubber and making it up chiefly into india-rubber cloth, under a patent with which Goodyear's did not interfere. And for the last four years they have been working the raw rubber by a process of vulcanization without the use of heat. The rubber is put into a solution of chloride of sulphur and sulphuret of carbon, and the change is effected in its properties in a few minutes. This process is called "hermising," to distinguish it from vulcanizing. The patent is Mr. Parmalee's. The hermised rubber possesses substantially the same properties as the vulcanized. It has the advantage, however, of being made of a lighter colour, and therefore of receiving quite brilliant tints. The lighter colour arises from the fact of rubber being less exposed to the atmosphere during the process, which exposure in the ordinary process turns the product almost black; and to make it lighter, zinc white or some other kind of white material is required to be added. India-rubber springs are passing out of use; it being found that the jolting of the vehicle causes the india-rubber at last to lose its elasticity. Vulcanized rubber subjected to the action of steam for the space of about three hours becomes perfectly brittle.

The New Zealand Iron Sand.—The following correspondence on this subject has taken place in the columns of the *Engineer*:—Mr. Mushet writes: "Sir,—Having received from Captain Morshead, for the purpose of converting into steel, about three tons of the Taranaki metalliferous sand, nothing surprised me more than the almost perfect purity of this sand; of which 98·25 per cent was strongly attracted by the magnet, and 1·75 per cent. only of quartz sand remained unacted upon. The analysis of this ore by Messrs. Johnson and Matthey showed the ore to be composed of:—

Oxide of iron	88·45
Titanic acid	11·43
Loss	·12
	100·00

Recently I have received from a party not interested in the matter, a fair sample of the Taranaki titanium sand, and which I find is composed as under:—

Magnetic iserine sand	33·60
Non-magnetic ilmenite sand	61·27
Sea sand	5·13
	100·00

And according to the analyses given of magnetic iserine and non-magnetic ilmenite, the Taranaki sand as it lies on the shore is composed as under:—

Oxides of iron	61·14
Titanic oxide	29·73
Sand	5·13
	100·00

And the yield from the ore in metallic iron will only amount to about 49 per cent. From another source, much nearer home, I have been supplied with a similar titaniferous sand of precisely similar quality; so that I infer that Captain Morshead has been misled as to the purity and richness in iron of the Taranaki sand, which, as supplied to me, appears to have been separated by a magnet from the non-magnetic bulk of the deposit. Or are there two deposits of titaniferous sand at Taranaki, one pure and the other impure? Perhaps Captain Morshead will be kind enough to explain. It will make a most essential difference in the success of the smelting operations which may be undertaken, whether the ore employed be the pure magnetic iserine or the mixed ore consisting chiefly of non-magnetic ilmenite." In reply to this, Captain Morshead writes as follows:—"Sir,—I can have no hesitation in affording to Mr. Mushet, through your columns, the explanation he seeks in his letter of the 20th March, and published by you on March 22. The sand I forwarded to him was, if anything, below a fair average sample, as could be vouched, if necessary, by four or five 'parties,' to use the words of Mr. Mushet, 'not interested in the matter.' There are not two deposits at Taranaki, one pure and the other impure, as suggested by Mr. Mushet. The whole is pure. I did not separate the three tons I gave Mr. Mushet (which were a portion of about ten tons sent over to me) from any other sand by a magnet. The suggestion of picking up such a quantity by such a process is a simple absurdity. If anyone will take the trouble to compare Mr. Mushet's present letter with those formerly written by him on the same subject, he will readily discover that Mr. Mushet, having failed to attain some end proposed by himself at the beginning of our acquaintance, is now seeking to destroy the effects of his former letters, and injure a project he can no longer control. For the present, I am content to rest upon the analysis of the material made (not for me, or at my request) by Messrs. Johnson and Matthey, corroborated as it is by the certificate contained in Mr. Mushet's last letter, of 'the almost perfect purity of the sand,' from which, I am glad to inform him, that a steel, far surpassing any made by him, has been manufactured by other parties, and which will shortly be introduced, in large quantities, into the market."

Vapour from Water.—Take a glass bottle of very clear glass and well cleaned with sulphuric acid, and afterwards with clean warm water, then place the bottle on a stand or block of wood, and let the rays of a strong light through a lens fall in the bottle, so that the focus of the lens is about in the middle or near the side of the bottle where you want to observe the vapours; when this is done pour a little hot water in the bottle and close it with a loose stopper, then the vapours can be seen very distinctly with a common magnifying glass or microscope with a long focus placed in an angle of about 150 deg. to 170 deg. with the rays of the light. Should condensation take place on the sides of the bottle a little shaking will take it away. If the experiment is made in a warm room, and the bottle well cleaned, as before described, condensation will not be any annoyance. Then raise the bottle so that the rays of the light fall over the surface of the water, and, by careful watching, the vapours can be seen rising from the surface of the water, and also disappearing on the surface of the water. I have seen them to rise from the water, and falling back again, and floating a short time on the surface, then disappear, as a drop of water will do sometimes. Here the vapours are in a restless state, but if the water is poured out of the bottle and placed on the stand again then the vapours will come more to rest. I believe that any person, after observing this, will come to the conclusion that they are water, therefore they cannot be formed in the body of water. If all vapours have disappeared by lowering the temperature above the surface of water a great quantity of vapours are produced in the bottle (miniature fog). This can be done by blowing cold air, or introducing a cold body or a few drops of cold water.—*F. Hoffmann, in the Engineer.*

ANSWERS TO CORRESPONDENTS.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

⁂ All *Editorial Communications* are to be addressed to Mr. Crookes, and *Advertisements* and *Business Communications* to the Publishers, Griffin, Bohn & Co., at the Office, 10, Stationers' Hall Court, London, E.C.

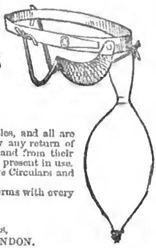
Vol. II. of the Chemical News, containing a copious Index, is now ready, price 12*s.*, by post, 12*s.* 8*d.*, handsomely bound in cloth, gold lettered. The cases for binding may be obtained at our Office, price 1*s.* 6*d.* Subscribers may have their copies bound for 2*s.* if sent to our Office, or, if accompanied by a cloth case, for 6*d.* A few copies of Vol. I. can still be had, price 10*s.* 6*d.*, by post 11*s.* 2*d.* Vol. III. commenced on January 5, 1861, and will be complete in 26 numbers.

Metallic.—We do not know where it is to be procured. An advertisement in our columns would at once bring you the desired information.

Arseniuretted Hydrogen.—*J. O.*—You can prepare this gas by forming an alloy of 32·1 parts by weight of zinc and 37·6 parts metallic arsenic; upon acting on this with hydrochloric acid, pure arseniuretted hydrogen is evolved, and may be collected over water. The most scrupulous care must be taken that not the smallest quantity be inhaled as it is a deadly poison.

Pink Manganese Salts.—*Enquirer.*—The pink colour of the salts of manganese is generally due to the presence of a higher oxide; sometimes, however, a trace of cobalt is present which causes the colour. The addition of a bitter sulphurous acid at once destroys the colour, if due to a higher oxide of manganese, whilst it is without action on the colour produced by cobalt solution.

Chrome-Green.—*W. D.*—An account of this colour was given in the First Volume of the Chemical News, p. 94.



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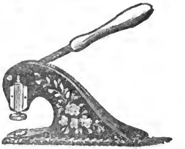

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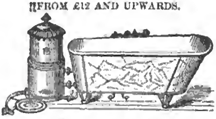

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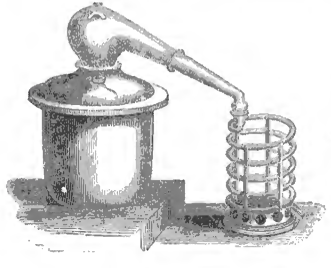

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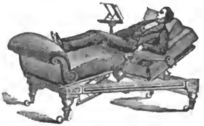

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PULVIS JACOBI *VER.*, NEWBERY'S.

To the Medical Profeſſion of Great Britain and Ireland.

GENTLEMEN,—We beg to call your attention to the following extracts from a Paper by the late John Cheyne, M.D., F.R.S.E., M.R.I.A., Phyſician to the Hardwicke Fever Hoſpital, Dublin, and Phyſician General to His late Majeſty's Forces in Ireland, &c. &c., contained in "*The Dublin Hoſpital Reports,*" *vol.* 1, *p.* 317.

> "Since this caſe came into my poſſeſſion, I have been led to make a good many clinical experiments, which have enabled me to verify the reports which I had heard of the efficacy of JAMES'S POWDER, in ſometimes removing the APOPLECTIC DIATHESIS in perſons advanced in life. It is, moreover, worthy of obſervation, that JAMES'S POWDER has been of remarkable utility in certain inſtances of DETERMINATION OF BLOOD TO THE HEAD, which occurred at an early period of life, and threatened to end by effuſion. Laſtly, in two caſes of GENERAL PLETHORA, in which, however, the head was more affected than any other part, JAMES'S POWDER was exhibited by me with perfect ſucceſs."

Alſo *vol.* 1, *p.* 320.

> "The following very ſimple method of exhibiting JAMES'S POWDER, in caſes of undue determination of blood to the head, is that which I have generally purſued. The patient is made to begin with a very moderate doſe, not more than two grains at bedtime, and to increaſe the doſe by half-a-grain every night, until ſome ſenſible effect is produced upon the ſtomach, bowels, or ſkin. Should the ſtomach be affected with ſickneſs, the doſe muſt be leſſened by one grain on the following night. By the addition of a little rhubarb to it, a larger quantity of JAMES'S POWDER may be adminiſtered than the ſtomach could otherwiſe bear. If the ſkin is ſoftened, or the bowels affected, the doſe ſhould not further be increaſed, but it muſt be repeated every night for a conſiderable length of time: in ſeveral inſtances I have known eighteen or twenty grains taken for a conſiderable period without any inconvenience."

The following extract (ſhowing the increaſing eſtimation in which the true Dr. James's Powder is held by Medical Practitioners) is taken from "The Lumlæan Lectures," 1858-59, delivered before the Royal College of Phyſicians, by A. Tweedie, M.D., Phyſician to the London Fever Hoſpital, who is admittedly one of the higheſt practical authorities on the ſubject of Fever. He ſays, (the "*Lancet,*" *June* 16, 1860, p. 590,) as follows:—

> "If the febrile excitement be ſuch as to require antimonial preparations, a pill containing 2 or 3 grains of James's Powder (prepared by Newbery) ſhould be taken at intervals according to circumſtances."

In writing to Meſſrs. Newbery, he ſays:—

> "I wiſh that your preparation was always uſed."

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This ſubſtitution muſt bring diſappointment, and of courſe deter Practitioners from preſcribing Dr. James's Powder, or lead them to regard it as a preparation on which no dependence can be placed.

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THE CHEMICAL NEWS.

Vol. III. No. 77.—May 25, 1861.

WATER ANALYSIS.

On a recent occasion[1] we had an opportunity of pointing out some notable discrepancies which have arisen from the want of a good general system of classification, according to which the numerical results furnished by the chemical examination of waters might be arranged, and represented in their probable state of combination, or as saline constituents actually present in the water. With the object of eliciting a more decided expression of opinion in regard to this subject, we advert to some additional considerations which seem to necessitate an early and careful review, in order that this frequently-recurring problem, water analysis, may be placed without delay on a more satisfactory foundation.

So little progress appears to have been made in this direction that the remarks of Fresenius, pronounced in the early days of quantitative chemistry, may yet be held as strictly applicable to the present time. This distinguished analyst says:—"With respect to the principles which guide chemists in inferring in what manner the acids and bases are combined as salts in the water, it is assumed that they are united according to their relative degrees of affinity, *i.e.*, the strongest acid is assumed to be combined with the strongest base, &c.—due attention being paid, however, to the greater or less degree of solubility of the salts, since it is well known that this exercises a considerable influence upon the manifestations of the force of affinity. Thus, for instance, when lime, potassa, and sulphuric acid are found in the water after boiling, the sulphuric acid is assumed to exist by preference in combination with the lime. It cannot be denied, however, that this way of proceeding leaves much to the individual views and discretion of the analyst, and consequently that different modes of associating the several ingredients found may lead to different results for one and the same analysis. A general understanding upon this point would be very desirable, because without it the comparison of two mineral waters is beset with difficulties. As long as a general understanding of this kind is wanting, a comparison between mineral waters can only be made with the direct and immediate results of the respective analyses."

The system recommended by Fresenius may be concisely stated as follows:—To boil a measured quantity of the water for a time sufficient to precipitate the whole of the carbonates of lime and magnesia originally dissolved in the water in that form; should carbonate of iron be also present, it will be separated as peroxide in the operation of boiling. The amounts of these constituents severally determined will represent the proportion of lime, &c., in combination with carbonic acid. The lime and magnesia still remaining in solution, and the alkaline bases have then to be estimated. Lastly, the sulphuric acid, chlorine, silica, and other constituents present in smaller proportion may be determined in the original water. As already stated, the lime not precipitated by boiling should be combined with sulphuric acid; any excess of this acid being placed in union, first with potassa, then with soda; the rest of the sodium to be represented as chloride, and in a similar state of combination also that portion of the magnesium which has not been precipitated on boiling in the form of carbonate. Nitric acid may, however, sometimes aid in retaining the magnesia in a permanently soluble form.

With regard to such samples of water as contain alkali partly in the form of carbonate (readily ascertained by adopting the process referred to by Mr. Spiller, in a letter inserted last week in our columns), the lime and magnesia must in such cases be stated as carbonate; and sulphuric acid, if likewise present, all in the form of alkaline sulphate.

A mere casual reference to the numerous published tables of analyses describing the constitution of mineral waters will be sufficient to convince an unprejudiced mind of the necessity for urging the importance of this subject, and taking steps to obviate the inconvenience arising from the want of an uniform system of tabulating the analytical results.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Expansion of Metals and Alloys, by F. Crace Calvert, *Ph.D., F.R.S., F.C.S., &c.,* R. Johnson *F.C.S., and* G. Cliff Lowe.

Having been engaged for some time in investigating several of the properties of pure metals, it was thought desirable to take advantage of having pure metals at our disposal, together with a series of definite alloys of those metals, to determine their rate of expansion; and we were encouraged in pursuing this investigation by finding that several of the authors who had previously published Tables of the Expansion of Metals differed widely in their results. These discrepancies, having reference to some metals most extensively used, might, we thought, be due either to the method employed, or the fact that metals of different degrees of purity had been experimented upon. Therefore, being sure of the purity of the metals that we intended to employ, we had recourse to a method, the accuracy of which, we trust, will appear satisfactory.

Owing to the difficulty of obtaining the metals in a pure state, in large quantities, we found it necessary to employ square bars, having a length of 60 millimètres by 10 millimètres in diameter, and devised the following process to determine with accuracy the expansion of such short bars. This we believe we have effected, as our apparatus will easily indicate an expansion amounting to the 50,000th part of an inch, or about the 2000th part of a millimètre.

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[1] Chemical News, vol. iii. p. 285.

thaline into alizarine. Experience has confirmed this idea. By the following process artificial alizarine may be prepared:—

Introduce a mixture of binitronaphthaline and concentrated sulphuric acid into a large porcelain capsule, heated in a sand-bath. When the temperature is raised, binitronaphthaline dissolves completely in sulphuric acid. When the mixture reaches 200° C. throw in minute grains of zinc, and in a few instants sulphurous acid is disengaged. The operation takes about half-an-hour. Then if a drop of the acid mixture is made to fall into cold water it developes a beautiful red-violet colour, owing to the formation of alizarine. Sometimes the reaction becomes energetic if a large mass is operated upon, if there is too much zinc, and if the temperature is not carefully attended to. In such a case the sulphuric acid boils rapidly; abundant white vapours are disengaged with extraordinary noise and violence. It must be added that the latter inconvenience is easily avoided by adding only small quantities of granulated zinc, and by watching the temperature. When this accident does happen, the proportion of alizarine is greatly diminished, but still a considerable quantity remains in the residue.

The reaction over, dilute the liquid with eight or ten times its volume of water, and then boil it. In a few minutes throw the liquid on a filter. Cooling causes it to deposit alizarine, in the form of a red jelly, sometimes adhering to the vessels, sometimes suspended in the liquid. In either case this jelly appears, under the microscope, to be composed of a mass of very distinct pointed filiform crystals. The mother-waters are coloured dark red, and hold in solution large quantities of alizarine. They can be immediately used for dyeing after dilution with water and proper saturation. They contain large quantities of sulphate of ammonia. Some undissolved alizarine remains on the filter, which is easily carried off by caustic alkalies or carbonates, and precipitated anew by acids.

In the preceding reaction, zinc can be replaced by various substances; for example, tin, iron, mercury, sulphur, charcoal, &c., &c.,—in a word, by all bodies, simple or compound, organic or inorganic, which react upon and reduce sulphuric acid at a high temperature.

The two following equations describe the reaction:—

$$\underbrace{C_{20}H_6(NO_4)_2}_{\text{Binitronaphthaline.}} + 12M + 18(SO_3.HO) = \underbrace{C_{20}H_6O_6}_{\text{Alizarine.}} +$$
$$2(SO_3.NH_4O) + 12SO_3.MO + 4SO_2 + 10HO.$$

$$\underbrace{C_{20}H_6(NO_4)2}_{\text{Binitronaphthaline.}} + 10C + 14(SO_3.HO) = \underbrace{C_{20}H_6O_6}_{\text{Alizarine.}} +$$
$$2(SO_3.NH_4O) + 10CO_2 + 12SO_2 + 6HO.$$

In the first equation, it is a metal which reacts on sulphuric acid. In the second, it is the carbon itself.

The alizarine obtained by the preceding process possesses all the characteristics and reactions of ordinary alizarine. Almost insoluble in water, it dissolves in alcohol and ether. It volatilises between 215° and 240°, with a yellow vapour, yielding dark red, crystalline needles; but the colour of these crystals is rather variable. Hydrochloric and concentrated sulphuric acid will not attack it. It dissolves in caustic and carbonated alkalies, with a beautiful deep blue-purple colour; acids precipitate this solution in orange-red flakes. Like madder alizarine, it furnishes most beautifully coloured lakes. Artificial alizarine dyes like natural alizarine, and imparts the same pure tints.

The elementary analysis of madder alizarine has hitherto given variable results. Doubtless, the reason is the difficulty of freeing the natural product from impurities. The elementary analysis (which I shall make in a few days) of artificial alizarine will definitively establish the formula of this important colouring matter.

M. Dumas, while presenting the preceding paper in the name of its author, remarked, that the identity of alizarine with M. Roussin's artificial product was not thoroughly established. The elementary analysis of the latter has not yet been made. It has not as yet been possible to test certain applications to dyeing and printing processes characteristic of alizarine. It is to be hoped that the Commission nominated will decide the question promptly, the interests at stake being considerable.—*Comptes-Rendus.*

On the Detection of Bisulphide of Carbon in Coal Gas, by Dr. E. HERZOG.

A SOLUTION is prepared by saturating absolute alcohol with ammonia gas. Then a concentrated aqueous solution of acetate of lead is made, and, to insure saturation, a small portion of the solid salt is left in contact. Both these fluids must be preserved in well-stoppered bottles.

The gas to be tested may be conveniently delivered from a length of vulcanised india-rubber tubing, to the end of which is adapted a narrow glass tube, about five or six inches long. Five drops of the sugar of lead solution are then mixed in a test-tube, with about sixty or seventy drops of the alcoholic ammonia. No precipitate will be formed providing the latter solution has not been allowed to absorb any carbonic acid.

The glass tube delivering the supply of coal gas is now immersed in the mixed solution to a depth just sufficient to allow the gas to be forced out by the existing pressure, and to escape in small bubbles. In the event of bisulphide of carbon being present, the liquid becomes gradually coloured, and soon afterwards a yellowish-red precipitate is formed, which, by longer action, assumes a brownish colour. If carbonic acid existed originally in the gas, then a white precipitate is thrown down, which imparts to the yellow-red a somewhat lighter colour.

As a confirmatory experiment, the gas may be first passed through the alcohol ammonia fluid alone and the lead solution subsequently added, when an orange coloured precipitate, appearing either immediately or very shortly afterwards, will be formed if bisulphide of carbon is present. In order to meet the objection that sulphuretted hydrogen may perhaps have occasioned this reaction, let some of the gas be first passed through a small quantity of the simple lead solution. The smallest trace of sulphuretted hydrogen causes a blackening of the liquid, whereas bisulphide of carbon does not alter it in the slightest degree.

It should be mentioned that if the yellow-red precipitate be allowed to remain under the fluid, it gradually changes colour, and becomes white after the lapse of about twenty-four hours. If, however, the precipitate be filtered immediately, slightly washed, and dried, it becomes a dark brown.

With regard to the explanation of the chemical reactions which occur in this process, the interesting observations made by MM. Zeise and Debus may be quoted as proving that, by the action of sulphide of carbon on ammonia, according to the concentration and temperature of the fluids and the proportion borne by the ammonia to the sulphide, so will the relative amounts of the products of decomposition vary. In concentrated solutions, and when ammonia is in excess, sulphocarbonate of ammonium and sulphocyanide of ammonium are formed; in dilute fluids and when sulphide of carbon is in excess, xanthonate of ammonia. Therefore, by this experiment one or other product will preponderate according to circumstances, dependent upon the larger or smaller quantity of sulphide of carbon contained in the gas. In any case, compounds of lead are formed corresponding to the ammonia compounds, which possess at first an orange-red and afterwards a golden-yellow colour.

Notwithstanding the complicated nature of the chemical reactions involved in the testing of gas by this process, the author recommends its adoption on account of tho practical simplicity which, in his hands, attended the working of a great number of comparative experiments.—*Chemischen Centralblatt.*

TECHNICAL CHEMISTRY.

On the Composition of Cast Iron and Steel, by M. E. Fremy.

(Continued from page 321.)

These bodies were reduced to very fine filings, and this metallic powder freed from all foreign matter was submitted at a red heat to the action of dry hydrogen. In these three trials the filings during the whole of the experiment disengaged considerable quantities of ammonia. This experiment leaves no doubt on the subject, and proves that nitrogen, contrary to the hitherto received opinions, is a constituent of steel. Steel is not then a simple carburet, but a nitro-carburet of iron.

If I be not deceived as to the bearing of my inquiries, it appears to me that they ought to have a certain influence on the metallurgic operations relating to the manufacture of steel. Thus in the cementation of iron, all tho conditions appear to be realised which give to the metal not only carbon but also nitrogen. It is probable that the different qualities of steel depend on the duration of the cementation, and also on the relative proportions of the two elements which combine with iron. In the preparation of steel by puddling, it is equally important to determine what are the varieties of iron most likely to yield tho proportion of nitrogen required for the constitution of steel; or those which, not containing sufficient, require to have it imparted while being converted into steel.

I have spoken of a steel which has for bases carbon and nitrogen; but this compound is not the only iron alloy, the composition and properties of which are of interest to metallurgical industry. It is probable that bodies bearing some analogy to carbon or nitrogen would serve also in the production of steels. Is it not already known that granulated iron, which is harder than ordinary iron, and which in some respects is allied to steel, is principally obtained by the reduction of minerals containing phosphorus? If the combination of iron with carbon and nitrogen ought to be taken as the type of steel, it would be very curious to determine the modifications undergone by iron when carbon or nitrogen are replaced by other simple bodies. I shall devote another communication to this interesting subject, showing that there are many steels, and that they form a whole group of compounds, each of which ought to be successively examined.

It appears to me that the following conclusions may be drawn from the new facts which I have communicated to the Academy:—

1. In studying the successive or simultaneous action of nitrogen and carbon on iron, ammonia may be used advantageously to furnish nitrogen, and lighting-gas carbon; the chemical reactions thus produced by gases, yield pure compounds; they can be easily observed and regulated.

2. When iron has been submitted, not for too long a time, to the action of ammoniacal gas, it produces no crust of nitride of iron, it is simply nitrogenised, becomes then of a zinc white, partly preserving its malleability, and resembling a real alloy.

3. Iron heated in a current of lighting-gas immediately becomes carburetted and transformed into a very soft, grey graphite like, fusible cast-iron, which is suitable for the finest castings. Steel is never formed in this reaction of lighting-gas on cast-iron.

4. Steel is formed by the reaction of carbon and nitrogen on iron.

5. Pure iron, which, under the influence of lighting-gas, is transformed into very fusible cast-iron, is previously nitrogenised, loses its fusibility, and is converted into steel by the action of the gas. Fragments of the same metal have been nitrogenised for different lengths of time, and then submitted to the action of lighting-gas; those retaining a small proportion of nitrogen were very incompletely converted into steel; those, on the contrary, which were strongly nitrogenised formed a beautiful steel. It is, then, in some measure, the quantity of nitrogen contained by iron during carburation which determines the degree of acieration.

6. It does not seem to me possible to admit that cementation may be produced exclusively by a carburetted volatile body, since lighting-gas acting at red heat on iron forms only cast-iron, while by tho previous presence of nitrogen in the metal it is immediately converted into steel.

7. When iron is transformed into steel the nitrogen is not eliminated by carbon, for I have ascertained that all the steels of commerce are nitrogenised, and disengage an abundance of ammonia when submitted to tho action of dry hydrogen.

8. All these facts tend to the following conclusion, which is a condensation of my paper:—That steel is not, as it has hitherto been considered, a carburet of iron, but rather a nitro-carburetted iron. I have adopted the title of nitro-carburetted iron to explain the composition of steel because it well expresses my opinion of tho composition of this body, in which such small proportions of metalloid so completely modify the properties of iron.

The paper having been read,

M. Dumas rose and congratulated M. Fremy and the Academy on the important results which must flow from the labours of the author of the paper. The theory of the production of steel seemed henceforth determined, and it might reasonably be hoped that great practical results would ensue. Who, for instance, did not foresee—and it was for M. Fremy to follow out the demonstration—that great advantage would result from these new, methodical, regular, and certain processes, when there was occasion either to case-harden the surface or edge of certain iron implements or instruments? After having forged, filed, and finished them off in the state of iron, a current of ammoniacal and carburetted gases would convert them more or less completely into steel. The depth of the stratum of steel being regulated by the duration of this gaseous cementation with a certainty never obtainable by cementation with powders, or by the use of horn or animal matters in the empirical processes. The Academy could but congratulate M. Fremy on his present success, and on his disinterestedness in publishing his important labours.

M. Morin remarked that M. Fremy's researches explained numerous empirical receipts and processes for the cementation of steel. In most of these processes mixtures were employed containing various proportions of carbon, and of more or less nitrogenised substances, such as ammoniacal salts, horn shavings, leather cuttings, soot, &c., &c., the result being a cementation more or less deep according to the use to which the instruments are to be applied. He thought it necessary also to observe that the character of steels produced by different methods varied greatly, not only where these methods differed, but also with almost identical processes. Moreover, certain kinds of steel, and it seems particularly those obtained by puddling, after undergoing many energetic fagottings, appear to be susceptible of losing their characteristic properties of hardness and elasticity acquired by tempering, and to acquire a considerable resemblance to the most ductile irons. Lastly, the cast steels produced by the new processes of fabrication, when properly forged, possessed an elastic resistance, capable of undergoing a much greater strain than those manufactured by the ordinary methods.

Besides the foregoing speakers, several members of the Academy joined in the discussion when M. Fremy's paper had been read, and among other remarks it was stated that the composition of cast iron might be different to that of steel. M. Chevreul then made two observations, one respecting black cast iron, and the other on the composition of steels.

1. **On Black Cast Iron.**—At the end of the last century (1799) Proust observed that when treated with weak sulphuric acid, black cast iron yielded an oily matter, a portion of which was carried off by the hydrogen gas and made the tubes of the apparatus greasy, while the other portion remained mixed with the black residue, from which alcohol could extract it. I never neglect an opportunity of quoting this observation of my illustrious fellow citizen, as an example of the possibility of producing, by chemical forces, compounds analogous to those of organic nature. Experience has long since proved that aqueous vapour by reacting on charcoal, yields, besides carbonic acid, or oxide of carbon, nothing but hydrogen. and not carburetted hydrogen, as hitherto believed:[1] the combination of the cast steel with the nascent hydrogen seeming to me difficult to admit; this has led me to conjecture that in Proust's experiment the water might assist in the production of the oily matter simultaneously with the carbon and hydrogen. Now, M. Fremy's observations seem to throw a light on the subject, by indicating that it is not carbon, as was represented, which yields the oily matter.

2. **Composition of Steels.** — Independently altogether of science, two bodies possessing different properties have never been confounded; so that when an iron was observed which hardened on being suddenly cooled, it was distinguished from one preserving its original ductility after undergoing the same cooling influence. Thenceforward the name of steel was bestowed upon the first substance to distinguish it from what is properly called iron, or, in other words, between steel, which tempering hardens, and iron, which tempering does not harden. Since the time of the revival of chemistry the difference between steel and iron was attributed to the presence of about a thousandth part of carbon in the former. Later, the influence of various bodies on steel was recognised. Berthier mentions chromium; Faraday and Stodart aluminium, platinum, and its accompanying metals; but the fact which to me seems of the greatest importance, is the method by which MM. Faraday and Stodart obtained from cast iron some centièmes of iridium and osmium, which when analysed yielded no trace of carbon. Setting aside the question whether steel is an indefinite compound of iron and one or several simple bodies distributed through the whole mass of the steel, or whether it is a definite compound of iron with one or several simple bodies distributed in indefinite proportions in the iron in excess of the elements of the definite compound, I conclude, from the whole of the facts I have stated, that in a chemical treatise steel in general must be regarded not as a definite compound by the nature of its constituent parts, but as a particular state of iron produced by the union of this metal with bodies the nature of which is variable, and it is from this point of view that, after defining steel, independently of all scientific considerations, as an iron which is hardened by tempering, I discriminate in my fourteenth lesson on "Chemistry applied to Dyeing," published in 1829, p. 78:—

1. Steels formed by iron and carbon;
2. Steels formed by iron, carbon, and a third body;
3. Steels formed by iron and some other body which is not carbon; or uncarbonised steels.

The results of M. Fremy's interesting experiments are, it seems to me, easily connected with what is already known of steel if looked upon from the point of view I have explained, instead of regarding them in the general way. It is now important to find out,—1, Whether it is true, as Guyton says, that diamond dust will turn iron into steel; 2, Whether acieration can be effected without the intervention of nitrogen.

M. Fremy said, in reply to the preceding observations, he was happy to observe the interest the Academy took in his enquiries on steel, and he thanked its members for the friendly way they had spoken of his labours. It was not his intention to treat, in the present communication, of the influence exercised by nitrogen and carbon on the properties of iron, but all questions relating to the fabrication of steel and cast iron had for a long time been carefully studied in his laboratory, and the results would by-and-by yield materials for papers bearing on the following points:—

1. The relative proportions of nitrogen and carbon required to be introduced into iron to form good steel;
2. Circumstances opposing acieration or altering the qualities of a steel when formed;
3. The mode in which carbon penetrates the metallic mass;
4. The reason why such small quantities of carbon and nitrogen can convert iron into steel;
5. The study of steels containing metals such as manganese, chromium, tungsten, aluminium, &c.;
6. Classification of different kinds of cast iron; examination of the effect which silicium, phosphorus, arsenic, and sulphur exert upon these compounds; study of the cast iron best suited to the fabrication of puddled steel. —*Comptes-Rendus.*

Griffin's Reverberatory Gas Furnace, for Analytical Operations with Platinum Crucibles.

This apparatus produces a high temperature without the use of a blowing-machine. It is especially suitable for the decomposition of siliceous minerals by fusion with alkaline carbonates in platinum crucibles, being capable of fusing 1000 grains of anhydrous carbonate of soda in ten minutes.

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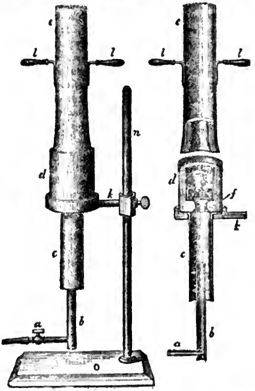
[1] "Eighth Lesson on Chemistry Applied to Dyeing," pp. 23, 24.

The furnace is represented by the annexed engraving.



It consists essentially in a combination of tubes, within which a mixture of coal-gas and air is burnt, and in which a small fire-clay or plumbago furnace holds the crucible in the point of greatest heat.

Letter *a* represents a brass tube and stop-cock of ¼ inch bore, for the supply of coal-gas at ordinary pressure. The end of it, fixed within the tube *b*, is pierced with several small holes for the emission of the gas. The brass tube *b* is 9 inches long and 1 inch bore, open at both ends. The iron tube *c* is 8 inches long and 2¼ inches diameter, open at both ends, but joined at the top to the iron body of the furnace *d*. This body is 4 inches high, and 4 inches in external diameter. The iron tube *e*, made conical at the lower end, is 15 inches long, 2¾ inches wide at the top, and 3½ inches wide at the bottom. The body of the furnace, *d*, is fitted with a lining of fire-clay or plumbago, the upper part of which is cylindrical, with a bore of 2½ inches, and the lower part forms a massive diaphragm, with a central opening of 1½ inches, as shown by *f* in the section. Upon this diaphragm is loosely placed a cast-iron ring with three knife-edges, represented in the section and separately by the figures *h* and *i*, the use of which is to support crucibles. By reversing this ring it can be made to suit crucibles of different sizes. In the lower part of the chimney, *e*, a reverberatory dome of fire-clay or of plumbago is fed. This serves to deflect the flame downwards upon the top of the crucibles. The space provided in the body of the furnace for the reception of crucibles is 2½ inches wide and 2½ inches high, and it will take in a crucible of nearly those dimensions.

The furnace is supported by a flanged iron ring, *k*, attached to a triangular socket, which can be screwed on the ⅝ inch rod of an ordinary iron retort-stand, as represented by *n*, *o*.

The gas, entering the furnace by the supply-pipe, *a*, mixes with the air, which rises through the lower end of the tube *b*, and burns with a smokeless flame when lighted at the upper end of that tube. This flame is then supplied with additional air by the lower end of the tube *c*, a great draught being produced by the action of the tall and wide chimney, *e*. When the pressure of the gas is strong, the flame reaches from the tube *b* to the top of the chimney *e*, being in that case two feet long. The point of greatest heat is at 8 or 9 inches above the upper end of the tube *b*, and the arrangement of the furnace is such as to fix the crucible in the focus of the heat. The condition of the crucible can at any time be seen on lifting the dome and chimney *e*, by means of the handles *l*, *l*.

The amount of gas supplied to the furnace is regulated by the stop-cock *a*, from which a flexible pipe should lead to a second stop-cock, attached to a fixed gas-pipe. The latter should be opened wider than is necessary to supply the furnace with gas. The pressure should then be regulated by the stop-cock *a*. This arrangement prevents, to some extent, the blowing down of the flame when the pressure of the gas in the pipe beyond the fixed stop-cock happens to be suddenly lessened.—*Pharmaceutical Journal.*

PHARMACY, TOXICOLOGY, &c.

Improvements in the Processes for the Preparation of Hydriodate of Ammonia and Veratria,[1] *by* Murray Thomson, *M.D., F.C.S., Lecturer on Chemistry, Edinburgh.*

Hydriodate of Ammonia.—There are at present two methods in use for the preparation of this salt. One of these is the same as that given in the Edinburgh Pharmacopœia for the preparation of iodide of potassium, only that the iodide of iron, which is first made, is decomposed by ammonia or carbonate of ammonia, instead of by carbonate of potass. By separating with a filter the insoluble oxide or carbonate of iron, the hydriodate of ammonia is obtained in solution, which it is now only necessary to evaporate, at not too high a temperature, so as to obtain the salt. The other method is to make first a solution of hydriodic acid, by passing sulphuretted hydrogen gas into iodine suspended in water. Hydriodic acid is thus formed, and sulphur precipitated; the latter is separated by filtration, and the acid filtrate is neutralised with ammonia or carbonate of ammonia, and then evaporated to dryness as before.

In either of these ways the salt may be made quite pure, at least so far as obtaining a solution of it is concerned. It is in the evaporation of this, so as to get the dry salt, that the difficulty lies in obtaining it quite colourless. Almost invariably, as the drying of the salt is just being finished, a slight decomposition takes place, and some of the iodine is set free, and communicates its brown colour to the whole mass. To obviate this tendency to decomposition is the purpose of my improvement, and it consists, in the first place, in taking care that the salt, during the time of its being dried, shall always contain a slight excess of ammonia; and, secondly, in reconverting the liberated iodine into hydriodic acid, by from time to time allowing a small amount of sulphuretted hydrogen gas to flow over the salt while it is being dried, an apparatus to evolve the gas being kept at hand during the operation. The effect of adding the gas is, that the brown salt almost immediately becomes white. As soon as this bleaching, so to speak, is effected, the stream of gas is withdrawn, and the drying continued, and only resumed if iodine should be liberated a second time. If the salt is well prepared at first, it seldom needs the second application. As soon as the salt is dry, it should, while warm, be put into a dry stoppered bottle, as it is deliquescent. As long as the salt is dry, it will keep a long time colourless, but if it get damp, it is partially decomposed and again gets brown, but its colourless condition may again be restored by a repetition of the above treatment.

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[1] Read before the Pharmaceutical Society of Edinburgh.

It must be admitted that an infinitesimal amount of sulphur finds its way in among the salt, by the use of the sulphuretted hydrogen gas as a decoloriser; in fact, whatever quantity of that gas is actually consumed, has its sulphur precipitated into the hydriodate. To show, however, that this is really no drawback to the use of the gas, I may mention that an exceedingly small amount of free iodine is enough to communicate a considerable depth of colour to a comparatively large amount of the hydriodate. Now, it is to yield hydrogen to this iodine that the sulphuretted hydrogen gas is employed. The greater part of the gas is, however, dissipated as such, and the quantity of sulphur, therefore, is necessarily small in quantity: in fact, it is so minute that I have not been able to detect its presence in the samples I have prepared by this process, although I employed our most delicate tests for sulphur. I beg, therefore, to recommend the above process for adoption in the preparation of hydriodate of ammonia, a salt which most will agree is far from being easily obtained in a colourless condition.

Preparation of Veratria.—In all the processes as yet published for the extraction of veratria from the cevadilla seeds, which contain it in largest amount, as those given by the London and Edinburgh Pharmacopœias, as also the one given by Wittstein (*Repertoire für der Pharmacie*), alcohol is the agent employed in the very first treatment of the seeds, so as to dissolve out the alkaloid.

I propose to substitute for the use of spirit at this stage that of boiling water acidulated with hydrochloric acid, proceeding as follows:—The cevadilla seeds are denuded of their capsules by the method recommended in the Edinburgh Pharmacopœia. Any convenient quantity of them is covered with the boiling acidulated water, which is allowed to act on the seeds for twenty-four hours. The liquor is then strained and poured out, and the residual seeds again treated with hot acidulated water as before, such treatment being repeated six or eight times. The accumulated liquors from these operations are now evaporated to about one-twentieth of their total bulk. Ammonia is now added until no more precipitate is thrown down. This precipitate of impure veratria is now separated by a filter, washed, and then dried on the water-bath. It is now reduced to powder and treated several times with hot alcohol, which will dissolve out the veratria and very little else. The use of alcohol here is not, however, essential, as the alkaloid might again be extracted by acidulated water; but on the whole it seems better to use spirit at this stage, as the veratria dissolves out comparatively free from impurities. It should also be remembered that the quantity of spirit needed to dissolve out all the veratria contained in this precipitate is far less than what would be needed to extract the same from the mass of seeds it corresponds to. The amount of alcohol used in the latter case is nearly six times that of the former.

After the spirit has dissolved out the whole of the veratria, the solution is distilled, by which much of the spirit is recovered. The half-aqueous half-spirituous residue is now evaporated, so as to expel all the spirit. Acidulated water and a moderate quantity of animal charcoal are now added, and the whole is heated and filtered. More acidulated water is added to the residue, the heating and filtering being again repeated. This should be done five or six times,—as long, in fact, as the filtrate will yield a precipitate by ammonia. Ammonia is now added to the collected filtrates, when the veratria is thrown down. It is now separated and dried. As so obtained, it has a pale straw colour. It will be obtained whiter by repeating the treatment with the acidulated water and charcoal.

By this process I have obtained at the rate of twenty grains of the alkaloid from one (avoirdupois) pound of seeds. In one or two more trials of this process, I have no doubt this amount might be somewhat increased.

I may make the following addendum on the subject of veratria, having reference to a test for it. In most chemical manuals which treat of this alkaloid, it is said that it gives with nitric acid a red or crimson colour, and with oil of vitriol also a red, which afterwards becomes yellow. In any of the samples I have either made or seen, I have not been able to get any colour at all with nitric acid, and with sulphuric acid the following is what I have observed:—When the veratria and the acid are mixed, no colour at all, or a very feeble one, is at first developed; but in the course of three or four minutes a full blood-red colour is given, and this may last for two or three hours quite unchanged, after which it gradually fades away. The only other substance which is likely to be mistaken for it is, therefore, salicine, but the points of distinction are these: with salicine, the colour given by sulphuric acid is immediate, and is not blood-red, but decidedly purple-red; and then the colour is so permanent as to last over six hours, by which time the colour of the veratria is quite gone.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, May 3, 1861.

On the Solar Eclipse of July 18, 1860, by Michael Faraday, *D.C.L., F.R.S., Fullerian Professor of Chemistry, R.I., Foreign Associate of the Academy of Sciences, Paris, &c.*

I do not wonder at your desire to be here this evening. We had the hopes of an account of the eclipse of last July from one who was the directing spirit of the investigations then made, and with every intent and desire to give us that account here, the very matter respecting the eclipse has so accumulated upon him as to prevent that taking place; and we cannot possibly be so unjust as to urge a compliance with an assent made at a distant time, when he thought he could accomplish what we desired, since he is now preparing results, and doing far higher service to science than he would be doing even here, where the purpose is to convey to your minds matter which I may, perhaps, be able, certainly not so well, but perhaps with sufficient clearness, to lay before you. Mr De la Rue was the person who guided the expedition, of which I am to be the mere mechanical agent in giving you an account.

There happened an eclipse in the month of July last year. You know very well that the three great bodies—the sun, our earth, and the moon, are rolling amongst each other; that, whereas the moon must ever cast a shadow as well as the earth, the sun being the source of light, those shadows travelling through space must be always cast somewhere, except when they coincide. When the shadows of the sun and moon coincide, we being outward from the sun, we have an eclipse of the sun; when otherwise, we have an eclipse of the moon. And it so happens that the eclipse of the 18th of July last year was a most remarkably fortunate one for the observers of Europe. It was visible on a part of the earth accessible to us, namely, Spain and part of Algeria, and at a time when the moon was nearest to us, and the sun furthest from us; and it so happened that the projected discs of those two bodies in the heavens were in that position in which the image of the moon overlapped and surpassed that of the sun; and, therefore, to those who took the centre of the line described by their shadows there was, in fact, a total eclipse of the sun. And not merely was there a total eclipse of the sun, but that total eclipse would last for the longest time that could very well happen in the juxtaposition of the three bodies—namely for three minutes and a half. The eclipse began in North America, came across the Atlantic, passed over Spain, crossed the Mediterranean into Algeria, through the northern parts of Africa, and terminated, as far as the records reach, just about the Red Sea. We have here maps showing the path of the eclipse in Spain, and in the centre of that path, for nearly sixty miles' width, so much of the sun was covered by the moon as to make the passage of the moon in that part occupy about three and a-half minutes—the partial eclipse, of course, lasting much longer.

Now, you will understand that since the time of Herschell the elder, men have thought much more philosophically about the condition of the sun and moon than they did in former times. He first gave us views respecting the constitution of the sun, gathered partly from the spots and holes occurring upon its surface, which at first almost alarmed and startled the philosophers, but which have been, as it were, only the first sowing of seeds of knowledge to produce fruit hereafter. This knowledge has been growing from year to year, from eclipse to eclipse, in a most extraordinary manner. Every effort is being made to take advantage of these rare opportunities for observing the sun physically and philosophically; for you may get a peep at the sun at these times which you can never get at any other. To observe the moon blotting out the sun from observation, and yet letting you close by the sun, so as to see the stars and planets which would be always visible in the day time, but for that wonderful flood of light which puts all others into the shade,—to get such an opportunity is most important to the philosopher, especially since the occasions are so rare that for a man who has seen one to have witnessed another, and so been able to add the experience of the first to his judgment in regard to the second, is a remarkable occurrence. That man, Airey, whom I like best to designate by his name which is his highest honour, has seen no less than three solar eclipses, and has stored up so much information and judgment respecting them as to make him the chief of the party who went forth to observe the recent phenomena. I believe philosophers in all countries send their reports to him, and they go forth to the world hereafter in a proper manner, about which as I have said, he is now engaged.

Now, what is the sun? Is it a great fire? No. Is it an electric spark? No. There are plenty of reasons for saying that the sun is not that. It is possible to raise a conjecture in the mind of the philosopher which shall give him a reasonable hope of ascertaining the nature of the sun. The more, however, we can see of the sun, the better we shall be able to judge of it. Not the more brightly we see the sun, but the more closely and distinctly; and the great use of the late expedition to Spain has been in its giving us a great deal more of that knowledge. What is the sun, then? I can show you a little sun here in the voltaic light. It is a brilliant spark, and what would it be to look at the edge of that light, to examine the condition of that part between it and darkness? You tell me, and you say truly, that if you look at the sun you cannot tell what is its form and condition; it is too brilliant and beautiful—"dark with excess of light." An eclipse is the only opportunity we have of seeing anything like what belongs to the sun,—essentially belongs to him, and keeps him in that condition which makes him essential to the whole world, to the worlds of worlds, to all the stars and planets that pass through space,—that condition which makes an essential part of his constitution, which belongs to his physical history even more than the light that shines upon us. The light has shone upon us so long, it shines upon us day by day, hour by hour, minute by minute; we could not live without it; neither plants nor animals could grow without it; we have it so constantly with us that it is regarded as one of the commonest things in the world. So we think of the sun; but to get at the truth of the matter with regard to its physical condition, we must also know that which belongs to its outside, I was going to say; we do not know how it is finished off in space; is it a hot body sustained by any action going on from day to day? What evidence have we? We must look at that when the eclipse comes on, when the sun is hidden—not by your hands, as in the case of our bright light here, but by the moon, which blots it out entirely;—then is the time for our examination, for if the least part of the light is present you have no chance of seeing anything beyond the light of the sun. The smallest particle of sunlight is far brighter than our voltaic light, and prevents your seeing anything else. It does happen now and then—it won't happen for many years to come again—that you have a complete occultation. Now, think of the philosophic spirit exhibited on the last occasion, in July. There was to be a hiding of the sun for three minutes; it was to take place in a distant country; it would be an expensive undertaking to get there, up in the mountains, or the plains; and there were the chances of the weather against any observations being made—let but a cloud come over the place for three minutes and a-half, and all the exertion would be lost;—yet men were found ready to go. Fifty philosophers started from Great Britain, and Government placed a fine ship at their disposal. Many of them left the Association at Oxford, which was then sitting, to go out to the mountains and plains, and make their observations. Fortunately, they had fair weather, and they went and succeeded. I am ashamed to say that I do not remember the names of many of those who so richly deserve our thanks for their efforts. Amongst them, however, a certain party was organised to observe the sun photographically. Perhaps you wonder that we should make any point of that. You know the wonders of photography—how it not only enables the sun to give us his own picture, but enables him to give us the pictures of other persons who are lighted up by his rays. Now, of what service is this in the case of an eclipse of the sun? Why, it can do wonders. We have tried, and we therefore know, what can be done. You look at the eclipse; you see the disc of the moon gradually advancing till the sun is perfectly obscured, and you find it is hidden for three minutes. It is only during those three minutes that you can see what you want—that interval between light and darkness which belongs to the outside of the photosphere of the sun,—and where is the mind that can remain steady enough, during that brief interval, to devote all its energies to the use of the means provided for recording the phenomena, and not be led away by those momentary splendours which, perhaps, will never be witnessed again? Who, among all those who went to Spain last July, will ever see another eclipse? I am told by astronomers—I know nothing of the subject myself—that the occurrence will not happen again during an ordinary life. What we want, then, is an eye that, in looking at the sun, shall not be blinded, as we are here in looking at our voltaic light. Providence has given to the eye such a wonderful power of adaptation, that if I look at this light for a time, the eye is so changed as to be incapable, for a moment or two, of ordinary vision. Now we want an accurate eye—which photography gives us—that can receive and record an impression of the condition of the sun when it is just hidden or of those parts which are visible,—an eye that can see and record these things, not misled by the previous brilliancy. For this is the beauty of photography: it can record what has happened, so that it can be examined afterwards. What are mind and memory in man when disturbed by these extraordinary appearances of a solar eclipse? Some of those zealous observers who went to Spain last year told me that there was not one of them who kept possession of his mind so thoroughly as not to be a little misled this way or that way by the wonders of the phenomena they witnessed; and he to whom I now principally allude, because his investigations most concern me, at one moment almost regretted that he had taken charge of the expedition under his care (which cost him personally 300*l*. or 400*l*, and required a large staff and a great deal of apparatus) because it deprived him of the pleasure of looking about and seeing the glorious sight comparatively undisturbed. I dare say that many of those who merely looked on were more happy in gazing at the beauties of the eclipse and the wonders of the landscape, than those who were engaged in special investigations which they had to record. You have, then, in photography an eye which can look at the sun, and which, looking there for a few seconds, can take upon its memory—that is its collodion—a perfect record of the sun's appearance, and keep it for ever; then, by means of successive plates, you have an eye which can take another look, and then another, and perhaps another, if the weather be propitious, and bring away all these impressions faithfully recorded for future observation. Now, I must try to impress you, if I can, with the real duty performed by this body of men to whom I have alluded. Mr. De la Rue went out with five assistants—Mr. Beckley, Mr. Downes, Mr. Beck, Mr. Reynolds, and Mr. Clark, all of whom were perfectly well educated in regard to the operations with which they were entrusted They were determined not to fail when the eventful three minutes came which would never return; and so well did they carry on their operations, under the generalship of their head, that they brought away not fewer than thirty-three photographs of the sun during the eclipse, four of them taken just about the period of totality. They narrowly escaped being burned up, for the corn around them once took fire; and they had also the chance of an accident from the wind, which blew very roughly; but they succeeded in those three minutes of utmost excitement, and procured those photographs of the sun which I am now about to show you. I have no time to tell you what the Government did in lending that fine ship, the *Himalaya*, for the occasion; what the Spanish authorities did; what Mr. Vignolles, the engineer of the railway, did; and, indeed, what they all did;—I will not take up your time in returning thanks to those who have worked so well for science. You may say, it was, no doubt, an easy thing for the sun to represent his own face in a photograph, since he can give us the faces of others; but there is this peculiarity about the sun, that over action destroys everything. You see here a representation of the sun with certain dark spots, which can be very well obtained by using the proper means; but leave the picture a little too long, and out go the spots; the matter is over-acted upon, all peculiarity disappears, and nothing but a dull appearance remains behind. It required a special instrument, a remarkably beautiful instrument, which has been constructed for some years, and placed at Kew, called the photoheliograph. This was placed in Mr. De la Rue's hands, with a sum of money by no means sufficient for the purpose, to enable him to carry out his operations The first view I shall show you will be a picture of these men themselves in a group. You will imagine that they have left England and arrived in Spain, landed at Bilboa, and gone over the mountains. Mr. Vignolles has conveyed two tons of apparatus to the spot, and they are all now upon the ground. You see the photographic house, and the instrument, which is like a mis-shapen telescope pointing up to the sky, being balanced upon its axis by a weight with a proper clock-work arrangement. As soon as the sun's rays enter the instrument, they pass through the glasses which the optician puts there, and are afterwards expanded by means of other arrangements until they fall upon a collodion plate below, of 4½ inches diameter. Upon that the sun has to make its own face; but let the sun look there but a moment, whilst you count one, and the face is spoiled. In order to get a proper impression, you require a time so short as scarcely to be measured except by micrometrical means. The only way was to take a certain opaque screen, and cut a small slit in it, and then, by a proper arrangement, to draw the slit as fast as possible across the bundle of rays passing at that spot; and that—I do not say second, but that instant, was sufficient to secure a full and perfect impression. This is the way in which pictures are taken by the photoheliograph. You have now a view of the *Himalaya* on its voyage outward, for you are fairly entitled to see how they proceeded from the commencement. Another view gives you a representation of one of the waggons in which luggage had to be carried over the country. Mr. De la Rue has set himself down upon a threshing floor,—a hard place where the corn is spread out, and the oxen wander to and fro treading it under their feet. You see also the chamber which had to be taken out and built. Here also is the telescope and the apparatus. Mr. De la Rue will now give us a view of the sun itself, with one of its spots. A second view shows us the same spot but in a different position, owing to the revolution that has taken place. In this way we are able to compare the history of these spots from time to time.

A few views will now show us the progress of the eclipse. The first shows the sun to be partially obscured, but the spots are visible. The next shows the moon's disc advancing. And the third exhibits an almost total obscuration, leaving only a crescent of light. The next view is taken at the period of totality, when there is an entire extinction of the sun by the moon. And now you see all these beautiful phenomena, these luminous appearances which have been so accurately photographed. You must not suppose that all these pictures precisely correspond in luminosity to the brilliancy of the objects represented. Though the negative may give an exact representation, the different impressions from the same negative will vary, and present some irregularities owing to the peculiar state of the atmosphere at the time. Clouds, dews, rain, all these different phenomena operate as a disturbing influence, and hence the irregularities in some of the impressions. However, we are obliged to compound for many things, and accept them as we can get them. But vain are all regrets. The services rendered by photography are still remarkable. I do not ask Mr. De la Rue what he remembers, what he observed, of the eclipse; I ask him what he has recorded and brought away. Two of us may differ in opinion as to what took place, but there is the fact, the record made at the time, to appeal to—the enduring representation of the exact state of the sun at that time. While Mr. De la Rue was directing the photographic operations, attending to the time, giving as much as a minute to the first photograph and about twenty seconds for the second one, he was looking through his telescope at the sun, either with or without shade, as the eclipse advanced, and making drawings by hand. I have some of them here, and they tally perfectly with the images and representations given by the photographs. In order to give you an idea of what was seen around the sun during the eclipse, I must refer you to our diagrams. There you perceive what are called red prominences—one never does get good names for these things—certain red appearances not of an ordinary kind, which are seen upon the sun when it is blotted out by the moon. You see them in different positions according as the moon advances. You observe one prominence, or rather independent existence, floating above the sun, touching neither the sun nor moon. You would wonder if I could tell you how these things have been recorded—these great prominences about the body of the sun. Here is one 28,000 miles in length. Now we know that the body of the sun is exceedingly light, far lighter than the matter of the earth. What, then, can this be which is floating above it, a cloud in the air, with the most luminous part towards the sun, whilst the other bodies had their luminous parts away from the sun? That one prominence is, I think, 28,000 miles in length—for these things can be measured and have been measured—and 74,000 miles above the sun. Now what is 74,000 miles? It is three times the circumference of the earth. Such is the height of that cloud, as far as we know it. It may be much farther in physical existence for what we can tell. Perhaps we may know hereafter. At present we are only taking the first steps. We are infants learning to walk with regard to this part of the sun. No one can imagine that what we are doing here is anything else than what will appear as absurd speculations and crude conjectures when we make further discoveries in the mode of observing these things. There has been no eclipse in which the mode of observation has not advanced on that of its predecessor in the most wonderful manner, and we shall still go on advancing. We now give you views of the eclipse in its various stages; and here we have a view of that beautiful appearance the corona, which I knew would call forth your admiration. This differs in character at different times, sometimes appearing like superposed discs, and at other times having this glorious stellar character streaming out behind the moon, the moon appearing like a brazen globe before it. This is not a photographic corona which you have before you, but it is an accurate drawing brought away from the spot.

The most important question to be solved by these observations was, what are these prominences? And the first part of that question is, what do they belong to? Are they solar? Or are they lunar? Or do they depend upon the sun, moon, and earth? Where do they exist, and what are they? One of the chief results, perhaps, of Mr. Downes's observations is, that they belong to the sun. Consider what is taking place. There is the disc of the moon moving over the disc of the sun, and it is looked at by persons who choose a position where they may get into the centre line of the projection. Amongst other prominences there was one, here represented in the diagram, which, I believe, appeared before the sun was entirely out of sight. Mr. Airey and several others saw it before the sun was completely obscured. Now, that is not a prominence which is first of all brought into sight by the shading away of the sun, and then cut off from the sight by the moon sliding over it; but it is a prominence which, when it comes into sight, goes sailing along upon the edge of the moon, and may remain in sight some time. There is another of the same kind, projecting above the moon at every part, and not disappearing at all from us by any occultation—only disappearing when the brilliant light of the sun, arising from the uncovering of his face, comes into view and extinguishes all other things. But then there is this other question—what do these prominences tell with regard to their place upon the sun or the moon? One or the other or both? What is their story? Their story, as I understand it, is that they are upon the sun, and for this simple reason. Very great care was taken by the astronomers to measure off the places of these appearances; they are all marked off in a beautiful manner, so that they can identify the different prominences when they see each others drawings, whether they stand in the line of the eclipse accurately, or a little to the right or the left—they all correspond in the position of the prominences. The sun, then, being put into the telescope, and motion given to it, so that it remains fixed in the telescope, the moon appears to pass over the sun in a given direction, and while it covers up some prominences it uncovers others, and slides between others; so that they would appear to belong to the sun and not to the moon. That is a question which seems to be entirely solved by these observations. Here is a prominence in the diagram; if it belonged to the moon, it would have moved with the moon; but belonging to the sun, it is left behind. A careful comparison of the centre of the moon and the centre of the sun, with the places where these prominences appeared, declares that they belong to the sun. That is a most essential and important point to have ascertained with certainty. No sound philosopher can run away and say that these things belong to one body when they belong to another. The great thing is to proceed cautiously, and draw our conclusions with care, lest we slip and fall. We must go on step by step, and be certain as we proceed. I have said that some of these objects are 74,000 miles distant from the sun. One or two of them appear like detached clouds, the time is too short to observe any change in their shape, but they are of different shapes and appearances in different eclipses. Of course you do not suppose that the profile of the sun directed towards us at the time of the eclipse is the only one that has these protuberances upon it. Surely the sun revolves, as the spots prove to us, and if another profile was presented to us we should have other prominences. We cannot doubt that these things which we see exist all over the face of the sun. We know that the moon is a round body because every profile she gives us is round. So it is with the sun; whatever profile we have, these prominences come into view, and we may therefore conclude that they exist all over the surface. It is a remarkable circumstance that a few days after the protuberances were seen there came into view a set of spots upon the face of the sun which corresponded in position according to the revolution of the sun, with the places where the prominences were observed during the eclipse. That is a suggestion that one catches hold of, with a view of verifying at a future time, and putting upon record with the other data which are given.

Another question has been whether the light that came from these prominences or from the corona was polarised. Now it is not my intention to take you into the question of the polarisation of light, but it is an important indication of the probable nature of these substances if the light from the corona or the protuberances is found to be polarized. The light which comes direct from the sun is not polarized, but that which is reflected is polarized; therefore there is an indication of something that sends that light to us in a modified form. Many questions of that sort will come before us in due form as the investigations proceed. Then with regard to its actinic character. We have before us the voltaic light, and we have so arranged it that it shall come through the lens, then through the prism, and then go away to the screen, and there you will see it, decomposed, according to Newton's discovery, into what he called the spectrum—that is, the rays separated. These rays have different powers, and here you begin to see some of the wonderful circumstances connected with the eclipse. You see that the different rays have different characters by their various colours on the screen. These rays write photographically upon proper media (we are now dealing photographically with the sun), and they write not in proportion to their brightness as we see them, not as they write upon the nerve of the human eye. And here comes a most important part of the philosophy of this subject. You see on the screen how far the rays are visible to you; but then we know that, chemically speaking and photographically speaking, beyond what you can see there are rays which are far more important to us, in many respects, than those which are apparent to the eye. To show this Mr. Downes will take a photographic view of the ray of light which you see going on to the screen, by intercepting it with a prepared plate. As soon as the plate is put in you see how far the rays are visible—the red, green, violet, &c., as you saw them on the screen, and we will mark the limits to which they visibly extend. If those were all the rays, you would of course find that the picture produced on the plate, when developed, would be limited to that space. Now the picture has been developed, and you observe that it extends beyond the limits of what was previously visible, and the outer rays have told chiefly in the production of the picture. They write their image far more beautifully by photography than they do upon the eye itself. These outer rays which have formed the darker part of the picture, where on the screen we could see nothing, are those magnificent and wonderful rays which make plants grow green, and make animals thrive and become cheerful. We have here a picture marked at the places where the red, and the green, and the blue rays fell, and you see these are not the places that exhibit the actinic power. In fact, the red ray has made no impression; the green has made but little; it is at the outer part, beyond the blue, where the most abundant indication of these beautiful actinic rays is apparent. We should make no advance unless we had men to make these observations, to think about them, and consent to have no answer to give to many questions which you may put to them.

My remarks may have suggested a score or a hundred questions to your minds—what is this? and what is that? Well, we must be content to confess our ignorance; we must be satisfied with getting only one point in advance, which will be a point of attachment for further progress. I had intended to say something about the spots in the sun, but our time is nearly expired. We have several drawings here showing a certain condition of the sun, which is only visible now and then, and which appears differently to different eyes. According to Mr. Nasmyth the whole photosphere of the sun is covered with images such as you have here represented, and he calls them willow leaves, a term as little expressive as possible of any meaning, so that you may get nothing but the simple observation. You perceive from our drawing that a spot in the sun is not a mere uniformly black thing. It is very dark in the centre, but gets lighter as you approach the circumference, giving an idea of different coats of atmosphere till you come to the outer one, which is the most luminous. These are the appearances which present themselves under circumstances the most favourable for observations. It happens, however, that certain conditions of the atmosphere give rise to the appearance of what are called sun-boils. This is a most serious evil in regard to taking the sun photographically, the quivering of the atmosphere destroying the character of the rays, and thus interfering with accuracy of representation. It is, perhaps, not more than two or three times in the year that true observations can be taken. The spot represented in the drawing was observed and taken during seven or eight days in which time it had been going on revolving on its own axis, and not going with the sun itself. What a wonderful thing that a spot of that kind, many thousand miles in diameter, can be going on as a great continent! There are many other things about the sun equally wonderful, which men have learned to think of and imagine. Imagine! No, it is something better than imagination. The poet never could have conjured up in his mind what we now soberly speak of as making part of the sun's constitution and existence. It wanted reason to give us thoughts of this kind.

But our time is gone. Before you leave I want to show you a photograph of the moon; for although I have been speaking of an eclipse of the sun you are still to understand that photography has advanced so far as to be able to give us representations of our own satellite. We cannot show you the whole of the moon at once; but we can show you a picture of the moon as taken by Mr. De la Rue, not by his hand, but by his mind, by means of photography; and if you apply the stereoscope you will there see that glorious body—for glorious she is, though by no means to be compared to the sun—as a perfect globe floating beautifully in space. You see here the most beautiful contrasts of light and shade, such as no ordinary picture could give and such as no imagination could supply. Here, I think, we must end our discourse. I hope you will accept what I have done to the best of my ability: and I am sure that we shall unanimously join in thanking Mr. De la Rue for all that he has accomplished.

SOCIETY OF ARTS.

Wednesday, May 1, 1861.

Peter Graham, *Esq., Member of Council, in the Chair.*

Mr. Julius Dahlke read a short, but on the whole, effective paper, at this Meeting of the Society, on "*Filtration and Filtering Media,*" of which we propose to sketch the more important points.

As we all know, filters, in some form or other, have been employed from a very early period, but the historical part of Mr. Dahlke's paper is very imperfect, and does not present a good view of the progress of the art of filtration, as it should have done, to carry out its title properly. Thus, while a casual mention was made by the Lecturer of the porous stone and earthenware filters of the ancient Egyptians and Chinese, the early Roman arrangements for this purpose were not even named, nor were the more recent processes of Peacock, Paul, Rommerhausen, Thom, Leloge, Sterling, Beart, Murray, &c., referred to in any way. As regards the scientific aspects of filtration also, Mr. Dahlke is equally deficient, but as other parts of his paper are not without merit, we proceed to quote the same.

"During the past seventy years, gravel, sand, and charcoal, used as a mixture, have been the agents most in vogue amongst filter makers, and it is only lately that due attention has been paid to charcoal as the most efficient filtering medium. Its use is much more frequent now, because not only has it a powerful detergent effect, but it possesses also the peculiar advantage of not becoming foul, while it protects from decomposition other bodies in contact with it.

"It has been often asked why animal charcoal is so effective as a filtering medium? Some attribute this to the presence of so much carbon; but that this is an insufficient reason, is shown by the fact that, although coke contains more carbon than sand, yet it is not superior as a filtering agent.

"Animal charcoal filters about three and a-half times more rapidly than either coke or sand, while it is also greatly superior in this, that it removes many inorganic impurities held in solution, over which the former substances exert no power.

"It appears that the more porosity a filtering medium possesses in itself, the more rapidly does it filter, and the greater is the effect it produces on the water. The latter will be still more decided when, with a greater porosity, peculiar substances are combined.

"This leads me to believe that we may attribute the extraordinary filtering quality of animal charcoal to the fact that its principal component parts are lime and carbon, so combined as to secure a wonderfully fine porosity. Vegetable charcoal, although very porous, and containing far more carbon, has less effect on water.

"I have observed that another substance, of which I shall presently speak, and which (although of an entirely different origin) possesses great similarity in this respect, may in many cases be successfully substituted for animal charcoal. Indeed, there are doubtless numerous substances and compounds which may be used with as great effect. Do we not see that Nature supplies tho most beautiful waters from limestone beds? It is hardly necessary to say, could we but imitate her action, that we should be able to do more in this as well as in other things, but we must content ourselves with as much success as our defective knowledge of her laws will permit us.

"Although we know of powerful agents for the removal of different impurities from water, circumstances may and do interpose which render it extremely difficult to obtain the medium in the requsite form for our purpose, and there is nothing yet discovered which will perfectly meet all the requirements of the case. Those who assert that it is possible to construct an apparatus to act as a universal filter for purifying any kind of water effectively whatever may be the impurities, remind me of the vendors of certain patent medicines, who vaunt their nostrums as capable of curing every disease. Their claims are about equally trustworthy.

"I should classify the art of filtration into three systems, viz.:—1st, where the action takes place simply on the surface of the filtering medium; 2nd, where the whole bulk of the filtering medium is calculated to operate on the water, and the detergent effect in its most delicate form may be produced; and 3rd, where both of these systems are conjointly employed.

"The first system requires a filtering medium of such a fine porosity that its pores must be smaller than the minute particles composing the impurities suspended in the water. Such an agent of course must sooner become clogged, than a filtering medium of coarser porosity, and which is meant to act with its whole bulk on the water. But both systems employed together may prove to be useful in several instances, as in the case of domestic filters. The greatest failing of these is that they must become clogged, and the more they are liable to this, tho more effectively they act. We often hear of self-cleansing domestic filters, but the fact is that no invention of the kind has been made yet, without involving complications too great for the purposes of ordinary domestic use.

"However, it is not difficult to make a filter for general domestic purposes—although the effective self-cleansing of such an apparatus is still a problem to be solved.

"If the filtering medium employed in this case be solid, and of a fine porosity in its upper part, the clogging impurities will not only be retained on the surface, but may be easily removed by scraping; and then, if the lower part of the filtering medium be prepared of a material capable of producing a detergent effect, it will act the more readily through not being interfered with by the rougher and clogging impurities.

"It should be remembered, too, that in most cases we have here only to deal with some rougher impurities which have found their way into the water on its passage from water works, or other source, to the tap of the consumer.

"Being deeply interested in the subject of filtration, I have never omitted an opportunity of carefully inspecting those house-cisterns which came under my observation; I have, however, seen but few to which the attention necessary to secure the due cleansing had been paid. Most of them were loaded with mud, and in some of them I actually noticed the growth of vegetation (fungi[1]). I conclude, from my observations, that hardly one-fourth of the house-cisterns in London are in such a condition as to afford the consumer a supply of wholesome water like that which flows from the main.

"The difficulty, or I may say the impossibility, of keeping water which is stored in cisterns entirely free from accidental contamination, should lead us to provide a domestic filter capable of removing chemical impurities, as, for example, any lead which may be held in solution; in fact, the practice of filtering water preserved in cisterns and intended for domestic use cannot be too warmly recommended.

"To remove lead from water, Professor Faraday recommends the practice of stirring up animal charcoal with the water so contaminated, the same being then allowed to settle. I have found, however, that, by using this material in a manner to be described hereafter, I never failed in producing the same effect by means of filtration.

"It is easy enough to purify small quantities of water, but the greater the quantity the greater are the difficulties of purification, especially when a certain chemical effect has to be produced.

"It will not be necessary for me to dwell upon the filtering processes required for large waterworks, as the supply is generally taken from such sources that the common sand filter-bed answers tho purpose; and where tho water is too hard for domestic uses, the beautiful process of Dr. Clark will meet and remedy the evil.

"Experience shows that it is not prudent to adopt the same means of purification for every kind of water, and I should make a difference in the treatment of the water used for domestic and that employed for manufacturing purposes. In the latter it will be often of the greatest importance to have the water as puro as possible, whereas certain so-called impurities in water may not be at all injurious to health. When we consider that no one would call human blood impure which contained 420 grains of saline matter per gallon, I do not know that wo are justified (of course, speaking in relation to health) in calling water impure which contains small quantities of certain saline matters, particularly when we have no medical evidence that the small portions of them drunk in such water ever did any harm. Besides which, it should be remarked that the quantity of lime and magnesian salts drunk in water must be greatly exceeded in amount by that which enters the system in the food."

Mr. Dahlke had better have omitted this last paragraph, his knowledge of our language not being sufficient to show him the true signification of the words "pure" and "impure." There are a good many substances essential to the legitimate composition of our bodies that cannot be considered other than "impurities," if present in the water we drink; there is, moreover, abundance of medical and social evidence that large quantities of lime and saline matters in drinking wator are productive of decidedly injurious effects, especially upon the skin, kidneys, and digestive organs.

Mr. Dahlke next said that "too pure water is distasteful, and unfit for drinking purposes," citing an illustrative case of some flat-tasting, remarkably pure water, which he had rendered more palatable by adding to it "some finely-dissolved organic impurities," and then filtering through animal charcoal!

Our author prefers animal charcoal to all other filtering media, and regrets that no practical method is yet known of moulding it into blocks without diminishing its powers. Charcoal, he said, as regards its filtering qualities, stands to coke as 15 to 4, and all attempts at solidifying it by calcination with pitch, tar, &c., have failed in practice, owing to the glazing effects of the bitumen, which greatly impairs its action.

Mr. Dahlke's filter wo shall shortly describe, as it appears to possess certain advantages over others of the same class that render it well worthy of notice; after various trials he found that the residue, after distillation, of the well-known Torbane-hill mineral, with a small addition of fine clay, will, if saturated with fatty or oily matter,[2] and calcined, furnish a very powerful filtering medium, capable of reducing the hardness of water, and removing its colour and odour. He also adds bone-dust both to improve the quality of the "filter-blocks," and in order to regulate their degree of porosity with greater precision. We must now, however, permit Mr. Dahlke to describe his own filter more particularly:—

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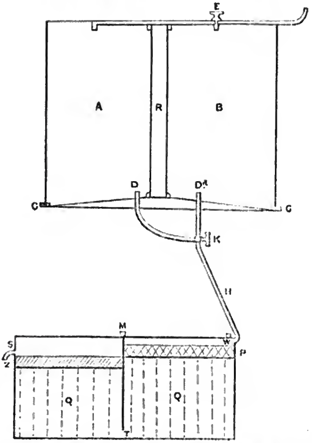
[1] Query—*Algæ* or *Confervæ*?

"Supposing 5000 gallons of water are daily required to be purified, that the water supplied contains nine grains of organic matter per gallon, has a bad smell, and is of seventeen degrees of hardness, I should employ an apparatus of which a woodcut is here given.



"The upper part is a tank made from slate, completely closed, and is divided into two compartments by means of a solid filter block with one inlet, and one outlet for each of them. The supply-pipe is provided with a three-way cock, E, which allows the necessary arrangement to be made for admitting the water to one compartment of the tank, and for causing it to pass into the other through the filter-block. If, for instance, it enters at A, it has to pass through R into B, flow off by DJ, and through K and H into the second apparatus.

"This tank, being only intended to free the water from the rougher and clogging impurities, its action is to be reversed as often as may appear necessary in order to remove the collected sediment. This is easily done by shutting off the supply-pipe for A, and opening that for B; but before the water is allowed to enter the connecting pipe of the second apparatus, the accumulated impurities must be removed through the opening of the corresponding plug C, and, that it may flow off easily, the bottoms of the two compartments are inclined towards the plugs.

"The two walls of the tank which face the filtering medium, are so fixed, that they can be opened from time to time, in order that the surface of the filter may be scoured, it being of such a consistency that its surface can be easily abraded by rubbing with hard stone.

"The water is thus freed from its coarser organic impurities, so far that they cannot possibly clog the second apparatus, which is thus retained almost entirely for chemical action on the water. It consists of a vessel which is also completely closed, with one inlet (W), and one outlet (S), divided from M to T, and fitted in the following manner:—P is a solid filter-block, cemented into the apparatus and covering the whole surface of the body Q, thus forming the top part of this filter; it is of much finer porosity than the filtering-block in the tank, in order to separate the finer particles of organic matter from the water. The space Q is filled up with coarse granular charcoal and a preparation of the Torbane-hill mineral spoken of, intended to operate upon the water so as to remove those matters held in solution, which impart colour, smell, and hardness. Z is a solid filter-plate, of a more neutral character, also cemented into the vessel to hold together with P, the packing of the apparatus.

"The reason why the two materials which fill Q are employed in a rather coarse grain, is, because the oxidation of several of the impurities taking place during the filtering process, will produce considerable quantities of gases which would soon accumulate in a finer medium and interfere with the rate of filtration, wherefore I rather prefer employing the mixture in a thicker layer.

"The dimensions of the apparatus, and the different filtering media employed in this case, will be 3 feet square by 4 inches in thickness for the filter-block in the tank; each compartment of Apparatus No. II. to be 18 inches high by 2 feet square (internal measure); its fittings are: the plate P, 3 inches thick, Q, 2 feet, and Z, 3 inches.

"The first clogging in Apparatus No. II. will take place on the surface of P, which can be easily avoided by removing the corresponding cover of the vessel, and rubbing the surface of P with a piece of hard stone, to which it will slowly yield, and thus be easily freed from its clogging matters, which cannot penetrate into it, the porosity of the filter-cake being too fine.

"The filter-block in the tank is so fixed that it can be taken out and replaced without much inconvenience, should it become worn out.

"The apparatus thus arranged is calculated for not less than 12 feet pressure, and its effect on such water as we supposed to have for illustration will be as follows:—Organic impurities reduced to about half-a-grain per gallon, no trace of smell or colour remains, and its hardness is reduced to about 7 or 8 degrees.

"With regard to the continuance of the chemical effect, I admit that this must have its limits; however, from the fact that the rougher clogging impurities are so easily removed from the apparatus, and that the quantity of the inorganic impurities which will be absorbed must necessarily be comparatively small, a good portion of them escaping in the form of gas, I do not hesitate to assert that it can be successfully employed, for a considerable time, before the filtering agents get exhausted, and repacking is required.

"I do not believe it would be possible to work such a quantity of water so effectively and lastingly in such a small apparatus, if the whole of the filtering medium were to be used in a loose state, as its porosity in the latter case could not be condensed to the state of fineness required.

"This apparatus being only meant to serve for the special purpose I spoke of, it is obvious that it will have to be altered according to circumstances, both as regards the filtering agents to be employed, and its mechanical arrangements.

"If the quantity of water to be filtered be so great that a very large filter-bed is required, I prefer employing the preparation of the Torbane-hill mineral, as described, in a granular state, rather than sand; for this reason, that it filters more than three times as quick, and is five times as light as the latter; consequently a ton of it will, by a layer of equal thickness, filter about sixteen times the quantity of water that a ton of sand would filter, with the advantage that the filtering would produce at the same time a greater decolorising effect and a considerable softening of the water. A clogging from the precipitation of chalk is not likely to take place, as this substance is separated in a crystalline and granular state. Moreover, those particles of the material which become saturated with organic impurities may, through calcination, regain the greater part of their former efficiency."

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[2] We would suggest blood, or milk, as more likely to answer the purpose.—Ed.

Omitting the concluding paragraph of the Paper, which is unimportant, we have now to present to our readers the "cream" of the discussion which followed.

Mr. Spencer rose first, to deliver a speech, of which the chief object seemed to be, to lay claim to Mr. Dahlke's invention or application entirely. He had broken open one of the filters in question, and found the filtering medium contained about ten per cent. of the magnetic oxide of iron, to which, he said, all its virtues were due. He had determined (quantitatively?) the presence of this substance by means of the magnet, and accused the Author of the paper of infringing his (Mr. Spencer's) patent.

Mr. Atkins set forth the merits of "carbon" *per se*, as a filtering agent, of which he had manufactured some 40,000 or 50,000 blocks, and proceeded to praise the Moulded Carbon Company and their manufactures in high terms.

Dr. Waller Lewis bore testimony to the efficacy of the filter supplied by Mr. Dahlke to a very large public department, as to its powers of deodorising and decolorising water. It was new to him to hear that too pure water was distasteful; and he was afraid Mr. Dahlke was assuming an untenable position. He thought inhabitants of towns were no judges of water, as their palates were disorganised by the impure water supplied to them. He had tasted the water of Lake Bala in North Wales, containing only 1½ grains of mineral matter per gallon. Many persons did not like it at first, if they had been accustomed to London water.

Mr. Painter dwelt upon the good qualities of a mixture of silica and carbon, and of the balls of the Moulded Carbon Company. These balls were cleaned by blowing air through them—a superior plan to that of Mr. Dahlke, he thought.

Mr. Danchell preferred animal charcoal to anything else, and had abandoned all other filtering media.

Mr. Wentworth Scott said he had been called upon to make various experiments upon the relative merits of animal charcoal and prepared carbons of several kinds, as regards their power of absorbing organic matter, which resulted in favour of the first-named substance. The Moulded Carbon Company's balls were subject to the defect of having in many cases their central cavity greatly on one side, thereby modifying their action injuriously. He recommended the *platinising* of the balls—a simple process—for waters containing much organic matter.

Mr. Morgan remarked that the public were the best judges of filters in the end. He regretted that scientific discussions were so frequently sullied by personalities, and commented severely, but justly, upon Mr. Spencer's speech.

After a brief reply from Mr. Dahlke, a vote of thanks was passed to him, on the motion of the Chairman, for his Paper, and the meeting then adjourned.

NOTICES OF BOOKS.

Elements of Agricultural Chemistry. By Thomas Anderson, M.D., F.R.S.E., F.C.S., Professor of Chemistry in the University of Glasgow. Edinburgh: A. and C. Black. 1861.

In many respects this is an admirable manual for the attentive study of the farmer. It contains a full and fair account of the relation of chemistry to agriculture, telling us what advantages the latter may reasonably expect from the former, and what it may not. The reader is gradually introduced, by easy stages, to such technicalities as are necessarily involved in the subject,—for the Author writes for those who are unacquainted even with the elementary facts of chemical science.

The book opens with an account of the constituents, organic and inorganic, of plants. The proximate vegetable and animal constituents, and the processes whereby these are originated by organic action on the soil and air, are described. The chapter on the General Principles of Manuring is crowded with facts, accurately but cautiously given. The distinctions made between *special* and *general* manures are remarkably sound. The following are the titles of the remaining chapters of the volume:—The Composition and Properties of Farm-yard and Liquid Manures—of Vegetable Manures—of Animal Manures—and of Mineral Manures—The Valuation of Manures—The Rotation of Crops—and the Feeding of Farm-Stock. Almost every page gives evidence of Dr. Anderson's large experience as a practical agricultural chemist. His views on the various controversies connected with this important branch of practical science are temperately expressed, and supported by a large number of well-contrived experiments.

The book, for the most part, is clearly written. Occasionally, however, we meet with ungrammatical or awkward constructions in it, which detract from its general literary excellence.

CORRESPONDENCE.

Dead Black for Metal Work.

To the Editor of the Chemical News.

Sir,—You would be doing a good many Riflemen a service if you would inform us of the best method of staining the sights of our rifles black. These sights are black when new, but after a little wear become bright and it is then difficult when the sun shines upon them to take so correct an aim. Some persons rub a little Prussian blue on them, but this is a dirty plan, and rubs off on our hands and clothes. Some method of giving them a permanent dead black stain would be the thing.

I am, &c.
A Rifleman.

Chemical Notices from Foreign Sources.

I. MINERAL CHEMISTRY.

Transformation of Nitrates into Ulmates.—M. Paul Thénard put some solution of glucose and some nitrate of baryta into a tube and heated them to 200°. He obtained thereby a colourless liquid and a white powder, which, when exposed to the air, browned and blackened or took the colour of the ulmic compounds. This seemed in contradiction to the already known fact that in the soil ulmates are converted into nitrates. But a further investigation has convinced M. Thénard that both processes go on simultaneously in the earth. That on the surface of the ground ulmates are converted into nitrates, and in the subsoil nitrates are changed into ulmates. Iron, according to the author, appears to be the carrier of the oxygen, which effects the transformation.

Comparative Action of Nitrates and Ammonia on Vegetation.—We may here quote the results of some experiments by Ville (*Comptes-Rendus*, t. li. p. 874), which go to prove that nitrate of potash has a much greater influence on the growth of plants than ammonia—that in fact the nitrogen of nitrates is much more assimilable than the nitrogen of ammonia.

its power of neutralising this extraneous acid, and in consequence restoring the natural colour of the litmus.

In carrying out some experiments, a short time since, for the purpose of observing critically the reactions afforded by the carbonates of lime, baryta, and magnesia, when dissolved in water impregnated with carbonic acid, the distinctions between the metals of the alkali group and those just now enumerated, appeared very decisive. When carefully prepared turmeric paper was the test employed, it was found that whilst very small proportions of the alkaline carbonates were readily detected, no direct action was apparent with pure solutions of the carbonates of the alkaline earths. When reddened litmus paper was the test used, it was found to be equally affected (restored to blue) by all the carbonates indiscriminately.

With this distinction clearly in view, I have frequently employed turmeric paper as a test for the presence of alkaline carbonates in natural waters. Many samples have not afforded the slightest indication. Artesian well-waters from different parts of London have all furnished direct proof of the existence of an alkaline carbonate among their soluble constituents. The sample of water from the coal strata under Bradford Moor, described by Mr. Abel,[1] possessed in a high degree this alkaline property, evidenced by turmeric, and easily accounted for by its containing no less than 43·5 grains of bicarbonate of soda in the gallon of water.—I am, &c.

JOHN SPILLER.

Woolwich, May 13.

MISCELLANEOUS.

Conversazione at the Society of Arts.—The Members of the Society of Arts held their first Conversazione this Session at the Society's House, Adelphi, on the evening of May 4th. A number of interesting objects were exhibited, including the spectrum apparatus of Bunsen and Kirchhoff, as manufactured by Mr. Ladd, Wenham's binocular microscope, by Messrs. Smith, Beck, and Beck, Messrs. Powell and Eland, and Mr. Baker; Abel's new mine or cannon fuses, some specimens of gutta-percha and varnishes, Indian "talc" (Mica), paintings, &c., &c. The "Exhibition of Recent Inventions" was also thrown open, and in the great room a miscellaneous series of transparent photographs were projected on to a large screen by means of the oxy-hydrogen light, by Mr. C. Jones. The scenes in Egypt and Italy were specially interesting, and the latter included a good portrait of Garibaldi (described as a "party" everyone would recognise) entirely spoilt by the colouring. The evening passed off in a manner which must have proved very satisfactory both to the Council, and to the large assemblage which responded to their invitation.

The Leicester Sewage Works.—With regard to the cost of deodorising the sewer water at Leicester, as the facts connected therewith have been occasionally misapprehended or misrepresented, the following information may not be considered misplaced in a report of this nature. The works erected there by a private company having failed to yield any remuneration, owing to the fact that the great dilution of sewage rendered the proportion of fertilising matter precipitated therefrom too small to be of appreciable value, they were (according to a stipulation in the original agreement) handed over to the Town Council of the borough, who have been in possession of, and have carried them on for above twelve months. It is not generally understood that these works consist of two parts. First, of the pumping engines employed for the purpose of lifting the water at the artificial outfall of the main sewer, which was made considerably lower than the natural one, which latter was at too high a level to allow of the complete drainage of the lower portions of the borough, and recourse was had to this plan of an artificial outfall, because it was cheaper than the construction of a natural outfall sewer would have been, which (owing to the nature of the locality) it would have been necessary to carry many miles below the borough, in order to reach a level low enough to create a sufficiently rapid current in the main sewers, but this plan was not finally determined upon without a thorough investigation of its merits, and the result has justified its adoption. The second part consists of the deodorising tanks and machinery. The actual cost of the disinfecting process carried on at these works during the twelve months ending November, 1859, was as follows:—The quantity of sewer water deodorised being 1,061,757,200 gallons, or 4,740,000 tons, equal to 2,908,920 gallons per twenty-four hours, or 41¼ gallons per diem per head of the population. The quantity of slacked chalk lime used was 580 tons, which cost £369 15s. The cost of labour in slacking and mixing the lime amounted to £78. The coals consumed by a small steam engine for working the agitators, and raising the precipitated matter from the bottom of the tanks, amounted to seventy-three tons, and cost £25 11s.; and the cost of labour for working the engine, attending to the tanks and machinery, and for attending to and storing the manure or precipitate was £76 14s., making a total of £550, which is equal to a cost of less than twopence per annum per individual, or twopence for every 16,000 gallons of sewer water deodorised. These works have now been in efficient daily and continuous operation for the last six years, during which period all the sewer water from the borough has been thoroughly disinfected and rendered innoxious before it has been permitted to flow into the river Soar; and to those who can recollect the condition of the Soar at the North Bridge, above and below, in former years, it will perhaps appear a desideratum that was worth paying for to obtain. These works, although commercially a failure, for the reason before given, have been most successful as a sanitary measure, and although the gentlemen who expended large sums in the solution of this important question have been great pecuniary losers, nevertheless, the borough of Leicester has derived great benefit from their operations; and I believe it is an undisputed fact that the sewerage of the borough has been more completely and economically effected than in any other town in England.—*Thos. Wickstead, C.E.*

ANSWERS TO CORRESPONDENTS.

. In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Magenta Dye.—Cotton without a Mordant writes as follows:—In reading the CHEMICAL NEWS for April 27, 1861, I find a letter signed "Sharp, Leeds," saying that Magenta cannot be dyed on cotton without a mordant; but I beg to say that he is labouring under a mistake, as I have for the last three or four months both dyed it in the cotton hank and in the cotton warp, without a mordant, and I can get by far the better colour without, than with any mordant.

Green Extract.—1. There is no proximate principle in yeast which may be separated from it and be used to induce fermentation. Yeast globules are necessary for this purpose. 2. The crystals should be examined—are they organic or inorganic, acid, basic, or neutral?

Ammonia will be answered in a subsequent number.

C.R.—1. We think it is uniformly distributed. 2. It has only been proved in a few instances. 3. In ordinary analysis it would be overlooked. 4. It could only be proved by searching Chemical analysis. Perhaps we could assist you better if you were to communicate privately with us.

A Subscriber.—Papers on the subject are to be met with in the recent volumes of the *Philosophical Magazine*. No connected account is to be met with. Your coil cannot be modified. It may be used as it is, but it will only produce a few of the effects to a very limited extent.

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[1] *Philosophical Magazine*, May, 1860.

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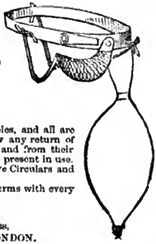

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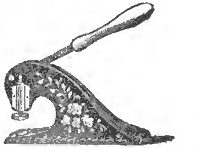

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"ARTHUR HILL HASSALL, M.D. Lond.,"
Analyst of the Lancet Sanitary Commission. Author of "Food and its Adulteration," "Adulterations Detected," and other Works.
London, October 31, 1860.

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"Worcester, January 1, 1861.

"Westbourne Park, Bayswater, London, W.
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THE

CHEMICAL NEWS

AND

Journal of Physical Science.

Edited by] [*Wm. Crookes, F.C.S.*

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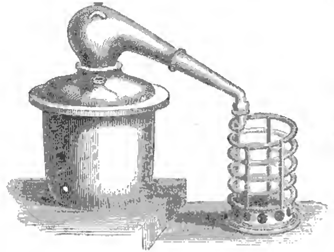

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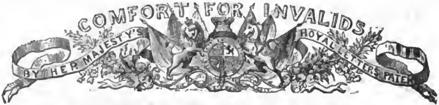
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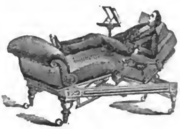
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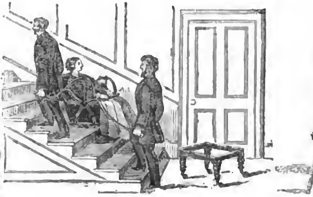
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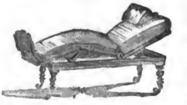
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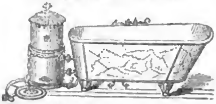

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THE CHEMICAL NEWS.

Vol. III. No. 80.—June 15, 1861.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On a Fifth Element belonging to the Alkali Group, by Professor Bunsen.

In the investigation of the new metal having so close an affinity to potassium, and which has been called cæsium by Bunsen, there appears to exist, besides this cæsium, yet a fifth alkali metal, which has hitherto escaped notice, and which appears to resemble potassium quite as much as cæsium does.

The platinum salt of cæsium is with much more difficultly soluble in water than that of potassium. If we attempt to separate the latter from the former by repeated boiling with water, we find that in proportion as the quantity of potassium diminishes, and the continuous potassium spectrum between Ka α and Ka β becomes faint, new lines appear, and among these in particular two very intense ones in the violet between Sr δ and Ka β. A point is soon reached at which the quantity of potassium is no longer diminished by boiling with water. This occurs when the united atomic weight of the metals combined with chlorine and platinum is 109 ($H = 1$). If, now, a mixture of the hydrates of potassium and cæsium be prepared from the platinum compounds, and about a fifth of this mixture be converted into carbonate, then absolute alcohol will abstract from the dried mixture almost exclusively hydrate of cæsium. If this operation be repeated, a limit is at length reached at which that which is dissolved in the alcohol has a constant composition. This occurs when the atomic weight has risen from 109 to 123·4 ($H = 1$). The substance which possesses this enormous atomic weight (next to gold and iodine, the greatest known) forms a deliquescent hydrate, as caustic as hydrate of potassium; it forms an equally deliquescent carbonate, which, at ordinary temperatures, is soluble to the extent of 10 parts in 100 of absolute alcohol; and an anhydrous nitrate, which crystallises, not rhombic, like nitre, but hexagonally, and by a hemiedral form, is isomorphous with nitrate of sodium, &c. The spectrum of this substance, purified up to an atomic weight of 123·4, shows the blue cæsium lines with great intensity, but the violet lines of the unpurified substances (atomic weight 109) so faintly, that a slight addition of chloride of potassium, which scarcely affects the lines Cs α, makes them disappear at once, on account of the brightness of the ground produced by the potassium. The few grammes of material for this investigation were obtained from 44,000 kilogrammes of the Dürkheim mineral water. By a repetition of the process, from 150 kilogrammes of Saxon lepidolite, there was obtained by the first treatment with chloride of platinum, a product which exhibited the violet lines between Sr δ and Ka β with the utmost intensity, but no traces of the lines Cs α. If this platinum salt from lepidolite had been a mixture of the cæsium and potassium compounds, the blue lines Cs α must have been visible along with the violet ones, because in the product obtained from the Dürhheim water, the violet lines disappear, first on the addition of chloride of potassium, and the cæsium lines much later, and, indeed, only when the potassium salt is in very great excess. It follows that there must exist, besides, potassium, sodium, lithium, and cæsium, yet a fifth alkali metal, which occurs in Dürkheim, Kreusnach, and other mineral springs, in small, but in lepidolite in larger quantities.—*Bericht der Akad. der Wissensch. zu Berlin,* 1861, s. 273—275.

On the Expansion of Metals and Alloys, by F. Crace Calvert, *Ph.D., F.R.S., F.C.S., &c.,* R. Johnson *F.C.S., and* G. Cliff Lowe.

(Continued from page 317.)

We now give the details of our experiments to enable the reader to estimate or verify the co-ordination of our results.

	Division of the Scale read off in 25-000ths of an inch in raising temperature from 10 to 90 degs.			Mean.	Divisions of same Scale read off in cooling from 90 to 100 degs.			Mean for 100 degs., calculated from these means, and corrected for expansion of vessel, &c., by deducting 20·0.	
	1st.	2nd.	3rd.		1st.	2nd.	3rd.	Mea.	
	°	°	°	°	°	°	°	°	°
Cadmium	174	171	172	172·3	176	173	172	173·7	196·2
Lead	155	156	157	156	161	160	159	160	177·5
Tin	142	142	145	143	147	148	147	147·3	161·5
Aluminium	120	120	120	120	122·5	121	122	121·8	131·1
Forged Zinc	119	121	120	120	120	119	120	119·7	129·8
Silver	110	109	109·5	109·5	111·5	110	110	110·5	117·5
Copper (pure), Cast..	106	105·5	106	105·8	103	103·5	107	104·5	111.4
" Hammered	99	99	99	99	101	99	100	100	104·4
Gold	81	80·5	—	80·7	81·5	81	—	81·3	81·3
Bismuth	78	77	77·5	77·5	81·5	80	80	80·5	78·8
Wrought Iron ..	69	72	73	71·3	73·5	72	72·5	72·7	70·0
Cast Iron	67	68·5	68·5	68·0	68	70·5	70·5	69·7	66·1
Steel (soft)	66·5	62	63	63·8	66·5	65	66·5	66·	61·1
Antimony	63	62	—	62·5	63	62	—	62·5	58·1
Platinum	57·5	—	—	57·5	58	—	—	58	52·2

From the above observations, we deduce the following Table of co-efficients of linear expansion from 0° to 100°:—

Cadmium (pure)	0·00332
Lead (pure)	0·00301
Tin (pure)	0·00273
Aluminium (commercial) . . .	0·00222
Zinc, forged (pure)	0·00220
Silver (pure)	0·00199
Gold (pure)	0·00138
Bismuth (pure)	0·00133
Wrought iron	0·00119

Cast iron	0·00112
Steel (soft)	0·00103
Antimony (pure)	0·00098
Platinum (commercial)	0·00068

On comparing these co-efficients with those found by previous experimenters, we find that they agree very closely in those cases when commercial metals have been employed. But when we come to those metals which we employed in a pure state, such as lead, tin, zinc, silver, cadmium, bismuth, antimony, and gold, we find a marked difference, which we attribute to our experiments having been made with pure metals, and we are confirmed in this view by the following experiments:—

We found that a small per-centage of impurity (such as 3 or 4 per cent. in iron) produced a considerable deduction in the ratio of expansion.

Wrought iron	0·00119
Cast iron	0·00112

The composition of these metals is as follows:—

Cast Iron.

	First Analysis.	Second Analysis.	Mean.
Carbon	2·320	2·230	2·275
Silicium	2·770	2·670	2·720
Phosphorus	0·580	0·710	0·645
Sulphur	0·318	0·288	0·301
Manganese and aluminium	—	traces	traces
Iron	94·059	94·059	94·059
	100·047	99·957	100·000

The wrought iron was from the same running, and was composed as follows:—

	First Analysis.	Second Analysis.	Mean.
Carbon	0·100	0·122	0·111
Silicium	0·095	0·082	0·088
Sulphur	0·093	0·096	0·094
Phosphorus	0·117	—	—
Iron	—	—	—

In the course of these experiments we have met with facts which, we believe, will prove highly interesting.

Iron Crystallised by Vibration.

	1st.	2nd.	3rd.	Mean.
90° to 10°	69	66·5	65·5	66.8
Mean of 100°, minus 20				63·5

Wrought Iron, as in preceding Table.

Co-efficient of expansion	0·00119
Ditto of iron crystallised by vibration	0·00108

We have been led to devote particular attention to these facts on account of the great use which is made of these metals in naval and other constructions. Although we have only given one example of the influence of crystallisation upon the ratio of expansion of metals, still we have observed several others in the course of our experiments upon metals; and in further researches upon minerals and rocks, which we shall shortly publish, this influence will be strikingly exemplified.

Another example of the influence of molecular condition upon the ratio of expansion is the change which takes place in steel when tempered at different degrees of temperature, showing that a slight change in the arrangements of the particles of a body affects its ratio of expansion.

The steel bar upon which these experiments were made was nearly double the length of the bars used in previous experiments. The figures here given are those read off in the same manner as those previously detailed; but the mean for 100° is calculated, and the correction applied to reduce it to the mean for a bar of 60 millimètres in length for the sake of uniformity.

	Raising Temperature from 16° to 19°.			Mean.	Cooling Temperature from 90° to 10°.			Mean.	Mean calculated and corrected for a bar 60 mills for 100°.
	1st.	2nd.	3rd.		1st.	2nd.	3rd.		
	°	°	°	°	°	°	°	°	°
Steel bar, as purchased	111·0	112·0	111·5	111·5	113·0	113·0	115·0	113·7	64·6
Steel bar at maximum of softness	107·0	108·0	107·5	107·5	107·0	112·0	111·0	110·0	62·3
Same bar at maximum of hardness	141·0	145·0	140·0	142·0	138·0	139·0	139·0	138·7	84·3

Co-efficient of first experiment		0·00109
,, second	,,	0·00106
,, third	,,	0·00142

In making experiments upon zinc with different axes of crystallisation, we met with further instances of the influence of molecular condition. The respective ratios of zinc, cast horizontally and vertically, were found to differ considerably, the former having only one axis of crystallisation, while the latter has two.

	Raising Temperature from 10° to 90°.				Mean.	Cooling Temperature from 90° to 10°.			Mean.	Mean for 100°, less correction.
	1st.	2nd.	3rd.	4th.		1st.	2nd.	3rd.		
	°	°	°	°	°	°	°	°	°	°
Zinc, cast vertically	224	226	227	226	226	232	233	234	233	266·9
Zinc, cast horizontally	187·0	186·5	187·0	—	186·8	190·5	193·0	192·5	192·0	216·7

Co-efficient of linear expansion of zinc:—

Cast horizontally	0·00367
Cast vertically	0·00452

We shall now proceed to give the results obtained with certain series of alloys composed of equivalents of pure metals.

Zinc and Tin.

	10° to 90°.				Mean for 100°, minus correction 20·0°.	90° to 10°.				Mean for 100°, minus correction 20·0°.
	1st.	2nd.	3rd.	Mea.		1st.	2nd.	3rd.	Mea.	
°	°	°	°	°	°	°	°	°	°	°
5. Zn = 74·43	159·0	156·0	—	157·5	176·8	137·5	156·0	—	156·7	175·8
Sn = 26·57	—	—	—	—	—	—	—	—	—	—
4. Zn = 68·86	148·0	149·0	—	148·5	165·6	149·0	151·0	150·0	150·0	167·5
Sn = 31·14	—	—	—	—	—	—	—	—	—	—
3. Zn = 62·43	142·5	143·5	—	143·0	157·5	151·0	147·5	147·5	148·6	165·8
Sn = 37·57	—	—	—	—	—	—	—	—	—	—
2. Zn = 53·11	142·5	141·5	—	142·0	157·5	140·0	142·0	—	141·0	156·2
Sn = 46·89	—	—	—	—	—	—	—	—	—	—
Zn = 35·61	129·0	131·0	—	130·0	142·5	132·0	132·0	—	132·0	145·0
Sn = 64·39	—	—	—	—	—	—	—	—	—	—
Zn = 21·65	138·0	138·0	140·0	138·6	153·3	141·0	141·0	—	141·0	156·2
2. Sn = 78·35	—	—	—	—	—	—	—	—	—	—
Zn = 15·15	138·0	140·0	—	139·0	153·7	142·0	141·0	—	141·5	156·8
3. Sn = 84·85	—	—	—	—	—	—	—	—	—	—
Zn = 12·12	134·0	134·0	—	134·0	147·5	139·0	136·0	—	137·5	151·8
4. Sn = 87·88	—	—	—	—	—	—	—	—	—	—
Zn = 9·95	134·0	135·0	136·0	135·0	148·7	137·0	137·0	137·0	137·0	151·2
5. Sn = 90·05	—	—	—	—	—	—	—	—	—	—

Lead and Antimony.

	10° to 90°.				Mean for 100°, minus correction 20·0°.	90° to 10°.				Mean for 100°, minus correction 20·0°.
	1st.	2nd.	3rd.	Mea.		1st.	2nd.	3rd.	Mea.	
°	°	°	°	°	°	°	°	°	°	°
5. Pb = 88·92	146·0	148·0	—	147·0	163·7	149·0	—	—	149·0	166·2
Sb = 11·08	—	—	—	—	—	—	—	—	—	—
4. Pb = 86·52	138·0	138·0	139·0	138·3	152·8	144·0	144·0	143·0	143·6	159·5
Sb = 13·48	—	—	—	—	—	—	—	—	—	—
3. Pb = 82·80	134·0	137·0	—	135·5	149·3	140·0	134·0	139·0	137·6	152·0
Sb = 17·20	—	—	—	—	—	—	—	—	—	—
2. Pb = 76·32	128·0	128·0	—	128·0	140·0	130·0	129·0	—	129·5	141·8
Sb = 23·68	—	—	—	—	—	—	—	—	—	—
Pb = 61·61	107·0	108·0	—	107·5	114·3	111·0	111·0	—	111·0	118·7
Sb = 38·39	—	—	—	—	—	—	—	—	—	—
Pb = 47·60	94·0	94·0	—	94·0	97·5	96·0	94·0	—	95·0	98·7
2. Sb = 52·40	—	—	—	—	—	—	—	—	—	—
Pb = 34·86	90·0	—	—	90·0	92·5	90·0	91·0	—	90·5	93·1
3. Sb = 65·14	—	—	—	—	—	—	—	—	—	—
Pb = 28·63	83·0	—	—	83·0	83·7	84·0	—	—	84·0	85·0
4. Sb = 71·37	—	—	—	—	—	—	—	—	—	—
Pb = 24·30	77·0	77·0	—	77·0	76·2	79·0	78·0	—	78·5	78·1
5. Sb = 75·70	—	—	—	—	—	—	—	—	—	—

Zinc and Copper.

	10° to 90°.				Mean for 100°, minus correction 20·0°.	90° to 10°.				Mean for 100°, minus correction 20·0°.
	1st.	2nd.	3rd.	Mea.		1st.	2nd.	3rd.	Mea.	
°	°	°	°	°	°	°	°	°	°	°
5. Zn = 83·70	128·0	128·0	—	128·0	140·0	130·0	130·0	—	130·0	142·5
Cu = 16·30	—	—	—	—	—	—	—	—	—	—
4. Zn = 80·43	129·0	130.5	—	129·7	142·1	131·0	131·5	—	131·2	144·0
Cu = 19·57	—	—	—	—	—	—	—	—	—	—
3. Zn = 75·36	125·6	—	—	125·5	136·8	125·0	—	—	125·0	136·2
Cu = 24·64	—	—	—	—	—	—	—	—	—	—
2. Zn = 67·26	119·0	118·0	—	118·5	128·1	121·0	119·0	—	120·0	130·0
Cu = 32·74	—	—	—	—	—	—	—	—	—	—
Zn = 50·68	118·0	—	—	118·0	127·5	118·0	—	—	118·0	127·5
Cu = 49·32	—	—	—	—	—	—	—	—	—	—
Zn = 33·94	107·0	107·0	—	107·0	113·7	110·0	110·0	—	110·0	117·5
2. Cu = 66·90	—	—	—	—	—	—	—	—	—	—
Zn = 25·52	100·0	101·0	—	100·5	105·6	103·0	102·5	—	102·7	108·2
3. Cu = 66·06	—	—	—	—	—	—	—	—	—	—
Zn = 20·44	104·5	—	—	104·5	110·6	105·0	—	—	105·5	111·8
4. Cu = 79·56	—	—	—	—	—	—	—	—	—	—
Zn = 17·05	102·0	—	—	102·0	107·5	102·0	—	—	102·0	107·5
Cu = 82·95	—	—	—	—	—	—	—	—	—	—

Copper and Tin.

	10° to 90°.				Mean for 100° C., minus correction 20·0°.	90° to 10°.				Mean for 100°, minus correction 20·0°.
	1st.	2nd.	3rd.	Mea.		1st.	2nd.	3rd.	Mea.	
°	°	°	°	°	°	°	°	°	°	°
5. Sn = 90·27	127·0	124·0	124·0	125·0	136·2	129·0	124·0	—	126·5	138·1
Cu = 9·73	—	—	—	—	—	—	—	—	—	—
4. Sn = 88·14	122·0	122·0	—	122·0	132·5	127·0	126·0	—	126·5	138·1
Cu = 11·86	—	—	—	—	—	—	—	—	—	—
3. Sn = 84·79	119·0	119·5	—	119·2	129·5	123·0	122·0	123·5	122·8	133·5
Cu = 15·21	—	—	—	—	—	—	—	—	—	—
2. Sn = 78·79	118·0	118·0	—	118·0	127·5	117·0	117·0	—	117·0	126·2
Cu = 21·21	—	—	—	—	—	—	—	—	—	—
Sn = 65·02	109·5	111·5	—	110·5	118·1	110·5	110·5	—	110·5	118·1
Cu = 34·98	—	—	—	—	—	—	—	—	—	—
Sn = 48·17	111·0	111·0	—	111·0	118·7	113·5	112·0	—	112·7	120·8
2. Cu = 51·83	—	—	—	—	—	—	—	—	—	—
Sn = 34·21	112·0	111·0	—	111·5	119·3	113·0	112·0	—	112·5	120·6
3. Cu = 61·79	—	—	—	—	—	—	—	—	—	—
Sn = 31·73	102·0	105·0	—	103·5	109·3	106·0	105·0	—	105·5	111·8
4 Cu = 68·27	—	—	—	—	—	—	—	—	—	—
Sn = 27·10	104·0	104·0	—	104·0	110·0	106·0	106·0	—	106·0	112·5
5. Cu = 72·90	—	—	—	—	—	—	—	—	—	—
Sn = 15·68	101·0	101·0	—	101·0	106·2	101·0	101·0	—	101·0	106·2
10. Cu = 84·32	—	—	—	—	—	—	—	—	—	—
Sn = 11·03	95·0	94·5	—	94·7	98·3	94·5	95·0	—	94·7	98·3
15. Cu = 88·97	—	—	—	—	—	—	—	—	—	—
Sn = 8·51	97·0	99·5	—	98·2	102·7	99·0	99·0	—	99·0	103·7
20. Cu = 91·49	—	—	—	—	—	—	—	—	—	—
Sn = 6·83	99·0	99·0	—	99·0	103·7	100·0	100·0	—	100·0	105.0
25. Cu = 93·17	—	—	—	—	—	—	—	—	—	—

The results obtained with the above series enable us to draw the following conclusions:—

1. That the various bronzes and brasses have a ratio of expansion less than that indicated by theory.

2. That, notwithstanding that the alloys Sn Cu_3 and Sn Cu_4 contain respectively 34·21 and 31·73 per cent. of a highly expansive metal—tin,—still their ratio of expansion is equal to that of pure copper, the presence of tin in these alloys having no influence on their rate of expansion.

(To be continued.)

On the Oxalates of Iron, by T. L. Phipson, *Ph.D., Member of the Chemical Society of Paris, &c. &c.*

The two oxalates of iron of which I am about to speak have not yet been submitted to any investigation. They have been seen by several chemists, who have mentioned their existence in a cursory manner; but their composition and properties have never been examined.

In Nature there exists an oxalate of protoxide of iron, known by the name of *Humboldtite* (*Oxalite* or *Eisenresin* of some mineralogists), and which, according to the analysis of M. de Rivero, consists of one atom of oxalic acid and one atom of protoxide of iron. I have not yet had an opportunity of examining this mineral, nor have I endeavoured to form it artificially in the laboratory.

1. Oxalate of Protoxide of Iron.—When a dissolution of oxalic acid or of oxalate of ammonia is added to sulphate of protoxide of iron, and the mixture violently stirred, a considerable quantity of a yellow precipitate is obtained in a short time. This bright-coloured precipitate is very stable, even in contact with strong acids. Neither air nor light have any action upon this new salt at the ordinary temperature of the atmosphere; it is, moreover, insoluble in water and in dilute sulphuric and hydrochloric acids; it cannot either be dissolved in any excess of oxalic acid. When treated with concentrated nitric acid, it is suddenly decomposed, with an abundant evolution of rutillant vapours.

The best method of analysing this salt I found to be the following:—By calcining the salt the iron can be estimated accurately as peroxide of iron; the other elements, water and oxalic acid, must be determined by combustion of the salt with oxide of copper, as in an ordinary organic analysis. The water is received and weighed in a chloride of calcium tube, and the oxalic acid calculated from the amount of carbonic acid produced by combustion:—

Two analyses made in this manner gave me the following results :—

	I.		II.	Theory.
Protoxide of iron .	19·44		19·40	20·00
Oxalic acid (by diff.)	59·56	(by combust.)	59·75	59·99
Water (direct) . .	21·00	(by diff.)	20·85	20·00
	100·00		100·00	99·99

This composition corresponds, therefore, with the formula,—

$$FeO3C_2O_3 + 4HO,$$

or, if we assume that oxalic acid contains hydrogen with the formula,—

$$FeO(C_2HO_4)_3 + HO.$$

When this yellow powder is submitted to the action of heat, it takes fire when its temperature is very slightly elevated, and burns like so much tinder, leaving a residue of very pure peroxide of iron. When ferricyanide of potassium is poured upon it, in sufficient quantity to obtain a partial decomposition only, a fine pale-green colour is formed, which might be used as an oil-colour or a water-colour, and the tint of which may be varied with the quantity of ferricyanide employed. When the decomposition with the latter salt is complete, the ordinary blue is obtained.

If the green powder, procured in the manner just stated, be thoroughly desiccated and then heated a little, it also takes fire and burns like tinder, leaving nothing but peroxide of iron.

2. Oxalate of Peroxide of Iron.—I obtained this salt by dissolving hydrate of peroxide of iron in an excess of oxalic acid; it is best to perform this operation at night or in the shade. A fine green solution is thus obtained, which, evaporated and then left to itself, soon gives emerald-green crystals. These are oblique prisms, and are very soluble in water.

When these crystals are exposed to the light of the sun they blacken on their borders, like chloride of silver; and if they be then sprinkled over with water, a peculiar crackling noise is heard, after which they dissolve, leaving a residue of yellow oxalate of protoxide. This experiment is very striking if a capsule, the bottom of which is covered with the crystals in question, be submitted to the direct influence of the solar rays,—one-half of the crystals being protected from the light by the projecting border of the capsule remaining unaltered; the other half of the crystals blackens rapidly.

The same decomposition takes place with the green solution. When this solution is exposed to solar light it gradually deposits yellow oxalate of protoxide of iron in microscopic crystals, and, in the space of a week or two, the whole solution becomes perfectly colourless. If a bar of iron be placed in the solution the reduction takes place without the influence of light, and a yellow powder of oxalate of protoxide is thrown down.

The analysis of these green crystals gave me the following result :—

	Found.	Calculated.
Peroxide of iron . .	20·00	20·25
Oxalic acid	45·00	45·58
Combined water . .	24·00	22·76
Water of crystallisation	11·00	11·41
	100·00	100·00

Whence we obtain the formula,—

$$Fe_2O_35C_2O_3 + 5HO + 10Aq.,$$

or, if we suppose oxalic acid to contain hydrogen,—

$$Fe_2O_3(C_2HO_4)_5 + 10HO.$$

The crystals lose their water of crystallisation at about 120° Centigrade, and, at the same time, their colour; when all this water of crystallisation is driven off they are perfectly white. The remainder of the water contained in the salt is only expelled by calcination. The same may be said of the greater portion of the water contained in the yellow oxalate of protoxide.

A curious fact connected with these two salts is, that the salt of protoxide is yellow, and the salt of peroxide green, contrary to what we generally remark with salts of iron. Generally, also, the salts of protoxide of iron are decomposed under the influence of exterior agents, and are converted into salts of peroxide, which are ordinarily more stable. In the cases before us precisely the reverse takes place.

The reduction of the peroxide salt into a salt of protoxide by the influence of light is very remarkable. It is, like all chemical actions of light, a phenomenon of reduction. I am not yet perfectly sure of what takes place in this circumstance. I have analysed the yellow salt deposited in this decomposition, and find it has for composition,—

$$FeO.3C_2O_3 + 4HO$$

In fact, that it is the same oxalate of protoxide of iron described above, and from which it differs only by being crystalline, the other being amorphous.

When we come to consider the constitution of these oxalates of iron, we may admit for oxalic acid the formula,—

$$C_2HO_4,$$

which is probably the most rational manner of representing its composition; the oxalate of peroxide of iron, for instance, instead of—

$$Fe_2O_3(C_2O_3)_5 + 5HO + 10Aq.,$$

becomes,—

$$Fe_2O_35(C_2HO_4) + 10HO,$$

and the oxalate of protoxide,—

$$FeO(C_2HO_4)_3 + HO.$$

The presence of *formic acid* among the products of distillation of oxalic acid authorises us to look upon oxalic acid as containing hydrogen. But, whilst admitting this, we cannot double the formula, as M. Wurtz has been obliged to do in supposing oxalic acid to be derived from glycol. In this theory oxalic acid is represented by $C_4H_2O_8 = 2C_2HO_4$; and it is evident that M. Wurtz's theory is completely erroneous, since if $C_4H_2O_8$ were the real formula of oxalic acid, it would be impossible to form salts such as those described in this paper.

On a New Principle in Volumetric Analysis.

In the *Annal. der Chemie und Pharmacie*, cxvi. p. 128, Dr. Mohr, of Coblentz, propounded a problem in volumetric analysis, and invited those interested in the subject to solve it. The first who did so was to receive one of Dr. Mohr's burettes as a prize. Dr. Mohr asserted at the same time that he had himself already solved it.

The problem was,—"To perform quantitative determinations without the use of weights, with volumetric solutions of unknown strength, and the strength of which must not be ascertained, and regulated." Dr. Mohr received a number of communications, in which, however, some condition was violated; either a weight of some kind was used, or the strength of the test liquid was indirectly determined by saturated solutions of chloride of sodium (as in Liebig's urea test), or in some other way.

The problem was first solved by Dr. Pauli, of the Union Alkali Works, St. Helen's, Lancashire, on December 28, 1860, in the following way. In one of the pans of a balance is put a piece of chemically-pure carbonate of soda, and in the other an equal weight of an undetermined soda, and both are measured by an acid of unknown strength. Suppose that in the first case 15 cubic centimètres, and in the second 11 cubic centimètres have been used, then, 15 : 11 : : 100 : 73·33. The soda, therefore, contains 73·33 per cent. carbonate of soda.

The solution of the question, for which Dr. Pauli only cites a special case, is generally this:—

Equal portions of the pure substance to be determined and the impure are to be weighed off, one against the other, and are then to be measured with the same fluid; then the cubic centimètres used for the pure substance represent 100 per cent, and the other number proportionately less. Suppose, for example, that it is proposed to determine the iron in an iron ore. A piece of pure iron wire is placed in the scale of a balance, and is exactly counterpoised by means of the powdered ore, both are then brought into solution as protoxide in the usual way, and are then treated with permanganate of unknown strength. If oxide of iron is to be determined, pure oxide must be used instead of the wire. If potashes are be examined, pure carbonate of potash (recently heated to redness) should be used. It would appear from this, for every analysis the strength must be determined by the pure substance. This, however, is easily avoided if we put a sixpence into the scale-pan, and weigh with this both the pure and impure substance. The number of cubic centimètres of the fluid holds good as long as the same fluid and the same sixpence are used, and this number may be marked upon the bottle as expressing 100 per cent. for the same substance.

This method is capable of universal application, and eliminates possible errors in weights and variations of temperature. It is only necessary that the substance to be determined should be available in the pure state. But how is the problem to be solved, if it remains as before, but with the further condition, "when the substance to be determined is not available in a pure state?" Dr. Mohr received two other solutions of the problem from Dr. Hiller and Herr Dietrich, both students in Heidelberg. Dr. Hiller has also solved the question, even with the condition that the pure substances should not be available, and in the same way as Dr. Mohr had already done it. For example,—if no chemically pure peroxide of manganese can be obtained for a manganese determination, according to Dr. Hiller, pure permanganate, or pure bichromate of potash, can be employed to weigh off the manganese; both are then converted by distillation with hydrochloric acid into chlorine, and then into iodine, and both fluids are then to be determined with the same unknown solution of hyposulphite of soda. Instead of using pure permanganate, it would be better perhaps to use pure iodine to counterpoise the manganese, then to dissolve the iodine in iodide of potassium, and proceed as before. We have now to convert the value of the iodine into the value of an equal weight of pure peroxide of manganese, and express it in the cubic centimètres of hyposulphite used. As 43·57 MnO_2 set 127 iodine at liberty, therefore any given weight of MnO_2 would set at liberty $\frac{127}{43.57}$, or 2·915 times as much iodine as is employed to counterbalance it. We have, therefore, to multiply the number of cubic centimètres of hyposulphite which have been used for a quantity of iodine equal to the manganese by 2·915, and then to proceed as if pure MnO_2 and common manganese had been weighed together.

If pure carbonate of soda cannot be obtained, pure carbonate of lime may be used; the cubic centimètres of the acid used must be multiplied by $\frac{50}{53}$, that is, 1 atom of carbonate of lime divided by an atom of carbonate of soda.—*Annalen der Chemie und Pharmacie*, cxvii. 386.

TECHNICAL CHEMISTRY.

On the Chemical Composition of Cast Iron and Steel, by M. Fremy.

(Continued from page 346.)

To establish clearly the existence of a nitrogenised charcoal, I have had recourse to a synthetic experiment which clears up all doubt on the subject. I made choice of a very pure organic matter,—for instance the best quality of sugar, nitrogenising it according to the invaluable directions given in a recent communication by M. Paul Thenard, and then submitting the compound to prolonged calcination—the charcoal thus obtained is highly nitrogenised, and disengaged ammonia for a long time by the action of hydrogen.

These experiments prove that organic nitrogenised substances, such as those composing animal and even vegetable tissues, will by calcination yield a nitrogenised charcoal, which by slow combustion, like that produced in cementation cases, will then disengage its nitrogen in the state of ammonia under the influence of hydrogen or of aqueous vapour. Nitrogenised charcoal is then a sort of magazine of nitrogen suitable for cementation; it decomposes with a degree of slowness favourable to the operation itself, and is perhaps one of the conditions of the quality of the product.

The facts I have brought before the Academy will probably be utilised in manufacture, and will account for certain practical operations well known among steel-workers. Thus all the organic bodies which produce rapid acieration, such as horn, soot, leather, animal excreta, are precisely those which yield highly nitrogenised charcoal. Ammoniacal compounds can nitrogenise even ternary organic substances.

The utility of nitrogen in charcoal explains a very curious practical fact, which is, that the charcoal of the cementation cases, after having been heated for a certain time, becomes exhausted, and requires to be replaced by fresh charcoal. The old theory, which assigned to carbon an exclusive part in cementation, could not account for this fact, admitting that the nitrogen is in the end carried off by the action of hydrogenised bodies, which, according to my novel views, is perfectly intelligible. The nitrogen for converting iron into steel is then yielded both by air and by wood charcoal, which always contains nitrogen. A notable quantity of nitrogen may even pre-exist in iron; the purest sorts of iron, and which are most easily convertible into steel, are just those containing nitrogen.

By submitting certain kinds of iron to the action of essence of turpentine vapours, I have sometimes produced an evident degree of acieration. As an explanation of this phenomenon, I have always found on examining the metal afterwards, that it contained nitrogen. Thus, those chemists who deny the influence of nitrogen in producing steel with carburets of hydrogen or diamond, are greatly at fault, for ordinary iron often retains sufficient nitrogen to form steel by the action of a simply carbonising body. Moreover, I shall by-and-by prove that nitrogen can be replaced in acieration by another metalloid bearing some chemical analogy to it, such as phosphorus, and yield compounds which may easily be confounded with normal steel.

After showing that steel is always produced in presence of nitrogen, the influence of this body being proved by laboratory experiments and practical observations, I must now inquire in what way nitrogen influences acieration.

As I said in a preceding communication, the action of nitrogen in acieration is twofold; not only does it combine with the iron, but it renders the metal porous, and makes clear the hitherto obscure phenomenon of cementation, namely, how a solid body can penetrate a metallic mass.

The most distinguished savants engaged in enquiries touching acieration know that cementation is a fact difficult to comprehend. I am about to prove that the properties of nitride of iron permit the most simple explanation of this penetration of the metal. In fact, it has been shown that nitrogen furnished by ammonia can penetrate a mass of iron and produce the compound I have studied under the name of nitrogenised iron; this body submitted to the action of hydrogen is reduced, leaving the metal remarkably porous. Steel is produced by heating this nitrogenised iron in a current of carburetted hydrogen gas; a portion of nitrogen remains combined with the metal; carbon can then penetrate the pores produced by the departure of nitrogen and the disengagement of hydrogen or of ammonia.

All the mysteries of cementation are thus easily explained; the numerous blisters characterising cementation steel—blistered steel, as it is called—the formation of which it was impossible to explain by the old theory, are now intelligible: they are produced by gases resulting from the action of hydro-carburised compounds on nitride of iron. Thus, the chemical phenomena of cementation may be summarised in the following manner:—Ammonia produces nitrogenised iron in disengaging hydrogen, which renders the iron porous; the hydro-carburetted gases then decompose the nitrogenised iron, acting by their hydrogen and carbon; excess of nitrogen is disengaged in the state of ammonia or cyanide of ammonium, which increases the porosity of the metal; while the carbon unites itself to the remaining nitrogen, forming the nitro-carburetted compound which seems to be the element essential to the composition of steel. This twofold action of nitrogen, which M. Despretz's experiments foreshadowed, is in a theoretical point of view an exceedingly interesting fact.

Is it not curious to see nitrogen, whose chemical activity is usually so little developed, combining with two bodies present in cementation cases, playing the part of a shuttle, and becoming a powerful cementation agent? It is impossible to consider the important part which I make nitrogenised iron play in cementation as a theoretical idea unconfirmed by experience. It cannot be forgotten that nitrogenised iron is produced at red heat, and that I have maintained it at that temperature for ten hours without its decomposing, and that, when afterwards submitted to a carburising influence, it has formed steel. These are precisely the conditions which we find realised in cementation cases.

I have now to inquire whether, in the fabrication of steel, by refining cast iron, nitrogen exerts an influence as important as in cementation. I first prove that in steel produced by puddling, or by a small furnace, the presence of nitrogen is easily shown, either by making a current of hydrogen act on the filings, and so disengaging ammonia, or by examining the nitrogenised residue resulting from the action of acids on this kind of steel.[1]

Steel produced in a small furnace, in contact with wood charcoal, and, consequently, under circumstances in which cyanides are readily formed, the nitration of iron and its transformation into steel can easily be understood.

In puddling furnaces acieration is sometimes due to the nitrogen contained in cast iron, but chiefly to the action of nitrogenised compounds furnished by the combustible, and the air, which acts rapidly on the iron when it commences to become red, porous, and in a nascent state.

(To be continued.)

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, March 8, 1861.

Discourse on some Phenomena attending Combustion in Rarefied Air, by Dr. Edward Frankland, *F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.*

The results which I shall endeavour to lay before you this evening originated in a series of observations which I undertook when accompanying Dr. Tyndall in the ascent of Mont Blanc in the autumn of 1859. I then took with me a number of candles—half-a-dozen stearine candles, in fact—which were carefully weighed in Chamounix before the ascent, and were burned there for the space of an hour. These candles were subsequently taken to the top of the mountain, and were there carefully burned again, with the complete exclusion of draft for the space of another hour. On returning from the summit the candles were again carefully weighed, and the difference between the two results would indicate, of course, whether an increase of atmospheric pressure had had the effect of causing the combustion to go on more rapidly; or, on the other hand, whether rarefied air had the effect of decreasing the rapidity of combustion. We might naturally suppose that, inasmuch as at the summit of Mont Blanc we had not much more than half the amount of air, in the same bulk that we had at the level of the sea, that we should meet with a diminution in the rate of combustion of such materials as candles. The results of those experiments which, as I have said, led me to the investigation forming the particular subject of my discourse this evening, are contained in this table, which shows the difference in the rate of combustion of the same candles at Chamounix and at the summit of Mont Blanc.

Rate of Combustion per Hour.

At Chamounix.					At the Summit of Mont Blanc.				
1st	candle	lost	9·2	grammes	1st	candle	lost	8·7	grammes
2nd	,,	,,	9·9	,,	2nd	,,	,,	9·5	,,
3rd	,,	,,	9·2	,,	3rd	,,	,,	9·2	,,
4th	,,	,,	10·4	,,	4th	,,	,,	8·8	,,
5th	,,	,,	9·5	,,	5th	,,	,,	9·3	,,
6th	,,	,,	9·2	,,	th	,,	,,	9·0	,,

If we add up these results together and take the mean we shall find that the rate of combustion at Chamounix was

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[1] Chemists wishing to prove the presence of nitrogen in steel by means of hydrogen must avoid the numerous influences which decompose ammonia. They must, besides, seek for nitrogen when not rendered evident by hydrogen, either in the liquids proceeding from the action of acids on steel, or in the brown, insoluble substance resulting from this action. Very pure hydrate of potash can also, when hot, disengage the nitrogen contained in steel.

9·6 grammes per hour; and at the summit of the mountain the rate of combustion was 9·1 grammes per hour. But you see that one of these candles No. 4 yielded abnormal results. There was evidently something the matter with this candle which caused it to burn an abnormal quantity of stearine at Chamounix, and the quantity burned was a trifle smaller at the summit of the mountain than that burned by the others. It would, therefore, I think, only be fair to leave this fourth candle out of the question, and then we arrive at this average result, omitting the fourth candle, that the quantity of stearine burned was 9·4 grammes at Chamounix, and at the summit of the mountain 9·2 grammes per hour. So you see we can assure ourselves by this result, that the rate of combustion is entirely unaffected by the pressure of the atmosphere, and that candles and combustibles of that kind burn with exactly the same consumption of combustible matter under one circumstance as under the other. Whilst making these experiments in the tent at the top of the mountain, where we remained twenty-two hours, I was very forcibly struck with the difference in the character of the flame. The flame of an ordinary stearine candle with which we are all very familiar, exhibits generally this phenomenon—we have at the lower part of the flame a blue portion which does not, however, rise to within a quarter of an inch of the apex of the wick, there it terminates, and at this point the luminous portion of the flame commences. When the same candles were burned at the summit of the mountain, I noticed that this blue portion of the flame now extended to about an eighth of an inch above the apex of the wick, consequently the candles gave a very much smaller amount of light. The luminous effect was obviously, to the most casual observer, considerably inferior to the luminosity of the same candles at the base of the mountain. This led me on returning to England to institute a series of experiments upon this subject, and I found that on burning a candle in air rarefied to the extent to which it was rarefied on the summit of Mont Blanc the illuminating power of the candle diminished rapidly, and to a very considerable extent. The experiment was made in the apparatus here upon the table, and I cannot do better than repeat it in the way in which it was made. Here we have our stearine candle enclosed in a glass cylinder in connection with a powerful air-pump, by means of which we can obtain any amount of rarefaction within the cylinder. External air can be admitted below, and the products of combustion be carried off by a glass chimney, or cylinder, which is about an inch and a-quarter in diameter, and is connected with this tube, so that the products of combustion are thus carefully removed, and the candle is kept constantly burning in pure air, a condition which is obviously quite essential to the proper performance of the experiment. Now we will endeavour to light our candle. [The exhaustion of the cylinder was then commenced.] We have already a rarefaction of air to the extent of some twenty-four or twenty-five inches I believe. We have lighted our candle. We have here a stop-cock, which will enable us at pleasure to regulate the supply of external air. The candle is now burning at the ordinary atmospheric pressure, and giving its ordinary amount of light. You know when we first ignite a candle, it burns with a dull flame, or a small flame at all events, until a certain quantity of stearine has got melted. Whilst the flame is improving I will project upon the screen the image of a vacuum gauge by which you will be able to see the tension of the air which we have in our exhausting apparatus during the different stages of combustion. [The lights were then turned down.] There [pointing to the image on the screen] we have our vacuum gauge, which I hope will be visible even to those at a distance. This is a column of mercury which is gradually rising. We will take our barometer to be at 30 inches, the usual atmospheric pressure. The guage is now, you see, at 22·5, the next figure being 22, therefore we have a pressure of about 8 inches; and at this pressure we will introduce our burning candle. You see now how I am reducing the pressure, and notice the effect upon the luminosity of the candle. Now our candle, you see, is burning under a pressure of about 8 inches of mercury, instead of about 30 inches, and if we had the means here of taking the illuminating power there can be no doubt that we should find we have now a very small fraction of the light given by this candle before the exhausting process took place. Now we will gradually allow this pressure to increase. In fact, we shall find it necessary to let a little air in. You see it is gradually rising—the mercury is gradually falling. We had first 22½, now 22, and now it is going down to 21. Now, our candle is burning in a pressure of 10 inches, and you see the pressure is gradually increasing. You will notice that we have a corresponding increase in our illuminating effect. Our candle, which was barely luminous at all under a pressure of 8 inches of mercury, is now considerably more luminous when that pressure is increased to something like 18 inches, at which it now stands. Now we have a pressure of 18 inches—in fact, a little more. There we have now about the light that the candle gave at the summit of Mont Blanc, and which is obviously very different from the light of a candle, burning with its usual brilliancy. We will just restore the candle to its original atmospheric pressure, and without disturbing the equilibrium of its combustion. Now that our candle is burning under atmospheric pressure, you see what a very different result we have with regard to the light that is emitted.

I did not call your attention to a phenomenon which is perceptible at a short distance from the candle when the illuminating effect is diminished to its lowest point—a phenomenon which is very beautiful if it can be seen; but, unfortunately, it cannot be seen at a distance. It is this: when the luminous portion of the flame of the candle has almost entirely disappeared there suddenly appears, round the nucleus of the flame, another flame which had hitherto been invisible if it had existed at all; it extends to the width of about a quarter to half an inch on each side of the flame, and it is of a very beautiful purple tint. It can only be well seen when the light from the centre part of the flame is cut off by holding the finger or a piece of card before the flame sufficient to intercept the rays from the centre part. When that is done this beautiful colour in the external coating of flame comes out very beautifully. This coloured flame I have examined by means of the prism, and it appears to possess those rays which are permitted to pass by a solution of permanganate of potash, although it contains rather more blue rays, apparently, than the light transmitted by the permanganate; I will endeavour to show you the kind of rays which are perceptible in the flame of the candle by means of the prismatic spectrum. [The gas-lights were again turned down.] Now, we will endeavour to throw upon this screen, first of all, the prismatic spectrum itself containing all the usual colours and then we will cut off the necessary colours. There, we have this beautiful spectrum which, you see, contains at this extremity an intense red ray, then orange, then yellow, then green, then blue, then indigo, and finally violet. Now, the external flame of the candle which is seen so beautifully when you are near to the candle, and when, through the rarefaction of the air the luminosity of the flame has become almost extinct, contains a combination of the rays from the two extremities of the spectrum; it contains a portion of this red ray and it contains a portion of this violet ray. I will now introduce a solution of permanganate of potash which, as I have said, resembles the colour of this outer flame. [A beam of electric light was passed through a solution of permanganate of potash, the two colours of which were thereby shown upon the screen.] These are the colours of which the external coating of the flame is composed. If we have a mixture of these two colours, red and violet, you will get something like a notion of the colour of this external flame of the candle which is seen when the luminosity has almost disappeared. Here we have something nearly approaching it. Now, this effect is very curiously connected with a result that was first observed by Mr. Gassiot in the transmission of electric sparks through highly rarified air or highly rarified gases. Mr. Gassiot found that when these gases contained even a trace of nitrogen that the colour at once became of this pinkish hue — became quite similar to what we have observed in the external flame of the candle. I cannot do better, perhaps, than just show you this colour, which is, perhaps, more than any other colour like that of the external flame; in fact, I believe the cause of the colour in both cases to be identical—namely, the ignition of nitrogen; and that this spectrum which I have been showing you on the screen, is really the spectrum of incandescent nitrogen. Here we have nitrogen in this tube which has been highly rarefied; it does not contain more, perhaps, than from one-tenth to two-tenths of an inch of mercurial pressure. [The rarefied nitrogen was contained in a long cylindrical glass jar, through which a series of electric sparks were then transmitted.] There is the light in question which is, I believe, due to exactly the same cause as that light surrounding the candle flame—namely, the incandescence of the nitrogen in both cases.

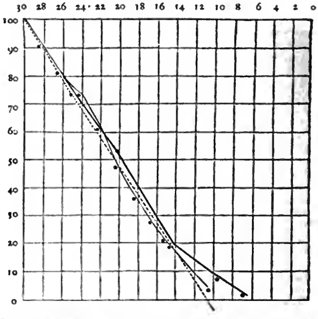
Upon commencing these experiments so as to bring them into strictly quantitative form, and ascertain with something like precision what is the actual diminution of light which takes place under these circumstances, I soon found that candles were perfectly useless for this purpose, and that when placed under these artificial conditions, candles, notwithstanding the quality of the materials of which they may be made, are liable to gutter; they burn with different lengths of wick, and the results, which occupied me several weeks in obtaining, were altogether unreliable. I therefore turned to gas, as being far more manageable and capable of uniform combustion under these, so to speak, abnormal conditions. We can, by means of a gas meter like this, regulate the supply of gas into this apparatus, and thus we have at our command a known quantity of combustible matter to be consumed in a certain time. The light emitted was measured by Bunsen's photometer, an instrument which is very well known. It consists of a central disc of paper, which is rendered transparent in the greater part of it by means of a solution of spermaceti in turpentine; and if this disc be placed between two sources of light in an otherwise dark room, and you move it until the illumination of both sides is equal, that equality of illumination will be manifested by the disappearance of that comparatively non-transparent spot of unsized paper in the centre of the disc; and by these means it is quite possible to determine, and with very considerable precision, the illuminating power of any flame. Here, to describe the apparatus employed, is a gas flame the combustion of which was rendered quite constant by passing the gas through a governor, regulating the pressure of the gas. Then the experimental gas flame was placed at the opposite extremity of the instrument, and this was graduated by a scale, so that on moving this disc, both sides of it could be simultaneously seen. This is, in fact, Bunsen's photometer. The first series of experiments was commenced with a pressure of 29·9 inches of air, and in each case I assumed the light of the experimental flame to be equal to 100, for the purpose of comparing the results. On reducing the pressure to 24·9 inches, the amount of light was decreased to 75. On reducing the pressure to 19·9 inches, the illuminating power was reduced to 52·9. Then, on reduction to 13·6 inches of pressure, the illuminating power was reduced to 20·2. On reducing it to 9·6, the illuminating power was reduced to 5·4, and finally, when the pressure was reduced down to 6·6, the illuminating power fell to 0·9, the fraction of a unit; so that you see we have decreased from 100 units of light down even to less than one unit. The light emitted by the combustion of the same kind of gas under this reduced pressure was not $\frac{1}{100}$th of the light under the ordinary atmospheric pressure.

This is the second series of experiments which were made.

Second Series.

Pressure of Air.	Illuminating Power.	
	Observed.	Calculated.
30·2	100	100
28·2	91·4	89·8
66·2	80·6	79·6
24·2	73·0	69 4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16 2	29·4	28·6
14 2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	—2·0

Each number is the result of the average of twenty observations—not merely a single observation, but the average of twenty in each case. This table goes down to 10·2 inches of pressure. Now, the result of these observations was this—that beginning at atmospheric pressure, and with 100 units of light, we decrease almost exactly at 5·1 units of light for each inch of mercurial pressure we diminish in coming down the scale. The results of these two series of experiments are seen upon this diagram, which will give you a clearer idea of the nature of these results and the accordance with each other, than any mere inspection of the tables.

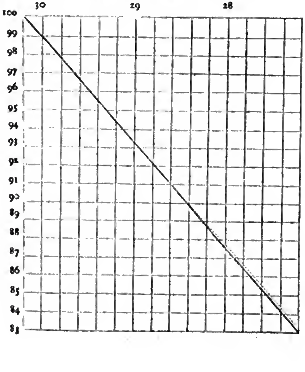


In this table the amount of light is represented by the horizontal lines. Here, beginning with no light at all, at the bottom of the table, you have 10, 20, 30, 40, 50. The perpendicular lines represent the amount of pressure—30 inches, 28, 26, 24, 22, 20, and so on. The black diagonal lines show the results of the two series of experiments. You see, therefore, that if the illuminating power of the flame had been constant for all pressures, then this line, instead of running down here diagonally, would run straight across the diagram. If, on the other hand, the illuminating power decreased exactly in the same ratio as the pressure—for the distance from 100 to nothing [is exactly the same as from 30 to nothing on the horizontal line here—then this line would proceed from this angle direct to the opposite angle [pointing]; but you see the light diminishes in a more rapid ration than the pressure. You find, also, another peculiarity—that, when you get down to something like 15 inches of pressure, there is a sudden divergence in that direction [pointing to the right of the diagram], showing that the light does not then decrease in so rapid a ratio as at a higher pressure; in fact, that, from 14 or 15 inches downwards, the decrease in the illuminating power is considerably less rapid than from 30 inches down to about 14 or 15. Taking, however, the atmospheric pressure down from 14 inches, we have then a series of descending steps, so to speak, representing, for each unit in the diminution of pressure, a diminution of nearly 5·1 units of light. We have upon these tables the calculated results upon this assumption, and you see how nearly they coincide with the observed diminutions in the experiments. Now, as the diminution of light for a comparatively small diminution of pressure is so great when we are operating nearly at atmospheric pressure, it became interesting to ascertain, by as delicate a series of experiments as could be made, the variations within three inches of mercurial pressure, and they were made in the same way as the above, but with a water-pressure gauge, so that the readings were considerably more delicate. The following figures are the result of twenty observations under each pressure:—

Third Series.

Pressure of Air.	Illuminating Power.	
	Observed.	Calculated.
30·2	100	100
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

Here, in the second column, you have the observed numbers, and in the third the calculated numbers, at the rate of a diminution of 5·1 units of light for each inch diminution of pressure. You see the line is almost perfectly straight, and coincides so nearly with the divergence which is represented here by this dotted line that somewhere about half way up the diagram the two lines coalesce and remain together during the rest of their passage to the top corner of the diagram. In both diagrams the dotted line represents the calculated numbers.



These, then, being the results of the combustion of gas and candle flames in rarefied air, some questions naturally arise as to what can be the cause of these phenomena. Before answering these questions, it would be well for us, perhaps, for a moment or two to glance at the conditions under which light is evolved from flames in general. In gas and candle flames the light is due to two sources; namely, first, the incandescence of gaseous matter, and secondly, the incandescence or glow of solid matter deposited within the flame. Now the light due to the incandescence of gaseous matter is so slight—does not certainly amount to more than 1 per cent. of an ordinary gas or candle flame—that it may be left out of consideration altogether, and it only comes into prominent play when we get to a very high degree of rarefaction, but it is a light which suffers little diminution, and hence it causes the experimental lines in the diagrams to diverge at low pressures from the calculated line. The incandescent gaseous matter retains the total luminiferous effect higher than it otherwise would be if this light from gaseous incandescence were subject to diminution by the rarefaction of the air. Here is one of the incandescent gas flames—namely, hydrogen. We have two of them in coal gas—namely, hydrogen and carbonic oxide. You see the light from that large amount of flame is scarcely worthy of consideration at all; but if we introduce into the flame some solid matter which is capable of being heated to incandescence, we shall at once obtain a much more powerful light—we shall, in fact, feed this burner with exactly the same hydrogen gas from the same source as that from which the other burner is fed, and the only difference in the two experiments will be, that into this hydrogen flame we have introduced a quantity of platinum wire coated with a little film of lime, which is obtained by burning upon the wire paper that has been immersed in chloride of calcium. [The lights were then turned down.] Here we have now the same flame with this solid matter immersed in it, and you see the difference in the amount of light emitted at the same temperature, for the flame has not increased in temperature—it has perhaps rather diminished in temperature by the presence of this platinum gauze. [The lights were then turned up.] Thus the presence of solid matter is one condition for the production of a considerable amount of light in flame. But it may be objected that if we require solid particles of matter in a flame—if we assume that in ordinary flames we have solid matters present—those solid matters ought to be capable of being perceived, and that flame, in fact, ought to be to a certain extent, opaque, owing to the presence of those solid matters floating within it. Such is, however, not the case. Flame is almost perfectly transparent; if devoid of smoke, it is practically quite transparent. I will endeavour to show you this by ascertaining whether we cannot, through a considerable body of flame, actually read a newspaper. I will just illuminate a printed paper by means of the electric lamp, and project an image of it upon the screen. There we have, I think, a tolerably readable page. I think no one would have very great difficulty in reading that. Now, I will interpose this gas-flame between the paper and the screen. You see there is an image of the flame upon the screen, but it does not in the slightest degree interfere with the reading of the paper. The transparency is so nearly perfect as not to affect the legibility of these printed letters. So that this mass of flame which appears to have substance in it, is to all intents and purposes, transparent. You may make the experiment yourselves if you hold a piece of printed paper very near a gas-flame, and look very closely at it; you can read through the thickest portion of the flame without difficulty. But we may prove in another way, and by means of the electric lamp, that solid particles may be and really are carried across a perfectly transparent space, and if I project the image of these carbon points from which we obtain our light, on the screen we shall see between them a perfectly transparent portion, apparently quite free from solid matter, but through which particles of solid matter in great quantities are constantly passing. Dr. Tyndall will be kind enough to throw these points upon the screen, and then we shall have the luminous image of the prisms of carbon which are brought nearly into contact, and between which the electric current is passing, bridging over the space; there we have them shown on the screen. I want you to notice the lower of these points; you see how it is growing—how there is a sort of branching out of that point towards the upper one, which is caused by the transference of the particles of carbon from one point to the other. The space between the points is perfectly transparent just as our gas-flame was, although these particles of carbon in the solid form are being carried over. See how they are growing upwards from the lower point, and how the upper one is wasting. That growth will take place through a considerable space of air, which, however, remains transparent all the time. The light of flame is also greatly dependent upon the temperature of the flame. With the same amount of solid matter floating in the flame the light augments by an increase of temperature, and *vice versâ*. [The lights were then turned up.]

These being, then, the general conditions upon which the light of flames depends, the first question that will naturally arise to our minds will be this,—Is the combustion which takes place in rarefied air a perfect combustion; because if it be an imperfect combustion, for a given amount of combustible substance consumed, the quantity of light would be considerably less than that which would be otherwise obtained? I have therefore investigated the point with considerable care, and having extracted the gases that were passing off from the flame, both when the candle was burning under ordinary atmospheric pressure, and when under a pressure of eight inches of mercury, I found that in both cases the combustion was perfect. There was not a trace of carbon escaped unconsumed; therefore the combustion, both in atmospheric and rarefied air, was perfect. In fact, if I may be allowed the expression, the combustion in rarefied air was more perfect; for there was a more rapid conversion of the elements of combustible matter into their final oxidized products—carbonic acid and water. This question, therefore, as to whether the diminution of light is caused by an imperfect combustion, must be answered in the negative. The next question that will arise is this,—Is the diminution of light caused by a diminution of temperature? It is well known that if you allow air to escape out of a vessel into another vessel containing rarefied air, or into another vacuous vessel, there is a considerable diminution of pressure in the one from which the air escapes, and it might not be unfairly concluded that the temperature does suffer in this way. But I believe this diminution of temperature, from the cause just mentioned, is practically very small, and is neutralized to a great extent by the smaller cooling effect of the atmosphere; the atmosphere being more rare, the quantity of heat taken away from the flame, and the amount of heat carried away by convection, is smaller than in ordinary circumstances. That this is so, may be rendered evident by a very simple experiment. We will ignite a platinum wire in such a way under the receiver that the incandescence shall be scarcely apparent. [The platinum wire was then rendered slightly incandescent by a current of electricity from six of the battery-cells.] You will notice now the extent to which that wire glows while we extract the air, and I think it will be noticed that as the rarefaction goes on, the intensity of that glow will increase. If you notice the illuminating effect within the jar, you will find it very considerably increased by this rarefaction of the air. Here we have in this electric current a constant source of heat, but the cooling effect of the air is diminished by the rarefaction, and consequently the wire becomes hotter, owing to the less amount of heat being extracted from it. This is the case with the flame of the candle, and this effect makes up nearly for the somewhat lower temperature that is produced by the combustion of the flame, so that this question must also be answered in the negative.

Now we come to a third question, namely, Is the diminution of light due to admission of atmospheric oxygen into the interior of the flame? If atmospheric oxygen could gain access to the interior of the flame then it would undoubtedly have the effect of destroying these incandescent particles of carbon which are now in the interior of the flame, and consequently of withdrawing the very source upon which $\frac{99}{100}$ths of the light depends. It is well known that air introduced into a gas flame has the effect of gradually diminishing the light. We have here an ordinary gas-flame giving the usual amount of light, and an arrangement by which we can show the effect of atmospheric oxygen upon the flame. Our supply of gas is regulated by this stop-cock. The gas passes along this tube and into the burner. Here we have a second tube inserted into the gas-tube, through which we can throw atmospheric air contained in this air-holder. Notice the remarkable effect upon the flame. We have got now the maximum illuminating power. Now I have admitted the smallest possible quantity of air. It has not yet reached the flame, but it will presently. You see at once, when the air comes into the flame, how gradually the size of the flame is reduced. If we had the means of measuring the illuminating power, we should find that power reduced also. We will now admit a further quantity of air, and you see, as soon as it reaches the flame, how the light is reduced. There is no doubt that this is the principal, if not the only cause of the diminution of light in these gas and candle flames. But how does the oxygen gain access to the interior of the flame in greater quantity in rarefied air than it does in condensed air? The cause is very easily explained. If you have a very rare or light gas, the particles of that gas are capable of moving about in all directions with very much greater facility than the particles of a comparatively dense gas. Hydrogen is very remarkable for what is termed its "mobility"—its power of intermingling itself with other gases. A jar of hydrogen left for a few moments with its mouth open downwards, exposed to the air, will lose all the hydrogen contained in it. The gas will rapidly pass out to the external air, even though the mouth of the jar be downwards, and consequently the gas has to go out against its own comparative lightness, and to make its way against gravity downwards. So when you rarefy the air contained in this vessel, the particles of the air are far more mobile than when the air is at the ordinary pressure, and they gain access into the interior of the flame with far greater facility. It has been found that when a candle or gas flame burns in air at the ordinary pressure, that as much as from 60 to 70 per cent. of nitrogen gains admission into the interior of the flame. Now, if we withdraw the gases from the interior of the flame as far as the flame remains blue, you find oxygen present. It is only when you get higher into the luminiferous portion of the flame that there is nitrogen and no oxygen. The presence of oxygen in the flame determines at once the combustion of the carbon particles, which constitute the source from which the light emanates under ordinary circumstances.

I feel it necessary to apologise for having brought these simple experiments before you. I have done so in reliance upon that interest which I know those who weekly assemble here feel in any new scientific fact, however abstract and technically valueless it may appear to be. I do not know that any useful application can be made of this law regarding the reduction of illuminating power; but possibly, if we find that this effect extends upwards to higher pressures, some such application might be made.

I have only yet been able to make a few experiments in this direction, but so far as they go, they seem to indicate that the same law holds good for higher pressures, and that at two atmospheres we should have about 2½ times the illuminating power which we have at the ordinary atmospheric pressure, thus greatly increasing the amount of light obtainable from a given quantity of combustible matter. These phenomena of flame did not escape Davy in his laborious and beautiful researches upon flame, although he does not appear to have paid more than a passing attention to the effects of pressure upon the light of combustion.

Weekly Evening Meeting, Friday, March 15, 1861.

A Lecture on Electrical Quantity and Intensity,
by Latimer Clark, *Esq.*

(*Continued from page 355.*)

The instances in which the quantity present is not simply dependent on the tension, are those in which other electrified bodies are present which, by their inductive influence, affect the quantity present in all bodies in their vicinity without necessarily affecting their tension. An insulated cylinder was connected with the positive pole of a Daniell's battery of 600 cells, its negative pole being connected with the earth ; so that the cylinder was in a condition to give off a powerful and visible current to another wire connected with the ground ; in this condition a positively electrified disc was approached to it and by its inductive influence was shown to render one end of the cylinder electrically negative so that a carrier ball applied to that end showed it to have a negative charge, thus presenting the apparent paradox of a negative electrified body giving off a positive current to the earth, or *vice versâ*. One end was negatively electrified, and the other end positively, but the tension was the same everywhere.

According to the ordinary way of regarding this class of phenomena, it was usual to state that the ends of the cylinder acquired a state of positive or negative intensity, or that they had their intensity changed : it was contended that this gave an inaccurate idea of the real nature of the change, and that the approach of an electrified body, however near or however violently it might be excited, could not in the slightest degree affect the tension of a conducting body, which was in connection with the earth; the only influence it could have would be to alter the quantity in the second body, by driving a portion of its electricity downwards to the earth. It might be assumed as a law that the tension of the electricity in every part of a conducting body of moderate dimensions was the same, notwithstanding the vicinity of other electrified bodies. If a positively electrified body were brought near an insulated conductor, the distribution of the electricity in the second body was changed, and its whole tension was raised, but the tension remained everywhere uniform, and was as high at the negative as at the positive end.

The fall of tension in electricity was always accompanied by its conversion into heat ; the ignition of wire by the voltaic current, the intense heat of the voltaic arc, and the heat and light of the electric discharge and of the spark, were all cases of the evolution of heat consequent on the fall of tension, and the quantity of heat evolved was apparently directly proportionate to the fall of tension within a given space and to the quantity of electricity passing.

In the case of electric telegraph conductors and submarine cables, it was shown, from a carefully-conducted and extensive series of experiments, that the tension falls with the most perfect regularity from the positive pole of the battery to the end in connection with the earth, in accordance with the law of Ohm; and since the quantity of electricity held under induction varies in the same ratio as the tension the distribution of the charge in a cable follows precisely the same law. It results from this, that if a cable with a current flowing through it, be divided into any number of equal sections, and the quantity in the section connected with the earth be taken as unity, the quantities in all the other parts, whatever their number, will be in the ratio 1, 3, 5, 7, 9, 11, &c. So that if a cable be divided into two halves, the quantities will be in the ratio of one to three.

The Speaker stated that he had ascertained that, in the voltaic battery, the presence of two metals was not an essential condition—the negative metal was not necessary for the formation of the electric current, but only for its after detection and exhibition. A simple mass of copper, iron, zinc, or any oxidisable metal, when laid on the moist earth, formed a complete battery in itself, giving positive electricity to the earth, and quickly assuming a negative tension, which it would communicate to any other body resting on it or in contact with it, as for instance, a length of submarine cable. If a cable thus charged were afterwards removed and applied to any more electro-negative metal, such as platinum, or to carbon, the charge would return to the earth ; and it was found by measurement that the charge thus acquired by a cable was exactly the same as if the two metals had been employed simultaneously in the ordinary form of a galvanic couple. If the mass of zinc were permanently connected with the non-oxidisable or less oxidisable metal, and thus with the earth the tension, being constantly destroyed and as constantly renewed, would form a constant current, becoming in fact a voltaic couple. If the connection with the earth, instead of being made through another metal, were made through any inert conducting substance or liquid, the same constant current would be produced, thus forming the well-known case of a voltaic battery with one metal and two liquids.

After the conclusion of the discourse, Mr. Clark stated, with reference to Faraday's discovery of specific inductive capacity, that in the course of some investigations conducted in conjunction with Professor Hughes, they had observed that every different dielectric possessed its own specific law of variation of inductive capacity with respect to distance. With air it varied as the distance, inversely; but with gutta-percha it was more nearly as the square root; with India-rubber and white wax it was intermediate between the two ratios, and the law of variation was different with every substance tried. From which it would result that observers who deduced the specific inductive capacity of any material from experiments on half-inch plates, would arrive at very different results from others who operated on inch plates.

CHEMICAL SOCIETY.

Thursday, May 2, 1861.

R. Warrington, *Esq., in the Chair.*

T. Griffiths, Esq., was elected a Fellow.

Mr. John Davidson read a paper "*On the Action of Dibromide of Ethylene on Pyridine.*" A mixture of the two substances with about one-sixth of its volume of alcohol was heated in sealed tubes to 100° C., whereby a silky crystalline mass was obtained. Representing pyridine by the formula $(\text{Ꞓ}_5\text{H}_5)'''\text{N}$, this new product would be the dibromide of ethylen-dipyridyl-diammonium, $(\text{Ꞓ}_2\text{H}_4)''(\text{Ꞓ}_5\text{H}_5)_2'''\text{N}_2\text{Br}_2$ corresponding to one of the principal products of the action of dibromide of ethylene on triethylamine, namely, the dibromide of ethylene, hexethyl, diamonium $(\text{Ꞓ}_2\text{H}_4)''(\text{Ꞓ}_2\text{H}_5)'_6\text{N}_2\text{Br}_2$. By acting on the new dibromide with chloride of silver, the corresponding chloride was obtained, which formed with dichloride of platinum, the double salt $(\text{Ꞓ}_2\text{H}_4)''(\text{Ꞓ}_5\text{H}_5)_2'''\text{N}_2\text{Cl}_2\,2\text{PtCl}_2$. By treating the dibromide with freshly precipitated oxide of silver, the corresponding dihydrate was obtained as a colourless powerfully alkaline but unstable liquid.

Professor **Bloxam** read a paper "*On the Amounts of Water Expelled from the Hydriates of Potash, Soda, and Baryta, by Boracic and Silicic Acids.*" The Author referred to his former experiments, which showed that one atom of boracic acid BO_3 at a bright red heat expelled one atom of carbonic acid CO_2 from carbonate of potash, two atoms from carbonate of soda, two and a-half atoms from the carbonates of lithia and baryta, and three atoms from carbonate of strontia. Hence, he inferred that boracic acid was tribasic, but that in some cases it was unable to satisfy its saturating capacity, on account of the affinity of some of the bases for CO_2. The affinity of these bases for HO, being less than those for CO_2, he thought that by employing hydrates instead of carbonates he might be able to obtain tribasic salts with them also. The Author described minutely the precautions he had taken to insure accurate results, and to overcome the difficulty arising in the case of hydrate of potash from an absorption of oxygen. The boracic acid and hydrates of potash and soda were heated in a silver tray contained in a glass tube through which a current of pure air was passed. The boracic acid and hydrate of baryta were fused together in a silver crucible. It was found that one atom of boracic acid expelled two atoms of water HO from hydrate of potash, and three atoms from the hydrates of soda and baryta. The Author argued that inasmuch as the water of hydrate of baryta was more easily expelled than that of hydrate of soda, and inasmuch as boracic acid drove out no more water from the former than from the latter, it followed that its maximum capacity of saturation was satisfied by the three atoms of base with which it united in each instance. He observed that the formulæ of boracic ether $3EtO.BO_3$, and octahedral borax $NaO.2HO.BO_3 + 3HO.BO_3$ were distinctly tribasic. Similar experiments were made with silicic acid, from which it appeared that one atom of SiO_2 drove out 1½ atoms HO from hydrate of potash, and 2 atoms from the hydrates of soda and baryta respectively. Hence, the Author inferred that the capacity for saturation of silicic acid was satisfied by 2 atoms of base, and gave a list of well defined bibasic silicates in illustration. He also referred to the analogies of boracic and silicic acid, and observed that ⅓ atom of boracic acid might be considered the representative of ½ atom of silicic, whereby the weights of comparable quantities of boron and silicon would be to one another as 3·6 to 7, or as 1 to 2.

Col. **Yorke** was glad to find that Mr. Bloxam's experiments supported the conclusion at which he had formerly arrived, and which had also been advocated by Dr. Odling, as to the bioxygen formula for silica. Despite Schrœder's late experiments, he could find no reason to look upon silica as a teroxide.

Dr. **Odling** said that according to his experiments 1 atom of boracic acid, BO_3, drove out 3 atoms of CO_2 from carbonate of lithia, and considered that Mr. Bloxam had under-estimated the amount expelled, by a miscalculation of his results. He thought that boron and silicon did not belong to the same group of elements, but rather that they occupied somewhat comparable positions in different groups, and that as sulphur was related to tellurium, phosphorus to bismuth, and silicon to tin, so was boron related to aluminum. Representing 27 parts of aluminum by the symbol All, we had the following comparable bodies:—

H_3BO_6 Boracic acid cryst.	H_3AllO_6 Alumina gelatinous.
HBO_4 Do. dried at 100° C.	$H\,All\,O_4$ Diaspore.
$Mg\,BO_4$ Boracite.	$Mg\,All\,O_4$ Spinelle.

Boracite was isomorphous with spinelle, and BO_3 seemed to replace $All\,O_3$ in several siliceous minerals. He considered that the two principal classes of borates were the monobasic, analogous to the metaphosphates, and the tribasic, analogous to the orthophosphates, but that intermediate salts also existed. He preferred to double the atomic weight of silica, and consequently to represent the most highly metallised silicates as tetrabasic.

Professor **Crace Calvert** read a paper "*On the Graphite of Cast Iron.*" He showed that, when cast iron was slowly acted upon by dilute acids, and preferably by acetic acid, a black plumbago-like mass was left, so soft as to be cut with a knife, having a sp. gr. 2·5, and a constant composition. It contained about 80 per cent. iron, 11 per cent. carbon, 2½ per cent. nitrogen, and 6 per cent. silicon. He adverted to the presence of nitrogen in cast iron, and thence inferred that mere nitrogen could not be the differentiating agent between iron and steel.

Mr. **De la Rue** and Mr. **Warington** referred to the similar change of iron into a soft black substance which took place in an iron grating habitually exposed to the action of sour beer-dregs in a London Brewery. The grating became so soft that it eventually gave way by the mere weight of an empty cask.

The Author of the paper and Messrs. Riley and Malone then took part in a short discussion on the relation of nitrogen to acierification.

CORRESPONDENCE.

The Sale of Poisons.

To the Editor of the Chemical News.

Sir,—The discussion on the prevention of poisoning, caused by accident or otherwise, seems at length to have ended without any practical benefit having been derived from it.

It has been said of railway accidents, that no adequate means will be taken to prevent their recurrence until a Bishop has been sacrificed; the same may be perhaps, but too truly, said of poisoning, that until a Right Hon. Lord or a Right Rev. Prelate has, by ignorance or negligence, accident or design, come to an untimely end, we may look in vain for legislative aid or public interference.

Our own efforts with regard to this important subject are spasmodic in the extreme, chiefly confined to picking to pieces each successive measure as it comes before our Legislature, showing in the most glaring manner its impractibilities and unreasonable clauses, bringing all our opposition to bear upon it, and ultimately crushing it; and then quietly return to our usual avocations, and gloat over our "success."

Is it not, then, a wonder that with such a perfect knowledge of what we do not want, and what will not be for the benefit of her Majesty's loving subjects; and with an almost unanimous knowledge of what we do want, and what will be for the public benefit, viz., the causing of all future chemists to pass an examination testing their ability and competency, and giving them the exclusive right of vending Poisons, that we do not more strenuously endeavour to obtain it?

These are the only legal restrictions necessary to prevent the recurrence of three-fourths, at least, of the accidents that have occurred, and it would be a guarantee to the public that entering a chemist's shop, that "the right man" would be found in "the right place." Of course I mean in the same way that we are certain that every F.R.C.P. or M.R.C.S. is perfectly competent,—the former to give advice regarding bodily ailments, the latter acquainted with the anatomy of our frame, and able to repair a broken limb or set right a dislocated joint.

To the objection that this would be a work of time, and that it would be no guarantee that those who are at present in business are competent to fulfil the duties required of them, I can only reply that all similar changes have been open to the same objection, and that change for the better is always a work of time.

The sale of poisons may be safely left in the hands of the educated chemist, no legal restriction is necessary with regard to the vending of this or that, for every chemist knows but too well that a summons before a coroner's inquest and a verdict censuring his conduct, is a certain forerunner to a presentation in all due form to the Commissioner of the nearest Court of Bankruptcy, and that he may as well put the shutters up at once. Enter any respectable chemist's shop, ask for two ounces of laudanum, and the intelligent proprietor or his assistant will, in all probability, ask you what it is required for, enquire regarding your knowledge of its properties, dose, &c., and unless the answers are perfectly satisfactory, he will, with the greatest politeness, decline to serve you, and the "word in season" has often, to my knowledge, proved of benefit. In a neighbouring town, a poor woman, a few months since, asked for two-pennyworth of oil of vitriol, and brought a cup to take it in: she was asked what purpose she required it for, and the answer was "her old man was very bad, he had a cough upon him for a long time, and she was told a teaspoonful in water was a very good thing for him,"—of course, she wanted elixir vitriol, which was given, and the proper dose told her; here, in all probability, a life was saved, yet the Humane Society presented no medal; I do not think he was even thanked; and he finds, as nearly all junior members of our fraternity find, quite enough to do to keep the wolf from the door. Innumerable instances of a similar nature may be given, but they are every-day occurrences, as every member of the profession knows.

But the man or woman who contemplates suicide by means of poison, does not procure it from a place questions are likely to be asked with regard to the purpose it is required for; they know full well that laudanum or oxalic acid, and kindred substances, may be obtained from almost any huckster's (whose knowledge does not even extend to the external difference of oxalic acid and Epsom salts, or the varied appearance of tincture of opium and tincture of rhubarb, and whose acquaintance with their properties is even less than that of their appearance), and that he will not be inquisitive, or even trouble himself with labelling the desired article. How often our efforts to prevent suicide are frustrated, the newspapers occasionally tell us; but in the great majority of cases there is an impossibility, from the absence of labels, to find out the vendor. Of course the nearest chemist gets the benefit of the blame: and I do think that, having to bear the blame, he has a right to claim the exclusive sale of the poison.

It would be, I believe, a fallacy to suppose that in a fallible world we can ever attain infallibility, and that as long as there are poisons people will be poisoned, in the same manner that as long as there are razors they will be used for other purposes beside the legitimate one of shaving; but we may reduce them to the minimum, and all suggestions aiming, or tending to attain that desired object, will ever be received and adopted by the majority of chemists.

The bottle invented by Messrs. Savory and Moore, or some practical modification, will prove, when better known, of great service. Labels are a great preventive of error, but labels do not fulfil every requirement, as all chemists know; two bottles were sent out, the one containing hartshorn and oil, the other a mixture, both properly labelled, but of the same size and shape, the hartshorn and oil was given instead of the mixture, the latter being of the same colour and composed of confection of almond, much the same in appearance as the former. In the South of England, a gentleman living in the country, and in the habit of keeping a few simple remedies by him, ordered his servant to take out an aperient draught from the medicine chest; but here again the laudanum was kept in a two-ounce vial, and the black draughts in bottles of the same size and form, and being very similar in appearance, laudanum was, through the carelessness of the man, given instead of the draught, although the bottles were properly labelled, and the laudanum marked "Poison." The accident being immediately discovered, and proper remedies applied, both cases fortunately recovered.

I could give instances *ad infinitum*, but will conclude with one more for the benefit of those who consider the label a certain preventive of error.

In the suburbs of a neighbouring and one of the largest manufacturing towns there lives a retired minister of one of the numerous Dissenting denominations, who had an only son, who promised to be the solace and support of his aged parents, holding a responsible position in a large metropolitan warehouse. But over-work, bodily and mental combined, with a naturally weak constitution, so much enfeebled him that during the winter of 1859 he was obliged to go home, hoping the rest and purer air of his birthplace would be the means of re-establishing his health. But the work was done; that arch fiend Consumption had struck vitality to its very core. Medical aid was of no avail, except by the help of sedatives to palliate the distressing sufferings. The old man had taken upon himself the duty of administering the medicine at the proper time, and kept the bottles containing the physic on a shelf in an occupied room. Unthinkingly a bottle of bug poison was placed among them by the servant, unknown to the old man, and, like all men arrived at his age, his perceptive faculties were blunted, and so in the dark he poured out a wineglassful of the bug poison, and gave it to his son. The mistake was soon discovered from the odour, naphtha being mixed with the solution of bichloride of mercury, so as to render the taking by accident almost impossible. But the fatal work was done. A constitution weakened by disease was unable to bear the shock, and so he died. A pitiable sight it was to see and hear that old man tell his sad, unvarnished tale before the coroner and his jury, his white hair bleached by the sun of seventy summers, and big tears rolling down his furrowed cheeks; his useless but bitter regrets that any act of his should have hastened, if but for an hour, his son "where all have gone, and all must go." A verdict in accordance with the evidence was given. The bottle was properly labelled "Poison." But I do not think this accident would have occurred if the contents had been placed in a bottle similar in principle to Savory and Moore's.

I am well aware that what I have written has been frequently said before much more ably and more forcibly; but, with the immortal Locke, I think "the pictures drawn in our minds are laid in fading colours, and, if not sometimes refreshed, vanish and disappear." This is strikingly illustrated with regard to the important subject of poisoning. Hoping you will accept this as my apology for troubling you,—I am, &c.

Wm. Bennington.

30, Oxford-street, Liverpool.

Magenta Dye.

To the Editor of the Chemical News.

Sir,—Having, till now, overlooked "Magenta's" reply of the 18th May, and as that communication has not altered the opinions I expressed in my last, perhaps "Magenta" would have no objection to make his discovery advantageous to himself, by forwarding me a sample, with quotations, for a quantity, or I should have pleasure in affording "Magenta" every facility for trying it on Coburgs and Orleans on a large scale, and, if found a *bonâ fide* and practicable article, I need not say the advantage will be great, and no one would have greater pleasure to bear testimony to the fact than—Yours, &c.

James Sharp.

Botterill's Dyeworks, Leeds.

Chemical Notices from Foreign Sources.

I. TECHNICAL CHEMISTRY.

Aich's Metal.—An alloy of iron, copper, and zinc, possessing extraordinary strength is described under this name (*Mittheilungen d. Gerwerbver f. d. Konigr. Hanover:* 1861, s. 59), the inventor being an Austrian marine officer. The specific gravity of the compound is said to be 8·40, but the proportions of the metals are not given. The colour of the metal is darker than brass. It can be worked more easily than iron, and almost as easily as copper. Its ductility is extraordinary, but its most valuable quality is its strength. Tested at the Vienna Arsenal, it was found, when cast, to be nearly as strong as Armstrong gun iron, but by hammering and rolling, its strength was almost doubled. Made into a tube and tested with gunpowder, it was found to have twice the strength of wrought iron, and five times that of bronze gun-metal.

Sterro-metal is another alloy somewhat resembling the foregoing, which it is proposed to use for ships-bolts (it not being acted on by sea-water) engraver's plates, bells, vacuum pans, &c. The composition of this alloy is not given. Both this and the foregoing may deserve the attention of English metallurgists.

Manufacture of Ultramarine.—C. Furstenau gives (*Dingler's Polytech. Journal*, Bd. clix. s. 63) the following as a good method of making a dark ultramarine. He takes of

Lightly-burnt kaolin . .	100 parts
Calcined soda	90 "
Refined roll sulphur . . .	100 "
Rosin	6 "
Dry pinewood charcoal . .	4 "

All these with the exception of the rosin are ground very fine. The rosin is then added in pieces the size of nuts, and the whole is ground together for four hours. The resulting grey powder is then placed in a clay oven and heated quickly to the melting point of an alloy of equal parts of gold and silver. After having been exposed to this heat for five or six hours, the oven is allowed to cool. After a couple of days the bluish-green mass is removed and ground fine in a mill. It is then roasted again in a cast-iron retort, where it assumes the blue colour. It is then finely levigated and dried for sale. The amount of product obtained from the proportions given is 122 of ultramarine.

II. ORGANIC CHEMISTRY.

Myronate of Potash.—Prof. Ludwig and W. Lange extracted this salt from black mustard, by the following modification of Bussy's process. (*Archiv. d. Pharm.* ii. Reihe. Bd. clv. s. 20.) They first extracted the fresh powdered mustard twice with hot alcohol, then treated the powder several times with cold water, evaporated the watery solution on a water bath, extracted the residue with hot dilute spirit of wine, and concentrated the filtered extract. From this the myronate of potash crystallizes out on standing. 500 grammes of mustard yielded one gramme of the salt, the analysis of which gave the formula $KO\ C_{20}\ H_{19}\ NS_4\ O_{18}$, and appeared to be constituted as follows:—$KO\ S_2\ O_4$ acid sulphite of potash + $C_6\ H_5$ + $Cy\ S_2$; mustard oil, + $C_{12}\ H_{14}\ O_{14}$ sugar; or $KO\ S_4\ O_4\ H_2\ O_2$ + $C_6\ H_5\ Cy$ cyanide of allyle + $C_{12}\ H_{12}\ O_{12}$ sugar.

Myronate of potash boiled with dilute sulphuric acid, the acid being afterwards separated by lime, did not ferment on the addition of yeast. Trommer's test showed, however, the presence of sugar in the solution. When boiled with potash the solution of the myronate turns yellow, evolves ammonia, and, on the addition of hydrochloric acid, sulphuretted hydrogen. Rendered alkaline again, the solution reduces oxide of copper. Caustic ammonia decomposes myronate of potash, forming a sulphate. Cold baryta water does not act on the myronate, but on boiling the mixture it becomes cloudy, and when hydrochloric acid is added, evolves sulphuretted hydrogen and carbonic acid, sulphate of baryta remaining undissolved. Some of the other reactions of myronate of potash are interesting. For instance, nitrate of silver at first produces no change, but after some minutes the mixture becomes cloudy, and in time a white precipitate falls. On boiling this precipitate dissolves, and sulphide of silver soon separates, a strong smell of the volatile oil of mustard being evolved showing the development of this body from myronate of potash, without the intervention of myrosine. Myronate of potash is easily soluble in water; the clear solution mixed with a colourless solution of myrosine soon becomes cloudy from the separation of sulphur, the smell of the volatile mustard oil being developed.

Composition of Pus.—Giesecke gives (*Ann. d. Chem. und Pharm.*, Bd. cxvii. s. 110) a very complete analysis of what may be called healthy pus. It was thick, pale yellow, faintly alkaline, had the sp. gr. 1022, and no smell. The composition was as follows:—

Water . .	88·76		
Solid matters	11·24	organic matter	10·12
		inorganic . .	1·12
	100·00		

The solid matter was composed of

4·38	albumen dissolved in pus serum
4·65	pus globules, mucus, with a little leucin and glutin
1·09	cholesterin, with some other neutral fat
0·59	chloride of sodium
0·32	soda (from albuminate of soda), a little phosphate of soda, and a very little sulphate of potash
0·21	phosphate of magnesia, lime, and oxide of iron.
11·24	

MISCELLANEOUS.

Chemical Society.—The next meeting of this Society will take place on Thursday next, June 20, when a discourse will be delivered "On the Application of the Induction Coil to Steinheil's Apparatus for Spectrum Analysis," by H. E. Roscoe, Ph.D.

ANSWERS TO CORRESPONDENTS.

Q. S. T.—A notice was given in our first volume of a patent for obtaining a very pure salt of potash in quantity from Suint.

J. S.—They will be concluded if possible in the present volume.

P. T. de Tomas.—Consult an elementary work on decimals. 21·4 is a greater number than 21·15. The degree of temperature at which platinum melts is not known: it has been estimated at different times at from 3000° Fahr. to any temperature you like.

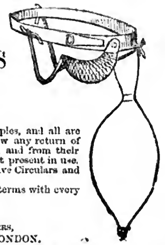
S.—In a week or two.

D. Thomas.—We can suggest no plan better than advertising your instruments for sale in our columns.

J. S. Pearson.—They all ultimately find their way to the sea. It will be some years, however, before all the fishes are poisoned, they will last our time.

Ammonia.—1. Liq. Vol. C. C. is generally made by diluting liq. ammoniæ with distilled water. 2. We have had no experience in the preparation of spt. ammon. arom. with nutmeg, so cannot say whether it would colour as much as cloves.

The Solar Eclipse.—The following letter from a Correspondent may amuse if it does not instruct our readers:—"I hasten to offer a theory suggested by Professor Faraday's beautiful discourse on the Eclipse of July, 1860. (Chemical News, June 1.) The prominence 28,000 miles in length and 74,000 miles above the sun was a planet which struck the embossed surface of the sun and rebounded to that height, where it is now dissolving for the supply of the chromageneous ether, which is the sun's great atmosphere, enabling him to speed on in his meteor course, as our own atmosphere enables sound to move."—"Kofos."



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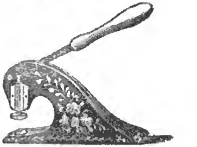

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DR. HASSALL'S REPORT

ON THE

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BY

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"The result of the examination of the samples is most satisfactory. It is not long since it was affirmed that pure Mustard could not be made, and that, if made, the public would not use it. The case is now altered, and the time is not very distant, when the pure article will supersede the compounds now so generally sold as Mustard, but which often possess scarcely any of the properties, or even the taste of Mustard.

"ARTHUR HILL HASSALL, M.D. Lond.,"
Analyst of the Lancet Sanitary Commission. Author of "Food and its Adulteration," "Adulterations Detected," and other Works.
London, October 31, 1860.

OPINION OF
SIR CHARLES HASTINGS, M.D., D.C.L.

"The pure Mustards now produced by Messrs. Evans, Lewis, and Co., of this city, have been submitted for my opinion of their genuine quality, together with the report of a careful Analysis to which they have been subjected by Dr. Hassall; and I feel confidence in expressing a decided opinion of the correctness of that Analysis. I can, therefore, congratulate the Profession and the general public, by whom it is used, on having within their reach so important an article for medicinal and culinary purposes as Mustards, entirely free from the irritating qualities of capsicums, or any other objectionable stimulant.

"CHARLES HASTINGS."
Member of General Council of Medical Education; Senior Physician to the Worcester General Infirmary; President of the Council of the British Medical Association.
"Worcester, January 1, 1861.

"Westbourne Park, Bayswater, London, W.
April 12, 1861.

"The importance of really genuine, unadulterated Mustard, for culinary purposes, or as a safe and simple vesicant, can hardly be over estimated; but until very recently, pure Mustard has been nearly unknown in London. In my recent paper "On Food and its Adulterations, and the Methods of Detecting them" (Vide JOURNAL OF THE SOCIETY OF ARTS, Feb. 1, and CHEMICAL NEWS, Feb. 9, 1861), I stated that the article in question was never sold pure. This observation, however, must now be slightly modified, as within the last few mouths Messrs. Evans, Lewis, and Co. have introduced to Londoners four qualities of good and genuine Mustard, of which A and B are perfectly pure, while C and D are merely coloured with a minute proportion of turmeric.

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Liquor Magnesiæ Permanganatis Pur:
(Condy) gr. ij. ad ℥i

The above solutions of pure permanganatis are prepared for internal or external use Antiseptic and Tonic.

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